Consequences of Medicaid Expansions on Three Outcomes: Demand for Private Insurance, Infant and Child Health, and Labor Supply

by

ESEL YILDIZ YAZICI

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UMI
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6/19/97 Michael Grossman

Date

Chair of Examining Committee

6/23/97 Linda L. Edwards

Date

Executive Officer

Professor Michael Grossman

Professor Theodore Joyce

Professor Robert Kaestner

Supervisory Committee

THE CITY UNIVERSITY OF NEW YORK

Abstract

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Adviser: Professor Michael Grossman

In the mid-1980s, Congress expanded Medicaid coverage to near-poor pregnant women and children. The aim was to provide health insurance coverage to the uninsured and increase their utilization of health care services. Beginning with the Deficit Reduction Act of 1984, the link between Medicaid eligibility and other cash assistance programs was severed. Low-income pregnant women and children, who initially did not fit into traditional welfare categories, gained access to publicly financed health care. These expansions in eligibility produced a sharp rise in the number of children and pregnant women covered by Medicaid.

The expansions in Medicaid eligibility had intended and unintended consequences on several outcomes. First of these was on private health insurance. There was a possibility that near-poor individuals who initially had private coverage switched to Medicaid. Since this indicates only a shift in financing of care from private to public insurance, the consequence would be little or no change in health care utilization and health. In the first essay, I found that Medicaid expansions had no effect on privately covered individuals, but a large impact on those who were initially uninsured.

The second potential consequence of Medicaid expansions was on infant and child health outcomes. Even though expanded eligibility gave rise to a substantial increase in enrollment rates, the effectiveness of the public program was conditional on improving the health of infants and children. In the second essay, the effect of Medicaid on infant and child health has been extensively examined and only limited evidence has been found on Medicaid coverage improving health outcomes.

A third possible effect of the Medicaid expansions was on labor supply and welfare participation of female heads. Elimination of the link between the cash assistance program and Medicaid created new incentives in the labor markets for welfare recipients. The expected consequences were an increase in labor supply and reduction in welfare rolls. I analyzed this issue in the third essay and found very little evidence on the effect of Medicaid on labor supply and welfare participation for female heads.

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Preface

In the 1980s, high rates of infant mortality and low birth weight as well as poor child health conditions became major policy concerns in the U.S. The expansions in Medicaid have been enacted to provide a solution for these social problems and to improve infant and child health. In the mid-1980s and early 1990s, the Medicaid program has been largely expanded to cover poor and near-poor pregnant women and children and to increase their utilization of health care services. Medicaid eligibility increased by over 100 percent between 1987 and 1992 (Currie and Gruber 1996b). By 1992, Medicaid expenditures reached to $112.9 billion with 35 million people participating in the public program (Coughlin et al. 1994).

Limited access to medical care was a major reason for the inferior state of infant and child health in the U.S. In 1988, 15.4 percent of poor children were with no health insurance coverage (Dubay and Kenney 1996a). Thus, the primary motivating factor of the expansions was to increase Medicaid eligibility thresholds to expand public health insurance coverage to uninsured pregnant women and children. By 1992, states were required to cover pregnant women and children with family incomes below 133 percent of federal poverty level. Furthermore, many states extended their benefits up to 185 percent of poverty level for pregnant women and infants. These large expansions in Medicaid induced many near-poor families to participate in the public program. Medicaid enrollment rates increased by over 100 percent from 1988 to 1992 for newly eligible pregnant women and children (Coughlin et al. 1994). There was, however, a possibility that individuals who were initially covered by private insurance were among these new Medicaid participants.

Between 1988 and 1991, the proportion of children with private insurance coverage decreased from 73.5 to 69.7 percent (Newacheck et al. 1995). Since these years correspond to the period of the Medicaid expansions, some of this decline in private insurance coverage may be attributed to the expansions. Crowding out of private health insurance would indicate little or no change in health care utilization, since it represents only a shift in financing of care from private insurance to Medicaid. It is noteworthy that the incidence of crowd out would diminish any effect of the expansions on infant and child health. As the number of privately covered individuals switching to the public program increases, Medicaid rolls would increase with or without any decrease in the number of uninsured individuals. As a result, there may not be an improvement in infant and child health, since these individuals previously had access to medical care, and since Medicaid may not necessarily provide superior health care.

The decline in private health insurance coverage may, however, be related to factors other than the Medicaid expansions. The recession in the late 1980s may be the primary cause of the erosion in private health insurance coverage. Therefore, the substantial increase in Medicaid enrollment rates may be associated with uninsured pregnant women's and children's participation in the public program. As uninsured individuals from low-income families enroll in Medicaid, their utilization of health care services would increase. Increase in utilization of health care services, however, may not result in improved health status. Utilization of health care services is associated with improved health depending on the effectiveness of the care given. Thus, publicly financed health care may or may not lead to significant improvements in health of infants and children from poor and near-poor families, even if previously uninsured individuals enrolled in Medicaid after the expansions.

Medicaid expansions, in addition to health insurance status and infant and child health, may have also affected labor supply and welfare participation decisions of female heads. With the legislative changes in the 1980s, the historical link between Medicaid and the cash assistance program was severed. Even though, the expansions targeted pregnant women and children with no ties to the cash assistance program, women initially on welfare may have also been affected by these changes. Female heads who were initially receiving AFDC benefits had the option to leave welfare, increase their labor supply, and keep publicly subsidized health insurance coverage for their children with the expansions. The consequences of these new incentives may indicate a decline in the AFDC rolls and increase in labor force participation for women initially on welfare.

This study analyzes the effect of Medicaid expansions on these three important issues: the crowding out of private health insurance, infant and child health, and labor supply decisions of female heads in three separate essays. Each essay addresses an important policy question posed by the recent expansions in the Medicaid program and attempts to provide answers to these questions.

Essay I

Medicaid Expansions and The Crowding Out of Private Health Insurance

A. Introduction

In the mid-1980s, Congress expanded Medicaid coverage to near-poor pregnant women and children in an attempt to reduce infant mortality and improve infant and child health by providing publicly subsidized prenatal care, delivery services and child health care. Beginning with the Deficit Reduction Act of 1984, the link between Medicaid eligibility and other cash assistance programs was severed, and low-income pregnant women and children who initially did not fit into traditional welfare categories, gained access to publicly financed health care. From 1988 to 1992, the percentage of children under eighteen enrolled in Medicaid rose from 15.6 percent to 21.6 percent. At the same time, the number of uninsured children declined slightly and a substantial decrease occurred in the number of children covered by private insurance (Newacheck et al. 1995). These figures suggest that part of the gains in Medicaid enrollment may be due to crowding out of private insurance.

Previous studies by Cutler and Gruber (1996), Dubay and Kenney (1996a), and Shore-Sheppard (1996a; 1996b) evaluated the effect of Medicaid expansions on private health insurance coverage and reached dissimilar conclusions. A common drawback of the past research is employment of pooled cross-sectional data that are unable to follow the same individuals over time. Consequently, these studies failed to distinguish between individuals who switched to Medicaid and those who became uninsured, and attributed the total reduction in private insurance coverage to Medicaid. Moreover, Cutler and Gruber (1996) and Shore-Sheppard (1996a) estimated the effect of Medicaid expansions on the total population, even though more than 80 percent of these individuals remained unaffected by the expansions.

The distinguishing aspect of this study is use of longitudinal data that can identify changes in health insurance coverage. Specifically, longitudinal data identify whether privately insured children switched to Medicaid following the expansions, or simply became uninsured due to secular trends in insurance coverage. Moreover, unlike most past research, which focuses on the total population, this paper evaluates the effect of Medicaid expansions on health insurance coverage of poor and near-poor children, the target group of the expansions. Finally, changes in health insurance coverage of poor children, specifically, those eligible for Medicaid prior to the expansions, are used to separate the effect of expanding Medicaid income eligibility for near-poor children from trends in insurance coverage common to poor and near-poor children. Poor children serve as "controls" in that they are likely to be affected in similar ways by common trends that also affect the insurance status of near-poor children, except for the expansions in income eligibility. Thus, comparing experiences of these two groups yields the effect of expanding Medicaid income eligibility on health insurance coverage of near-poor children, net of any unobserved trends.

Results indicate that expanding Medicaid income eligibility had no effect on private insurance coverage of near-poor children. These results contradict findings of some previous studies. Longitudinal data indicate that private insurance coverage for poor and near-poor populations declined by the same proportion, suggesting that there is little crowd out due to the eligibility expansions. In addition, the data show that some children who lost private insurance coverage became uninsured. Therefore, it is potentially misleading to attribute the total reduction in private insurance coverage to Medicaid, as is done in previous studies. Moreover, the results of this study suggest that the Medicaid expansions were successful in increasing the proportion of the population with health insurance coverage by reducing the number of uninsured individuals. The increase in Medicaid enrollment rates was not due to the expansion of Medicaid income eligibility thresholds, but due to other factors that affected both poor and near-poor children such as state efforts to streamline eligibility and reduction in stigma. Finally, the expansions led to greater Medicaid enrollment for eligible children from single parent families, families with a less educated parent, families with a non-working parent, and families residing in central cities.

B. Review of Previous Research

Recent studies have investigated the effects of Medicaid eligibility expansions on privately insured and uninsured women and children using pooled cross-sectional data (Cutler and Gruber 1996; Dubay and Kenney 1996a; Shore-Sheppard 1996a; 1996b). Cutler and Gruber (1996) examined the effect of Medicaid eligibility on health insurance coverage of all women of child-bearing age and children. They used the March Current Population Survey (CPS) between the years 1988 and 1993. The authors estimated linear probability models where the dependent variable is individual health insurance status (Medicaid, private insurance, or uninsured), and they controlled for secular trends in coverage by including a full set of year and state dummy variables in all regressions. Their estimation results showed that Medicaid expansions reduced private insurance coverage for children and women with crowd-out rates of 31 percent and over 100 percent, respectively. Cutler and Gruber (1996) also examined the effects of expanding Medicaid at the family level using a family eligibility measure, and they concluded that Medicaid expansions reduced private insurance coverage of children and women of child-bearing age by 50 percent.

There are two shortcomings related to the results of this study. First, Cutler and Gruber (1996) examined the effect of increasing eligibility on the total population rather than focusing on poor and near-poor pregnant women and children, the target group of Medicaid expansions. Measuring the effect of Medicaid eligibility expansions on the total population confound estimation results, since the target group of the Medicaid expansions represents less than 20 percent of the total population (Cole 1995). For example, the state and year controls used by Cutler and Gruber (1996) are insufficient to capture trends in health insurance coverage that are specific to groups affected by the Medicaid expansions. In particular, changes in health insurance coverage of the low income population may be largely different than those of the high income population (Acs and Steuerle 1993).

Second, Cutler and Gruber (1996) attributed the total reduction in private insurance coverage to Medicaid. It is, however, important to note that a significant number of eligible individuals may have lost private coverage in the years following the expansions and remained uninsured rather than enrolling in Medicaid. The loss of private coverage for these individuals may be temporary, and they may reacquire private health insurance in a short time period (Monheit and Schur 1988). The data employed by Cutler and Gruber (1996) cannot identify whether previously privately insured individuals became uninsured or participated in Medicaid. Cutler and Gruber (1996), by attributing the total decline in private insurance to Medicaid, find that 50 percent of enrollment in Medicaid came from the privately insured.

Another study on crowd out is by Dubay and Kenney (1996a). The authors also used the March CPS, as did Cutler and Gruber (1996), yet attempted to identify pregnant women by matching all infants under age one to their mothers. The March CPS is a nationally representative sample and contains detailed data on health insurance coverage, but it underreports Medicaid and other welfare program participation compared to the actual number of program participants (Ku et al. 1990). To overcome this problem, Dubay and Kenney (1996a) imputed health insurance status for each individual as opposed to using the reported measure. In particular, they assign Medicaid coverage using Medicaid eligibility. There are two problems related to this issue. First, Medicaid eligibility itself is measured with error. As Figure 3 of their paper indicates, 21.8 percent of ineligible individuals report Medicaid coverage. Second, since assigning Medicaid coverage for individuals artificially increases Medicaid participation rate, this creates an upward bias in their take-up rate estimates, and a downward bias in their crowd out estimates.

Dubay and Kenney's (1996a) focus on poor and near-poor pregnant women, the target group of Medicaid expansions, is an important advance. In this study, the authors accounted for time-varying state-year effects by using non-pregnant women and men ages 18-44 as control groups, as opposed to the state and year controls used by Cutler and Gruber (1996). Their employment of men as control group is an unappealing approach to net out secular trends, since men may have been affected differently by trends in health insurance coverage compared to pregnant women due to differences in their industry of employment and occupations (Sorensen 1990). Comparing changes in health insurance coverage for men and pregnant women, Dubay and Kenney (1996a) found 14 percent crowd out for pregnant women between the years 1988 and 1992. Similar to Cutler and Gruber (1996), however, this strategy attributes the total reduction in private coverage to Medicaid due to use of pooled cross-sectional data that cannot differentiate between individuals who lost private insurance and became uninsured, and those who switched from private insurance to Medicaid.

Shore-Sheppard (1996a), using the same data as Cutler and Gruber (1996) and Dubay and Kenney (1996b), examined the extent of crowd out for children and women ages 15 to 45. In this study, Shore-Sheppard (1996a) focused on the total population rather than on poor and near-poor women and children, a strategy that may be problematic as discussed previously. The author regressed state-level changes in health insurance coverage on changes in the fraction eligible. Similar to previous studies, this procedure attributed all changes in health insurance coverage to Medicaid. Shore-Sheppard (1996a) found an increase in private coverage for children and women of childbearing age between 1988 and 1993. Findings of this state-level analysis suggested that even though some individuals who were previously covered by private insurance switched to Medicaid or became uninsured following the expansions, private coverage increased overall due to the flows into private insurance. The author interpreted these results as no evidence of crowd out.

Finally, Shore-Sheppard (1996b) extended her previous work by examining the effect of the Medicaid expansions on the distribution of health insurance coverage by income groups. In particular, she focused on health insurance coverage rates of children from low income families, and found no crowding out of private insurance.

In summary, previous studies reached different conclusions regarding the effect of the Medicaid expansions on health insurance coverage. Surprisingly, these differences are found in studies that use the same pooled cross-sectional data. I build upon and extend past research using longitudinal data that allow me to identify changes in health insurance coverage of the same individuals over time compared to examining health insurance coverage of different groups of people at two points in time, as is done in previous studies using pooled cross-sectional data. Furthermore, I focus on poor and near-poor children, the target group of Medicaid expansions, rather than on the total population. Finally, I compare health insurance coverage of poor and near-poor children to identify the effect of expanding Medicaid income eligibility on health insurance coverage of near-poor children, net of any trends in insurance coverage that may affect both groups similarly.

C. Description of Laws [1]

Medicaid is a federal-state matching program providing medical assistance for (1) low-income aged, blind, or disabled individuals, (2) members of families with dependent children receiving welfare, and (3) low-income pregnant women and children. In the 1980s, Medicaid eligibility was greatly expanded for the third group. A series of legislation changes starting in 1984 severed the historical tie between Medicaid and the cash assistance program –Aid to Families with Dependent Children (AFDC).

The Deficit Reduction Act of 1984 (DEFRA) entitled first time pregnant women and pregnant women in two-parent families where the principal breadwinner was unemployed, to

[1] The discussions in this section are based on Medicaid Source Book (Congressional Research Service 1988; 1991), and Green Book (U.S. Committee on Ways and Means 1991).

Medicaid coverage, if they met AFDC income and resource standards.[2] DEFRA also provided that children up to age 5 born after September 30, 1983 were eligible. The Consolidated Omnibus Budget Reconciliation Act of 1985 (COBRA) further required the eligibility of pregnant women in two-parent families with an employed principal breadwinner. Both DEFRA and COBRA restricted coverage to families who met AFDC income and resource standards, but not the categorical tests for eligibility.

The Omnibus Budget Reconciliation Act of 1986 (OBRA 86), for the first time, gave states the option to severe the historical tie between AFDC and Medicaid. OBRA 86, effective April 1, 1987, expanded Medicaid eligibility to pregnant women and infants under age 1 with income as high as 100 percent of Federal poverty level (FPL). In addition coverage was extended to children under age 5 on a phased basis. OBRA 86 also permitted states to streamline eligibility for these groups. Actions adopted by states to expand and speed Medicaid enrollment include (1) dropping the assets test, (2) adopting continuous eligibility –continuous coverage over the pregnancy and post-partum period regardless of any changes in income, (3) adopting presumptive eligibility –granting access to prenatal care financed by Medicaid beginning on the date on which a qualified provider determines that the family income falls below the state's Medicaid eligibility threshold, (4) placing outstationing eligibility workers –assigning eligibility workers to accept applications in prenatal clinics, and (5) shortening Medicaid application forms.

Effective July 1, 1988, Omnibus Reconciliation Act of 1987 (OBRA 87) expanded Medicaid coverage to pregnant women and infants under age 1 whose family income were below 185 percent of FPL. It also permitted states to enroll children under age 2, 3, 4, or 5 who were

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[2] To receive AFDC payments, a family must pass a gross income test and a countable income test. Families with gross income exceeding 185 percent of the State's need standard are ineligible for AFDC. Additionally, families' gross income less certain disregards must be less than State's need standard. AFDC applicants meeting the income criteria must also pass a resource test. The value of a family's property cannot exceed $1,000 per family unit. Categorically needy children who have been deprived of parental support or care because their father or mother is absent from the home continuously, is incapacitated, is deceased or is unemployed, and some others living with such children are eligible for AFDC.

born after September 30, 1983 with family incomes up to 100 percent of FPL. Effective October 1, 1988, states were required to cover children up to age 7 (optional up to age 8) who were born after September 30, 1983 and who met income and resource requirements of the state's Medicaid plan.

Omnibus Reconciliation Act of 1989 (OBRA 89), effective April 1, 1990 offered Medicaid coverage to pregnant women and children under age 6 born after September 30, 1983 with family incomes below 133 1/3 of FPL. OBRA 89 was the end of these expansions, since it required publicly subsidized care to groups with higher incomes. Finally, Effective July 1, 1991 Omnibus Reconciliation Act of 1990 (OBRA 90) extended coverage for children under age 19 who were born after September 30, 1983 with family incomes below 100 percent of FPL.

In summary, beginning in 1984, Medicaid expansions extended coverage to low-income pregnant women and children with no ties to the AFDC program, and raised the eligibility income thresholds up to 185 percent of FPL. As a result, in 1991, all states had Medicaid eligibility thresholds at or above 133 percent of FPL. The legislative changes that took place nationwide aimed to ease access to health care of those with no health insurance coverage. Whether the Medicaid expansions were successful in achieving this aim is an empirical question that will be addressed in the next sections.

D. Analytical Model

Medicaid expansions in the mid-1980s aimed to increase access to prenatal care, delivery services and child health care by raising the income level at which pregnant women and children would become eligible for subsidized health insurance coverage. By reducing the money price of these services to zero, the program intended to decrease the number of uninsured pregnant women and children who received inadequate care. An unintended effect, however, may also be a reduction in the number of those who are privately insured. Low-income families may choose to substitute publicly subsidized care for private care. Thus, it is possible that Medicaid expansions may have caused crowding out of private health insurance. Although this issue is an empirical question, the following theoretical model provides some insight on why those who are uninsured and privately insured would choose to enroll in Medicaid following the expansions in the mid-1980s.

The analytical framework is based on Grossman's (1972) model of the demand for health. The utility of a parent depends on child health (*H*), consumption of other market goods (*X*), and leisure (*L*):

(1) $$U = U(H, X, L)$$

Health is produced using medical care (*M*):

(2) $$H = H(M)$$

Additionally, waiting and travel time (*TH*) are required to obtain one unit of medical care. Utility and health functions are assumed to have the same form for all individuals. The budget constraint, however, differs depending on the type of health insurance coverage – private insurance, Medicaid, and uninsured.

1. Private Insurance Coverage

Private insurance can be paid either by the individual, the employer, or a combination. The total cost of health insurance (*B*) is subtracted out of the individual's total earnings (wT). In the case of employer-sponsored insurance, however, the amount deducted depends on the payment arrangements between the employer and the employee. Define γ as the fraction of *B* paid directly by the worker.[3] Thus, the total amount of income earned is $[wT - \gamma B]$, where $0 \leq \gamma \leq 1$. Note also that the benefit payment does not depend on the hours of work. The budget

[3] For simplicity, assume that γ is not a function of B, put differently, it does not take into account reductions in the wage as B rises.

constraint of an individual with private insurance is:

(3) $X + w[M\,TH_p + L_p] = wT - \gamma B + N$

The total income is the sum of wage income and property income (N) minus the value of health insurance benefit B times the share paid by the employee. The subscript p indicates that time input and leisure are allocated under the assumption that the individual is covered by private insurance. If $\gamma = 1$, then the full cost of health insurance benefit is subtracted out of total earnings. If $\gamma = 0$, then employer pays for the health insurance, and the worker receives full amount of the earnings, wT.

The price of the consumption good is normalized to one. The price of time allocated to receive medical care is the hourly wage rate and indicates the opportunity cost of time. The total cost of medical care is equal to $[w\,M\,TH_p + \gamma B]$.

2. Medicaid Coverage

An individual who participates in Medicaid obtains subsidized health insurance coverage for medical care. The only cost of Medicaid coverage is the amount of time spent in receiving medical care. The budget constraint is given by:

(4) $X + w[M\,TH_m + L_m] = wT + N$

The subscript m indicates that time input and leisure are allocated under the assumption that the individual is covered by Medicaid. The cost of medical care is the value of $[w\,M\,TH_m]$, and depends only on the amount of medical care obtained.

3. No Health Insurance Coverage

An uninsured individual, in addition to the time price, pays the unit price of P for each amount of medical care obtained. The budget constraint is given by:

(5) $$X + P\,M + w[M\,TH_u + L_u] = wT + N$$

The subscript u indicates that time input and leisure are allocated under the assumption that the individual does not have any health insurance coverage. The total cost of obtaining medical care is $[P\,M + w\,M\,TH_u]$.

4. Indifference among Types of Health Insurance Statuses

In order to compare these three types of health insurance statuses, a relation between different time inputs is constructed to ease interpretation of the results. Assume that the time allocated to receive medical care by privately insured and uninsured individuals is proportional to the time input of Medicaid recipients.

(6) $$\begin{aligned} TH_p &= \alpha\, TH_m \\ TH_u &= \beta\, TH_m \end{aligned}$$

Under these conditions, complete indifference among the three alternatives of health insurance can be obtained by the following identity:[4]

(7) $$\underbrace{\alpha\, w\, M\, TH_m + \gamma B}_{Private} = \underbrace{w\, M\, TH_m}_{Medicaid} = \underbrace{P\,M + \beta\, w\, M\, TH_m}_{Uninsured}$$

Thus, if the opportunity cost of the time spent in health production of a Medicaid recipient is lower than the cost incurred to a privately insured individual, or it is lower than the amount paid for medical care by an uninsured individual, then Medicaid participation will increase.

5. Uninsured to Medicaid (Take Up)

The total price of medical care paid by an uninsured individual is usually assumed to be larger than the time price paid by a Medicaid recipient $[P > (1-\beta)\, w\, TH_m]$. Thus, the low cost

[4] This specification allows X, L, and M to be identical in each category of health insurance coverage.

of Medicaid increases the likelihood of participation in the program by near-poor families who were ineligible prior to the expansions. We expect more uninsured individuals to enroll in Medicaid following the increase in Medicaid income thresholds.[5] Moreover, Medicaid expansions will decrease the price of medical care relative to the price of consumption good X. Thus, an increase in the consumption of medical care M and a reduction in the consumption of good X may be expected.[6] Medicaid expansions would increase utilization of health care services among previously uninsured.

6. Privately insured to Medicaid (Crowd Out)

If the total price of medical care paid by the privately insured individual is higher than the time cost of Medicaid-financed care, $[\gamma B > (1-\alpha)\, M\, TH_m]$, then individuals would substitute Medicaid for private insurance. However, the extent of substitution between private insurance and Medicaid may be determined by the wage rate, and the quality of care available through Medicaid.

A high current wage rate may lead the time component of the price, $[(1-\alpha)\, M\, TH_m]$, to be greater than the money component, (γB), of medical care. Thus, individuals with relatively higher current wages would choose to keep their private insurance and those with relatively low wages would substitute Medicaid for private insurance.[7] For an individual with a low current wage but a high expected future wage, however, the choice may be different. For example, pregnant women who deliver, lose their coverage after the postpartum period (sixty days after delivery). Large transaction costs may discourage women with private insurance from giving it

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[5] Some uninsured individuals may reject Medicaid due to stigma associated in the public program (Moffitt 1983).

[6] Assuming that the substitution effect is larger than the income effect for consumption good X.

[7] The discussion is based on wage variations within a certain range and recognizes that most people eligible for Medicaid have low wage levels.

up. Individuals with low current wage rates may anticipate future wage growth particularly if they are investing in human capital. If individuals with low current income but higher expected future income develop medical problems in the future, these pre-existing conditions may prevent them from obtaining private health insurance. In summary, the effect of the expansions on privately insured individuals with a given wage rate is ambiguous.

A second issue is that crowd out is expected to be more significant in areas where private providers are more likely to accept Medicaid recipients. Medicaid may provide relatively lower quality of service, and an individual who values health highly may be willing to continue to receive care from his or her private provider. Then, publicly subsidized care may be more attractive for those who are eligible and living in areas where there are private providers who are willing to accept Medicaid patients compared to those living in areas where Medicaid recipients are mostly restricted to clinics and outpatient departments in hospitals.

In sum, Medicaid expansions may affect privately insured pregnant women and children as well as those who are uninsured. First, as eligibility for free health insurance coverage increases following the expansions, the number of uninsured individuals in the population is expected to decrease over time. Second, some privately insured individuals with incomes below the Medicaid income thresholds became eligible for the public program after the expansions. Since Medicaid may have a positive time price and may not provide the same quality of health care as private insurance, Medicaid expansions would have an ambiguous effect on those with private insurance coverage.

E. Data

The data sets used in this analysis are the National Longitudinal Survey of Youth (NLSY) for the years 1989 and 1992, and the 1988 National Maternal Infant Health Survey (NMIHS) and its 1991 Longitudinal Follow-up (LF). These data provide a unique opportunity to examine the effects of Medicaid expansions on changes in health insurance coverage of poor and near-poor children. The NLSY is a longitudinal survey of youth who were born between the years 1957 and 1965. It oversamples black, Hispanic, and economically disadvantaged non-black and non-Hispanic youths. For the purposes of this paper, I focus on the children of this group for the years 1989 and 1992 to examine the effect of the Medicaid expansions on health insurance coverage over time.[8]

The NMIHS interviewed 9,953 women who had live births, 3,309 women who had fetal deaths, and 5,273 women who had infant deaths in 1988, and linked survey information to birth and death certificates. The 1991 LF conducted interviews with 8,285 children of the 1988 live birth cohort. The NMIHS oversamples black and low birth weight infants. This analysis is limited only to the live birth sample of the NMIHS, since health care insurance coverage information over time is only available for this group. In the NMIHS, all children are infants in 1988 and are aged 2 to 4 in 1991.[9]

Both of the data sets contain detailed information on health insurance coverage of children. The insurance coverage can be grouped under four main categories: Private, Medicaid, uninsured, and other such as CHAMPUS and Indian Health Service.[10,11] In addition, they include

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[8] Health insurance coverage information of the youngest child in the NLSY was only available for 1989, 1990, and 1992.

[9] The age of the sample child in 1991 depends on the date of delivery and the month of the interview in 1991. For example, a child born in December 1988 and interviewed in January 1991 is 2 years old.

[10] A drawback in the LF, however, is the measurement error related to Medicaid coverage. The only available information in the LF is whether the child has ever been covered by Medicaid. On the other hand, respondents were asked whether there was ever a period that the child was not covered by any health insurance, and whether the child's health insurance coverage pays all, part, or none of the health care bills. Using this information, I was able to identify health insurance coverage status for 87 percent of children in the sample.

[11] Children who reported dual coverage are not excluded, since estimation results do not change with and without them. Dual coverage rates increase, however, over time in both the NLSY and the NMIHS.

extensive information on demographic characteristics, such as family income, family composition, and labor force data. Appendix Tables A1 to A4 present summary statistics for the NLSY and the NMIHS, respectively.

1. Medicaid Eligibility

For each child, eligibility is imputed using age, family wage income, federal poverty level, and Medicaid income thresholds.[12] The data for Medicaid eligibility thresholds and effective dates for expansions of coverage for children for each state come from Hill (1992, Table 1 and Table 2). Family wage income, rather than total family income, is used to calculate eligibility.[13] For the focus group, low-income families, wage income is almost the only source of income.[14]

Tables 1 and 2 compare Medicaid eligibility and Medicaid coverage rates for children using alternative income measures from the NLSY and the NMIHS, respectively. For the NLSY, utilization of the family wage income provides more accurate estimates of eligibility compared to total family income as illustrated in Table 1. The proportion of children ineligible for Medicaid, but covered by Medicaid is much lower using the family wage measure.[15]

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[12] All relatives and other children are considered as family members in the NMIHS to calculate the corresponding federal poverty level for a family, with reference to the following definition. "The Census Bureau defines families as follows (Rawlings, 1993:B-2): family: a group of two persons or more related by birth, marriage, or adoption and residing together; all such persons (including related subfamily members) are considered as members of one family" (National Research Council 1995).

[13] Family income is calculated in regard to the definitions given in footnote 2 in section *Description of Laws*. In particular, child care expenses and standard earned income allowance are subtracted of the family income.

[14] Currie and Gruber (1996a) report that 75 percent of the average child's family income comes from his or her parents' earnings.

[15] For the purposes of this study, I use eligibility calculated using family wage in the last year rather than family wage at the time of the interview. Estimates using both wage measures are practically the same, but the sample size is larger using family wage in the last year.

For the NMIHS, both family wage income and total family income are measured with significant error in 1988, as evidenced in Table 2.[16,17] On the other hand, the measurement error related to income plagues all previous studies. For example, Dubay and Kenney (1996a), using the March CPS, report that 21.8 percent of pregnant women with incomes between 100 and 185 percent of poverty in 1988 are covered by Medicaid, even though these women were ineligible for Medicaid in that year. I use eligibility calculated using family wage income in the analyses, since the proportion of children ineligible for Medicaid, but covered by Medicaid is lower using this measure compared to that using total family income.

2. Endogeneity of Medicaid Eligibility

Eligibility is primarily determined by state rules, but macroeconomic trends and changes in individual behavior may also affect eligibility. Some families who would normally have incomes above the Medicaid thresholds may reduce their incomes up to the program cutoff, by reducing their hours of work or altering their demographic characteristics (i.e. by getting divorced), to gain access to the publicly subsidized insurance coverage (Ashenfelter 1983; Yelowitz 1995a; 1995b). Therefore, it is possible that eligibility is correlated with unobserved determinants of health insurance coverage.

I address this issue by calculating Medicaid eligibility in 1989 (1988 for the NMIHS sample) using 1989 rules and 1989 income, and then using 1992 rules (1991 for the NMIHS

[16] The 1988 NMIHS interviewed women with mean lag of 15 months, and asked about their family income for the 12 months before their delivery. Accordingly, family income is subject to severe measurement error in the 1988 sample. The 1991 LF, however, provides information on the last month's income for each family member, and it is accurate.

[17] I also used 1989 and 1992 CPS to predict family wage for the NMIHS. In 1989, using the March CPS, I regressed family income for women with a child at age 0, on demographic characteristics such as education, marital status, number of children in the family, and labor force participation. Then, I used the estimated coefficients to predict the 1988 wage for families in the NMIHS. Using the 1992 March CPS, I repeated the same procedure for women with a child at age 3 to predict 1991 wage for families in the NMIHS. The results, however, failed to improve the eligibility estimations for the NMIHS sample.

sample) and 1989 income. This procedure controls for any endogenous changes in the family income measure. For example, an individual who was ineligible in 1989 with an income level of 300 percent of poverty level, may have lowered his or her income intentionally and made himself or herself eligible in 1992. Thus, using family income in 1989 to calculate eligibility in 1992 will exclude individuals who adjust their income levels in order to gain access to Medicaid coverage.

F. Empirical Analysis and Results

1. Changes in the Distribution of Health Insurance Coverage

The effect of Medicaid expansions on health insurance coverage for children can be analyzed by examining changes in the distribution of coverage for the same individuals over time. Observing changes in health insurance coverage for a child between 1989 and 1992 (1988 and 1991 for the NMIHS sample) will provide estimates of crowd out and take-up rates following the expansions. It is, however, important to examine changes in insurance coverage for the group of children targeted by the Medicaid expansions. Accordingly, I focus on children who were ineligible prior to the expansions, but who would be eligible under the expanded rules, and children who were eligible before and after the expansions. For these children, Medicaid eligibility in 1992 is determined using their 1989 family income and 1992 rules. As discussed in the previous section, employment of a constant population directly addresses the potential endogeneity of the eligibility measure.

Table 3 presents changes in health insurance coverage for children using the NLSY and the NMIHS sample. The first panel shows changes in insurance coverage for children who were ineligible in 1989, but who would be eligible under the 1992 rules for the NLSY sample. As the first row of this panel shows, 15 percent of children who were privately insured in 1989 enrolled in Medicaid in 1992, 12.4 percent became uninsured, 69.4 percent remained in private insurance, and 5.2 percent enrolled in other insurance coverage. These findings indicate that even though some children switched from private insurance to Medicaid, approximately the same percentage of privately insured children became uninsured or enrolled in other insurance coverage.

Some previous studies interpreted the total reduction in private coverage as crowd out. It is, however, potentially misleading to attribute the total decline in private coverage to Medicaid. Linear probability models that are used to estimate the effect of Medicaid expansions on privately insured individuals cannot distinguish between individuals who switched to Medicaid and those who became uninsured, and will result in rather larger estimates of crowd out. Indeed, Cutler and Gruber (1996) using linear probability models estimate a crowd out rate of 50 percent for women of childbearing age and children. These models simply show a reduction in private coverage associated with the Medicaid expansions that offsets an increase in Medicaid participation, but the dynamics of health insurance coverage are apparently much more complicated.

The effect of the Medicaid expansions on uninsured children who were ineligible in 1989, but who would be eligible under the 1992 rules are presented in the second row of the first panel of Table 3. For this group, 30.6 percent of previously uninsured children enrolled in Medicaid in 1992, 36.9 percent remained uninsured, 29.7 percent obtained private coverage, and 3.6 percent enrolled in other insurance coverage. These figures suggest that a significant amount of near-poor children participated in Medicaid following the eligibility expansions.

The figures in the first panel of Table 3 indicate 15 percent crowd out for privately insured children and 30.6 percent increase in Medicaid participation for uninsured children. Findings of the second panel obtained using the NMIHS sample are quite similar and supportive of these results for children who were ineligible in 1988, but who would be eligible under the 1991 rules, and suggest a crowd rate of 13.4 percent and a take-up rate of 33.1 percent. Although these findings are evidence of the increase in Medicaid enrollment rates, they may as well be representing the effect of macroeconomic trends in the economy. In particular, nationally, there was a decline in employment-based health insurance from 56.1 percent in 1988 to 54.3 percent in

1993 (Fronstin 1996), thus, crowd out rates may be lower than the figures of Table 3 suggest.[18]

To control for unobserved time-varying factors, and to measure the net effect of expanding Medicaid income eligibility on the target population, I examine changes in health insurance coverage for children who were eligible for Medicaid before and after the expansions. As the third and fourth panels of Table 3 illustrate, the expansions lead to greater Medicaid enrollment for this group compared to children who were ineligible prior to the expansions, but who would be eligible under the expanded rules using the NLSY and the NMIHS samples, respectively. These children were always eligible for Medicaid, yet they only participated in the public program following the expansions. One possible explanation for the large effect of the expansions on health insurance coverage of children who were always eligible in the public program is that many states implemented programs to streamline access to coverage and simplify eligibility process simultaneously with the income expansions. A second explanation is that Medicaid expansions severed the link between the publicly subsidized insurance coverage and the cash assistance program, AFDC, and reduced the stigma associated with the welfare program.[19] A third explanation is that the expansions may have increased the number of providers who accept Medicaid patients. Another explanation is that employers may have decided to drop health insurance coverage as greater numbers of their employees became affected by Medicaid because of the expansions. Finally, macroeconomic trends may have led this group to switch to Medicaid. For example, those who were poor, yet working and privately covered, may have lost their jobs and may have enrolled in Medicaid.

Comparing changes in coverage of the first two panels – children who were ineligible prior to the expansions, but would be eligible under the expanded rules – to changes in coverage

[18] There was a positive trend for private insurance coverage in the NLSY. In 1989, respondents of this sample were aged 24 to 31. Therefore, they are more likely to acquire better jobs over time, that would be reflected as a gain in private coverage from 1989 to 1992. See Table 4.

[19] Moffitt (1983) addresses the disutility arising from participation in a welfare program (welfare stigma) for individuals in the low-income population.

of the last two panels – children who were eligible before and after the expansions – shows only the effect of increased income thresholds on health insurance coverage. Streamlined enrollment procedures, reduction in stigma, or secular trends in health insurance coverage probably affect newly eligible and always eligible groups similarly, and comparing experiences of children who would only be eligible under the expanded rules, to children who were always eligible will provide evidence of the net effect of expanding Medicaid income eligibility on private coverage and Medicaid participation. Accordingly, a comparison of the crowd out and take up estimates indicates that **only** expanding Medicaid income eligibility has no effect on Medicaid participation or private insurance coverage, since percentage of children who switched from private insurance to Medicaid and percentage of uninsured children who enrolled in Medicaid are almost the same for both groups.

2. Difference-in-Differences Estimates

Difference-in-differences (DD) estimators are used to control for secular trends that the previous analysis had to infer with rough comparisons. In regard to the underlying assumption of the DD estimators, this procedure determines the effect of Medicaid expansions on health insurance coverage using treatment and control groups. Health insurance coverage of the treatment group changes over time due to Medicaid eligibility expansions and other unobservable factors. Health insurance coverage of the control group, however, changes only due to the effect of time-varying shocks that are not related to increased eligibility. The difference in the change in insurance coverage between treatment and control groups indicates the net effect of the increase in income thresholds on the target group of the Medicaid expansions.

The treatment group in 1989 (1988 for the NMIHS sample) is defined as children who are ineligible in 1989, but who would be eligible under the 1992 (1991 for the NMIHS sample) rules. Similarly, the treatment group in 1992 (1991 for the NMIHS sample) is defined as children who are eligible in 1992, but who would be ineligible under the 1989 (1988 for the NMIHS sample) rules. The control group is children who were always eligible for Medicaid before and after the expansions.

Table 4 presents the results of DD estimates for children by treatment and control groups in 1989 and 1992 using the NLSY sample. The top panel shows the results pertaining to Medicaid participation. Expanding eligibility has a positive and statistically significant effect on both treatment and control groups. Medicaid participation increases for children who were ineligible before the expansions, but who were eligible after the expansions – treatment group – and for children who were eligible before and after the expansions – control group. The treatment group may participate in Medicaid due to expanded income thresholds, streamlined eligibility, reduced stigma, or secular trends in health insurance coverage. On the other hand, the control group may increase participation in Medicaid due to the same factors that affect the treatment group, except for the expansions in income eligibility. The difference in Medicaid participation from 1989 to 1992 indicates a similar trend for both treatment and control groups and is consistent with the results of Table 3. The difference-in-differences shows only the effect of increased income thresholds on treatment group, rather than other factors that affect both groups in the same way. The DD estimator is 0.4 percentage points and statistically insignificant, suggesting that even though Medicaid expansions were successful in increasing participation in the public program, **increasing income thresholds only**, had no effect.

The second panel of Table 4 presents the effect of Medicaid expansions on private insurance coverage. Among children who were ineligible before the expansions, but eligible after the expansions – the treatment group – private coverage decreases. Some previously privately covered children became uninsured or switched to Medicaid, and some previously uninsured children enrolled in private insurance. The difference of private insurance coverage from 1989 to 1992 indicates the effect of Medicaid expansions and secular trends in health insurance coverage on the treatment group. As the first row of the second panel indicates private coverage declined by 1.8 percentage points for the treatment group. Combined with the first row of the top panel, this result suggests a crowd out rate of 12 percent (1.8/15.1) without controlling for unobserved trends. Similarly, among the control group, children who were always eligible for Medicaid before and after the expansions, private coverage decreases by an insignificant 0.4 percentage points from 1989 to 1992. The difference-in-differences estimate for private insurance coverage shows that the effect of expanding income eligibility is -1.4 percentage points after controlling for secular trends in insurance coverage. This estimate is also insignificant. These results suggest no crowding out of private insurance following the increase in Medicaid income thresholds, since there is virtually no change in Medicaid participation, and the DD estimates of both Medicaid participation and private coverage are insignificant.

The effect of Medicaid expansions on children who were previously uninsured is presented in the last panel of Table 4. The expansions have a negative and significant effect on both treatment and control groups. Medicaid expansions have approximately the same effect on children who were ineligible before the expansions, but were eligible after the expansions, and children who were eligible before and after the expansions. The difference in the proportion of uninsured children from 1989 to 1992 indicates a declining trend for both treatment and control groups that is consistent with the results of Table 3. The decrease in the proportion of uninsured children is approximately equal to the increase in Medicaid participation for both groups. Statistically significant changes in Medicaid enrollment and uninsured by the same magnitude and statistically insignificant change in private insurance coverage suggest little crowd out for the target group of Medicaid expansions.

The results of Table 4 pertaining to the NLSY sample are consistent with those of Table 3. They both show little crowd out and large take-up rates for poor and near-poor children. Table 3 shows the distribution of health insurance coverage for a constant population, and provides estimates of potential eligibility on health insurance coverage of children. Table 4 indicates changes in health insurance coverage for a population that changes over time. The similarity of the estimates of potential and actual eligibility suggests that the endogeneity of eligibility is not an issue.[20] Moreover, the DD procedure used in Table 4 shows the net effect of expanding Medicaid income eligibility by providing direct comparisons of experiences of treatment and control groups that the analysis in Table 3 had to infer with rough comparisons.

Table 5 presents the results of DD estimates for children by treatment and control groups in 1988 and 1991 using the NMIHS sample. A potential problem related to the NMIHS sample is the inaccuracy of the eligibility measure, calculated using the family wage in 1988. For example, as the top panel of this table indicates, Medicaid participation is 24.7 percent for the treatment group who was ineligible for Medicaid in 1988. The measurement error related to income, however, is similar to that in the March CPS as reported by Dubay and Kenney (1996a). Therefore, the results pertaining to Table 5 will be discussed based on the direction of the estimates rather than their magnitudes.

The first two panels of Table 5 present the effect of the expansions on Medicaid participation and private coverage for treatment and control groups. Medicaid participation increases and private coverage decreases for children who were ineligible prior to the expansions, but eligible after the expansions, and for children who were always eligible before and after the expansions. Similar to Table 4, the results of Table 5 suggest no crowding out of private insurance following the increase in Medicaid income thresholds, since there is no increase in Medicaid participation due to an increase in Medicaid income thresholds, and the DD estimate of change in private coverage is statistically insignificant and positive.

The results pertaining to uninsured children are presented in the last panel of Table 5. These estimates are similar and supportive to those of Table 4 using the NLSY sample, and show a decrease in the proportion of uninsured children from 1988 to 1991 for both treatment and control groups. In sum, findings of Table 5, although subject to measurement error, indicate

[20] Cutler and Gruber (1996) use instruments for Medicaid eligibility, assuming that eligibility is endogenous, but do not present OLS estimation results nor statistical tests to justify the use of instruments.

trends for Medicaid participation and private insurance coverage similar to those in Table 4. These findings suggest no crowd out for privately insured children and large increase in Medicaid participation for uninsured children between the years 1988 and 1991.

3. Demographic Characteristics, Streamlining Eligibility, and Medicaid Expansions

As discussed in the theoretical model, Medicaid expansions may have had differential effects on individuals. For example, individuals with higher human capital (e.g. education), yet low current wage may anticipate future wage growth, and choose not to participate in Medicaid. These differential effects are estimated using pooled cross-sectional data by interacting Medicaid eligibility with demographic characteristics such as marital status, education, labor force participation, and residence in a SMSA. Additionally, the effect of streamlining eligibility is estimated using interactions of Medicaid eligibility with state implemented programs such as dropping asset tests, outstationing of eligibility workers, and shortening of the Medicaid application form.[21]

Tables 6 and 7 present the regression estimates using the NLSY and the NMIHS, respectively. Each column contains separate regressions for Medicaid participation, private insurance, and uninsured. Linear probability models are obtained for ease of computation and interpretation. The coefficients of each set of demographic characteristics and state implemented rules are obtained with separate regressions. Except the first row, eligibility is entered in each set of regression as an interaction with the mutually exclusive events (i.e. married, separated, never married). Standard errors are corrected for heteroscedasticity and repeat sample problems using Huber's (1967) method. State dummy variables are included in all regressions. The sample is restricted to children who were ineligible in 1989 (1988), but who would be eligible under the 1992 (1991) rules, and children who were eligible both under the 1989 (1988) and 1992 (1991)

[21] A detailed discussion for these state-implemented programs can be found in Frost et al. (1993).

rules.

The first row presents estimates of the effect of expanding Medicaid income eligibility on health insurance coverage for children. The coefficients have the expected signs, and are statistically significant for Medicaid coverage and private insurance coverage. Note, however, that the decrease in private coverage is misleadingly suggestive of crowd out, since privately insured individuals switched to Medicaid, to other coverage, or became uninsured.

Estimates of the interactions between marital status and eligibility indicate that conditional on being eligible, children of separated or never married women have a higher probability of Medicaid participation and lower probability of private insurance coverage or no insurance coverage than those of married women. Estimates of the interactions between education and Medicaid eligibility indicate that conditional on being eligible, children of higher educated individuals are less likely to participate in Medicaid and more likely to have private insurance coverage than those of women with lower education. These results suggest that children of women with more human capital are less likely to participate in Medicaid. Women with higehr human capital have an anticipated future wage growth, or a high of value of time that is most needed for the publicly subsidized coverage. Estimates of the interaction between labor force participation and eligibility suggest that children from eligible families with a non-working head of household are more likely to enroll in Medicaid, and less likely to have private coverage than those from eligible families with a working head of household. Finally, eligible children from families whose current residence is in a central city of SMSA are more likely to participate in Medicaid, indicating that being eligible and living in areas where there are private providers who are willing to accept Medicaid patients increases Medicaid enrollment.[22]

Estimates from Table 6 of the effect of streamlining eligibility conditional on being

[22] Eligibility and residence interactions are not included in Table 7, because of the lack of information on residence measure. In the 1988 NMIHS, questions on whether the respondent was living in an urban area come from the birth certificate. In the 1991 LF, respondents were not asked about their area of residence.

eligible indicate that some of these state-implemented programs increased Medicaid enrollment for children.[23] Conditional on being eligible, dropping asset tests decreased the percent of uninsured individuals. It had no effect, however, on those who were covered by Medicaid or by private insurance. Outstationing eligibility workers, or shortening of application forms decreased the probability of being uninsured, increased the probability of covered by Medicaid, and had no effect on private coverage. These findings are in accord with those of Kenney and Dubay (1995) who, in a similar context, find a sizable effect of streamlining eligibility using county level data. These results are evidence of one source of the trends in insurance coverage that affected both poor and near-poor children as discussed in regard to the findings in Tables 3 to 5.

G. Summary and Conclusion

This study analyzed the effect of Medicaid eligibility expansions on health insurance coverage for children using two panel data sets: the National Longitudinal Survey of Youth (NLSY) and the National Maternal and Infant Health Survey (NMIHS). Results suggested that expanding Medicaid income eligibility had no effect on private insurance coverage for near-poor children.

An interesting finding of this study is that Medicaid expansions had approximately the same effect on Medicaid enrollment rates for those who were initially ineligible, but who became eligible after the expansions, and those who were eligible before and after the expansions. These results suggested that Medicaid expansions were successful in increasing the proportion of the population with health coverage. They achieved this not because of the expanded eligibility, but by severing the link between Medicaid and the cash assistance program, by streamlining eligibility, or by increasing the number of providers who accept Medicaid patients, that in return

[23] In Table 4, results of the F-test that measures whether there is a difference between the coefficients are insignificant in all models of the effect of streamlining eligibility on health insurance status.

affected the take-up rates of both newly eligible and always eligible populations.

Further expansions in Medicaid income thresholds may successfully continue to decrease the proportion of uninsured pregnant women and children, without causing crowding out of private insurance. Evidence from longitudinal data shows that privately insured individuals kept their coverage regardless of the large Medicaid expansions in the last decade. Reasons to decline free coverage by privately insured individuals may be high time costs associated with Medicaid or quality of care available through Medicaid, which raise the question of how efficient and beneficial are the services provided through the public program. Thus, future research is necessary to evaluate the effectiveness of Medicaid participation on infant and child health.

Table 1

Comparisons of Medicaid Eligibility and Medicaid Coverage Rates for Children.
Using Total Family Income in the Last Year for the NLSY

	1989	1992
Non-Eligible and Not Covered	74.3	68.0
Non-Eligible but Covered	4.6	3.9
Eligible but Not Covered	13.7	15.5
Eligible and Covered	7.5	12.7

Notes: The first and the second column represent Medicaid eligibility and coverage for children in 1989 and 1992 respectively. Eligibility is calculated using family income in the last year , federal poverty level, and state-established Medicaid income thresholds.

Comparisons of Medicaid Eligibility and Medicaid Coverage Rates for Children.
Using Family Wage Income in the Last Year for the NLSY

	1989	1992
Non-Eligible and Not Covered	70.6	65.1
Non-Eligible but Covered	1.5	1.6
Eligible but Not Covered	16.7	17.6
Eligible and Covered	11.1	15.7

Notes: The first and the second column represent Medicaid eligibility and coverage for children in 1989 and 1992 respectively. Eligibility is calculated using family wage in the last year, federal poverty level, and state-established Medicaid income thresholds.

Comparisons of Medicaid Eligibility and Medicaid Coverage Rates for Children.
Using Family Wage Income at the Time of the Interview for the NLSY

	1989	1992
Non-Eligible and Not Covered	69.5	63.9
Non-Eligible but Covered	1.7	1.8
Eligible but Not Covered	16.8	18.0
Eligible and Covered	12.0	16.3

Notes: The first and the second column represent Medicaid eligibility and coverage for children in 1989 and 1992 respectively. Eligibility is calculated using family wage at the time of the interview, federal poverty level, and state-established Medicaid income thresholds.

Table 2

Comparisons of Medicaid Eligibility and Medicaid Coverage Rates for Children.
Using Total Family Income in the 12 Months Before Delivery for the NMIHS

	1988	1991
Non-Eligible and Not Covered	54.2	44.4
Non-Eligible but Covered	10.8	5.7
Eligible but Not Covered	13.3	21.4
Eligible and Covered	21.6	28.5

Notes: The first column represents Medicaid eligibility and coverage for infants in 1988. The second column represents Medicaid eligibility and coverage for children aged 2 to 4 in 1991. Eligibility is calculated using family income in the 12 months before delivery, federal poverty level, and state-established Medicaid income thresholds.

Comparisons of Medicaid Eligibility and Medicaid Coverage Rates for Children.
Using Family Wage Income in the 12 Months Before Delivery for the NMIHS

	1988	1991
Non-Eligible and Not Covered	52.1	43.7
Non-Eligible but Covered	5.5	1.9
Eligible but Not Covered	16.0	22.1
Eligible and Covered	26.4	32.2

Notes: The first column represents Medicaid eligibility and coverage for infants in 1988. The second column represents Medicaid eligibility and coverage for children aged 2 to 4 in 1991. Eligibility is calculated using family wage in the 12 months before delivery, federal poverty level, and state-established Medicaid income thresholds.

Comparisons of Medicaid Eligibility and Medicaid Coverage Rates for Children.
Using Predicted Family Wage Income Measure in NMIHS

	1988	1991
Non-Eligible and Not Covered	53.0	46.4
Non-Eligible but Covered	8.2	5.7
Eligible but Not Covered	15.5	19.9
Eligible and Covered	23.3	28.0

Notes: The first column represents Medicaid eligibility and coverage for infants in 1988. The second column represents Medicaid eligibility and coverage for children aged 2 to 4 in 1991. Eligibility is calculated using family wage predicted of the March CPS, federal poverty level, and state-established Medicaid income thresholds.

Table 3

Changes in the Distribution of Health Insurance Coverage in 1992 for Children Who Were Ineligible Under the 1989 Rules, but Would Be Eligible Under the 1992 Rules Using NLSY

1989			1992			
			Medicaid	Uninsured	Private	Other
Private	= 1	[N=193]	15.0	12.4	69.4	5.2
Uninsured	= 1	[N=111]	30.6	36.9	29.7	3.6
Other	= 1	[N=18]	16.7	11.1	44.4	33.3
Medicaid	= 1	[N=31]	61.3	16.1	19.4	9.7
			[N=85]	[N=72]	[N=181]	[N=23]

Changes in the Distribution of Health Insurance Coverage in 1991 for Children Who Were Ineligible Under the 1988 Rules, but Would Be Eligible Under the 1991 Rules Using NMIHS

1988			1991			
			Medicaid	Uninsured	Private	Other
Private	= 1	[N=343]	13.4	9.0	65.0	7.3
Uninsured	= 1	[N=151]	33.1	23.8	32.5	5.3
Other	= 1	[N=37]	21.6	5.4	27.0	45.9
Medicaid	= 1	[N=165]	59.4	12.1	22.4	6.7
			[N=202]	[N=89]	[N=319]	[N=61]

Changes in the Distribution of Health Insurance Coverage in 1992 for Children Who Were Eligible Under the 1989 and 1992 Rules Using NLSY

1989			1992			
			Medicaid	Uninsured	Private	Other
Private	= 1	[N=287]	15.7	11.5	67.6	8.7
Uninsured	= 1	[N=284]	39.4	27.1	27.8	7.0
Other	= 1	[N= 46]	19.6	4.3	56.5	32.6
Medicaid	= 1	[N=439]	79.0	8.9	11.4	3.2
			[N=513]	[N=151]	[N=349]	[N=74]

Changes in the Distribution of Health Insurance Coverage in 1991 for Children Who Were Eligible Under the 1988 and 1991 Rules Using NMIHS

1988			1991			
			Medicaid	Uninsured	Private	Other
Private	= 1	[N=351]	24.5	8.8	55.6	6.3
Uninsured	= 1	[N=335]	58.2	14.3	22.1	8.4
Other	= 1	[N=90]	54.4	5.6	20.0	16.7
Medicaid	= 1	[N=1266]	80.3	4.5	11.8	9.4
			[N=1346]	[N=141]	[N=436]	[N=184]

Notes: Each cell of the second column represents participation in health insurance coverage or uninsurance in percentages for a constant population. Number of observation are given in brackets. Total coverage in 1989 (1988) is not equal to total coverage in 1992 (1991) due to dual coverage. Dual coverage rates are higher in 1992 (1991) compared to those in 1989 (1988) in both the NLSY and the NMIHS.

Table 4

DD Estimates of the Effects of Medicaid Eligibility Expansions on Health Insurance Coverage for Children by Treatment and Control Groups in 1989 and 1992, Using NLSY (Family Wage)

Medicaid

	1989	1992	Difference
Treatment (1989: E_{89}=0, $E_{92\,RULES}$ =1) (1992: $E_{89RULES}$=0, E_{92} =1)	0.083 (0.013) [481]	0.234 (0.022) [380]	0.151 (0.024)
Control (1989: E_{89}=1, $E_{92\,RULES}$ =1) (1992: $E_{89RULES}$=1, E_{92} =1)	0.400 (0.013) [1394]	0.547 (0.014) [1236]	0.147 (0.019)
Difference-in differences			0.004 (0.037)

Private Insurance

	1989	1992	Difference
Treatment (1989: E_{89}=0, $E_{92\,RULES}$ =1) (1992: $E_{89RULES}$=0, E_{92} =1)	0.524 (0.023) [481]	0.505 (0.026) [380]	-0.018 (0.034)
Control (1989: E_{89}=1, $E_{92\,RULES}$ =1) (1992: $E_{89RULES}$=1, E_{92} =1)	0.277 (0.012) [1394]	0.273 (0.013) [1236]	-0.004 (0.017)
Difference-in differences			-0.014 (0.036)

No Health Insurance Coverage

	1989	1992	Difference
Treatment (1989: E_{89}=0, $E_{92\,RULES}$ =1) (1992: $E_{89RULES}$=0, E_{92} =1)	0.356 (0.022) [481]	0.197 (0.022) [380]	-0.159 (0.031)
Control (1989: E_{89}=1, $E_{92\,RULES}$ =1) (1992: $E_{89RULES}$=1, E_{92} =1)	0.295 (0.012) [1394]	0.152 (0.010) [1236]	-0.143 (0.016)
Difference-in differences			-0.016 (0.033)

Notes: Standard errors are in parentheses. Number of observations are in brackets. The sum of means for the treatments does not add to one since some individuals reported dual-coverage, and the other coverage is omitted.

Table 5

DD Estimates of the Effects of Medicaid Eligibility Expansions on Health Insurance Coverage for Children by Treatment and Control Groups in 1988 and 1991, Using NMIHS (Family Wage)

Medicaid

	1988	1991	Difference
Treatment	0.247	0.316	0.069
(1988: E_{88}=0, $E_{91\ RULES}$ =1) (1991: $E_{88RULES}$=0, E_{91} =1)	(0.014) [940]	(0.013) [1223]	(0.020)
Control	0.622	0.745	0.123
(1988: E_{88}=1, $E_{91\ RULES}$ =1) (1991: $E_{88RULES}$=1, E_{91} =1)	(0.009) [2995]	(0.009) [2256]	(0.013)
Difference-in differences			-0.054 (0.024)

Private Insurance

	1988	1991	Difference
Treatment	0.472	0.457	-0.015
(1988: E_{88}=0, $E_{91\ RULES}$ =1) (1991: $E_{88RULES}$=0, E_{91} =1)	(0.016) [940]	(0.014) [1223]	(0.022)
Control	0.169	0.149	-0.020
(1988: E_{88}=1, $E_{91\ RULES}$ =1) (1991: $E_{88RULES}$=1, E_{91} =1)	(0.007) [2995]	(0.007) [2256]	(0.010)
Difference-in differences			0.005 (0.021)

No Health Insurance Coverage

	1988	1991	Difference
Treatment	0.244	0.120	-0.124
(1988: E_{88}=0, $E_{91\ RULES}$ =1) (1991: $E_{88RULES}$=0, E_{91} =1)	(0.014) [940]	(0.009) [1223]	(0.016)
Control	0.177	0.064	-0.113
(1988: E_{88}=1, $E_{91\ RULES}$ =1) (1991: $E_{88RULES}$=1, E_{91} =1)	(0.007) [2995]	(0.005) [2256]	(0.009)
Difference-in differences			-0.011 (0.018)

Notes: Standard errors are in parentheses. Number of observations are in brackets. The sum of means for the treatments does not add to one since some individuals reported dual-coverage, and the other coverage is omitted.

Table 6

Linear Regression Estimates of the Effect of Demographic Characteristics and Streamlining Eligibility on Health Insurance Coverage Conditional on Being Eligible for Medicaid Under the 1992 Using the NLSY

	Medicaid	Private	Uninsured
Medicaid Eligibility	0.384**	-0.342**	-0.026
	(0.014)	(0.019)	(0.017)
Eligibility x Married	0.174**	-0.184**	0.022
	(0.018)	(0.023)	(0.020)
Eligibility x Separated	0.464**	-0.402**	-0.058*
	(0.023)	(0.023)	(0.022)
Eligibility x Never Married	0.555**	-0.470**	-0.053*
	(0.020)	(0.021)	(0.021)
F- test	197.584**	99.693**	9.189**
Eligibility x High School Dropout	0.466**	-0.444**	0.021
	(0.021)	(0.022)	(0.022)
Eligibility x High School Graduate	0.383**	-0.327**	-0.044*
	(0.018)	(0.021)	(0.019)
Eligibility x Some College	0.262**	-0.237**	-0.043
	(0.029)	(0.031)	(0.026)
Eligibility x B.A. and Above	0.093	0.039	-0.121**
	(0.053)	(0.062)	(0.041)
F- test	28.562**	38.354**	5.221**
Eligibility x Head of Household Working	0.172**	-0.225**	0.040*
	(0.016)	(0.022)	(0.020)
Eligibility x Head of Household Not Working	0.532**	-0.424**	-0.073**
	(0.016)	(0.020)	(0.018)
F- test	436.969**	119.775**	42.045**
N	3434	3434	3434

Notes: Standard errors are corrected using Huber's method. The coefficients of each set of demographic characteristics and state implemented rules are obtained with separate regressions. Except the first row, eligibility is entered in each set of regression as an interaction with the mutually exclusive events. State dummy variables are included in all regressions. F-tests are used to test equality of the coefficients. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$.

Table 6, Continued

Linear Regression Estimates of the Effect of Demographic Characteristics and Streamlining Eligibility on Health Insurance Coverage Conditional on Being Eligible for Medicaid Under the 1992 Using the NLSY

	Medicaid	Private	Uninsured
Eligibility x Residence in SMSA, in Central City	0.473**	-0.374**	-0.089**
	(0.030)	(0.028)	(0.027)
Eligibility x Residence in SMSA, not Central City	0.353**	-0.319**	-0.020
	(0.019)	(0.022)	(0.020)
Eligibility x Residence not in SMSA	0.359**	-0.305**	-0.027
	(0.025)	(0.028)	(0.025)
F- test	10.128**	2.584	3.234**
Eligibility x Drop Asset Tests	0.378**	-0.339**	-0.056**
	(0.015)	(0.019)	(0.017)
Eligibility x Retain Asset Tests	0.400**	-0.351**	0.071**
	(0.023)	(0.029)	(0.029)
F- test	1.198	0.177	19.307**
Eligibility x Outstationing of Eligibility Workers	0.408**	-0.354**	-0.062**
	(0.016)	(0.020)	(0.018)
Eligibility x No Outstationing of Eligibility Workers	0.346**	-0.321**	-0.034
	(0.018)	(0.022)	(0.021)
F- test	11.285**	2.662	24.103**
Eligibility x Shorthening of Application Forms	0.406**	-0.358**	-0.066**
	(0.017)	(0.021)	(0.019)
Eligibility x No Shorthening of Application Forms	0.358**	-0.321**	0.025
	(0.018)	(0.022)	(0.021)
F- test	6.968**	2.522	16.880**
N	3434	3434	3434

Notes: Standard errors are corrected using Huber's method. The coefficients of each set of demographic characteristics and state implemented rules are obtained with separate regressions. Except the first row, eligibility is entered in each set of regression as an interaction with the mutually exclusive events. State dummy variables are included in all regressions. F-tests are used to test equality of the coefficients. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$.

Table 7

Linear Regression Estimates of the Effect of Demographic Characteristics and Streamlining Eligibility on Health Insurance Coverage Conditional on Being Eligible for Medicaid Under the 1991 Using NMIHS

	Medicaid	Private	Uninsured
Medicaid Eligibility	0.377**	-0.338**	-0.048**
	(0.017)	(0.018)	(0.012)
Eligibility x Married	0.189**	-0.157**	-0.022
	(0.022)	(0.024)	(0.015)
Eligibility x Separated	0.398**	-0.372**	-0.041*
	(0.028)	(0.025)	(0.018)
Eligibility x Never Married	0.457**	-0.412**	-0.065**
	(0.018)	(0.018)	(0.012)
F- test	93.715**	105.282**	6.901**
Eligibility x High School Dropout	0.446**	-0.420**	-0.043**
	(0.019)	(0.018)	(0.013)
Eligibility x High School Graduate	0.372**	-0.323**	-0.058**
	(0.019)	(0.020)	(0.013)
Eligibility x Some College	0.272**	-0.226**	-0.047**
	(0.027)	(0.027)	(0.016)
Eligibility x B.A. and Above	0.058	-0.010	0.026
	(0.048)	(0.052)	(0.037)
F- test	33.402**	48.994**	3.104*
Eligibility x Head of Household Working	0.166**	-0.131**	-0.027*
	(0.022)	(0.022)	(0.014)
Eligibility x Head of Household Not Working	0.490**	-0.446**	-0.066**
	(0.018)	(0.018)	(0.012)
F- test	340.157**	396.549**	12.895**
N	3935	3935	3935

Notes: Standard errors are corrected using Huber's method. Each set of demographic characteristics and state implemented rules are estimated with separate regressions. Medicaid eligibility is calculated using family wage income, federal poverty level, and state-established Medicaid income thresholds. State dummy variables are included in all regressions. F-tests are used to test equality of the coefficients. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$.

Table 7, Continued

Linear Regression Estimates of the Effect of Demographic Characteristics and Streamlining Eligibility on Health Insurance Coverage Conditional on Being Eligible for Medicaid Under the 1991 Using NMIHS

	Medicaid	Private	Uninsured
Eligibility x Drop Asset Tests	0.366**	-0.315**	-0.044**
	(0.024)	(0.025)	(0.016)
Eligibility x Retain Asset Tests	0.382**	-0.348**	-0.049**
	(0.019)	(0.019)	(0.013)
F- test	0.372	1.801	0.123
Eligibility x Outstationing of Eligibility Workers	0.407**	-0.308**	-0.056**
	(0.035)	(0.036)	(0.021)
Eligibility x Not Outstationing of Eligibility Workers	0.369**	-0.345**	-0.045**
	(0.018)	(0.019)	(0.013)
F- test	1.012	1.126	0.209
Eligibility x Shorthening Application Forms	0.322**	-0.320**	-0.021
	(0.041)	(0.040)	(0.025)
Eligibility x No Shorthening of Application Forms	0.384**	-0.340**	-0.051**
	(0.017)	(0.018)	(0.012)
F- test	2.185	0.275	1.409
N	3935	3935	3935

Notes: Standard errors are corrected using Huber's method. Each set of demographic characteristics and state implemented rules are estimated with separate regressions. Medicaid eligibility is calculated using family wage income, federal poverty level, and state-established Medicaid income thresholds. State dummy variables are included in all regressions. F-tests are used to test equality of the coefficients. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$.

Table A1

Summary Statistics for 1989 and 1992 Using NLSY

	1989	1992
Medicaid Eligibility	0.279	0.333
	(0.45)	(0.47)
Medicaid Participation	0.123	0.169
	(0.33)	(0.37)
Private Health Insurance	0.652	0.685
	(0.48)	(0.46)
Uninsured	0.194	0.112
	(0.40)	(0.31)
Other Health Insurance	0.039	0.051
	(0.19)	(0.22)
Family Income ($1000)	42.940	53.718
	(126.30)	(127.82)
Family Wage ($1000)	25.504	45.112
	(43.93)	(255.12)
Family Wage (Current) ($1000)	25.685	32.228
	(34.31)	(54.97)
Black	0.257	0.286
	(0.44)	(0.45)
Married	0.718	0.720
	(0.45)	(0.45)
Divorced-Separated	0.139	0.150
	(0.35)	(0.36)
Never Married	0.142	0.130
	(0.35)	(0.34)
Respondent Working	0.630	0.662
	(0.48)	(0.47)
Spouse Working	0.513	0.515
	(0.50)	(0.50)
High School Dropout	0.211	0.164
	(0.41)	(0.37)
High School	0.496	0.477
	(0.50)	(0.50)
Some College	0.187	0.223
	(0.39)	(0.42)
BA Plus	0.106	0.136
	(0.31)	(0.34)
Age of the Youngest Child	3.452	4.477
	(3.25)	(3.92)
Family Size	3.917	4.003
	(1.32)	(1.30)
N	5452	5272

Notes: Standard deviations are in parentheses.

Table A2

Summary Statistics for 1989 and 1992 for the Eligible Population Using NLSY

	1989	1992
Medicaid Participation	0.400 (0.49)	0.473 (0.50)
Private Health Insurance	0.277 (0.45)	0.327 (0.47)
Uninsured	0.295 (0.46)	0.163 (0.37)
Other Health Insurance	0.044 (0.21)	0.066 (0.25)
Family Income ($1000)	17.717 (75.03)	21.739 (73.72)
Family Wage ($1000)	3.759 (5.21)	6.511 (7.23)
Family Wage (Current) ($1000)	5.054 (8.19)	7.848 (12.45)
Black	0.382 (0.49)	0.420 (0.49)
Married	0.394 (0.49)	0.421 (0.49)
Divorced-Separated	0.277 (0.45)	0.281 (0.45)
Never Married	0.329 (0.47)	0.297 (0.46)
Respondent Working	0.396 (0.49)	0.474 (0.50)
Spouse Working	0.290 (0.45)	0.307 (0.46)
High School Dropout	0.346 (0.48)	0.285 (0.45)
High School	0.487 (0.50)	0.502 (0.50)
Some College	0.139 (0.35)	0.174 (0.38)
BA Plus	0.027 (0.16)	0.039 (0.19)
Age of the Youngest Child	3.200 (3.08)	4.416 (3.59)
Family Size	4.248 (1.80)	4.339 (1.71)
N	1394	1630

Notes: Standard deviations are in parentheses.

Table A3

Summary Statistics for 1988 and 1991 Using NMIHS

	1988	1991
Medicaid Eligibility	0.424 (0.49)	0.556 (0.50)
Medicaid Participation	0.327 (0.47)	0.339 (0.48)
Private Health Insurance	0.471 (0.50)	0.492 (0.50)
Uninsured	0.173 (0.38)	0.079 (0.27)
Other Health Insurance	0.042 (0.20)	0.081 (0.28)
Family Income ($1000)	22.757 (20.89)	27.488 (27.89)
Family Wage ($1000)	20.427 (27.32)	22.397 (24.67)
Black	0.494 (0.50)	0.480 (0.50)
Married	0.558 (0.50)	0.581 (0.50)
Divorced-Separated	0.088 (0.28)	0.122 (0.33)
Never Married	0.339 (0.47)	0.285 (0.46)
Respondent Working	0.367 (0.48)	0.600 (0.50)
Spouse Working	0.553 (0.50)	0.521 (0.50)
High School Dropout	0.193 (0.39)	0.171 (0.38)
High School	0.404 (0.49)	0.387 (0.49)
Some College	0.230 (0.42)	0.264 (0.45)
BA Plus	0.134 (0.34)	0.152 (0.36)
Age of the Youngest Child	0.000 (0.00)	2.999 (0.46)
Family Size	4.566 (1.89)	4.219 (1.51)
N	9953	8145

Notes: Standard deviations are in parentheses.

Table A4

Summary Statistics for 1988 and 1991 for the Eligible Population Using NMIHS

	1988	1991
Medicaid Participation	0.622 (0.49)	0.595 (0.50)
Private Health Insurance	0.168 (0.37)	0.258 (0.44)
Uninsured	0.180 (0.38)	0.084 (0.28)
Other Health Insurance	0.044 (0.20)	0.092 (0.29)
Family Income ($1000)	9.303 (10.46)	13.936 (14.03)
Family Wage ($1000)	2.546 (4.29)	6.036 (7.26)
Black	0.695 (0.46)	0.629 (0.49)
Married	0.279 (0.45)	0.372 (0.49)
Divorced-Separated	0.127 (0.33)	0.172 (0.38)
Never Married	0.580 (0.49)	0.445 (0.50)
Respondent Working	0.182 (0.39)	0.431 (0.50)
Spouse Working	0.239 (0.43)	0.293 (0.46)
High School Dropout	0.339 (0.47)	0.275 (0.45)
High School	0.407 (0.49)	0.429 (0.50)
Some College	0.153 (0.36)	0.204 (0.41)
BA Plus	0.032 (0.18)	0.051 (0.22)
Age of the Youngest Child	0.000 (0.00)	3.042 (0.47)
Family Size	5.117 (2.27)	4.347 (1.62)
N	2592	3821

Notes: Standard deviations are in parentheses.

Essay II

Does Medicaid Improve Infant and Child Health?

A. Introduction

According to a variety of infant and child health indicators, the U.S. lags behind other industrialized countries. The U.S. has higher rates of low birth weight and infant mortality, and lower immunization rates. Moreover, children in the U.S. have a greater likelihood of being affected by chronic and disabling conditions.

It is widely believed that a major reason for the inferior state of infant and child health in the U.S. is limited access to medical care. In an era of rising health care costs, limited access often takes the form of lack of health care insurance or insufficient health insurance coverage. This is particularly true for low-income families. According to this view, if more low-income families were eligible for health insurance coverage, they would avail themselves of these opportunities, use more health care services and consequently enjoy improved health status. Yet the link between eligibility for health insurance coverage and improved health status is not straightforward. Expanded eligibility does not necessarily increase enrollment as families may not take advantage of the new opportunities for coverage. Enrollment itself may or may not lead to increased and appropriate utilization of services if families cannot locate providers. Finally, the connection between service utilization and health status is ambiguous and depends on the effectiveness of the care received.

Beginning in the mid-1980s, Congress attempted to increase access to health care for children and pregnant women through a series of expansions in Medicaid eligibility. These expansions in eligibility and associated improvements in the enrollment process produced a sharp rise in the number of children covered by Medicaid. According to the Congressional Research Service (1993), the number of poor children under seven enrolled in Medicaid increased from 3.6 to 4.8 million between 1988 to 1991. In addition, the proportion of all births financed by

Medicaid rose from 14.5 percent in 1985 to 32 percent by 1991 (Frost et al. 1993).

Overall, the expansions in Medicaid eligibility for children constitute the largest expansion of public health insurance coverage for children since the original introduction of the Medicaid program. Yet, except for a handful of narrowly focused studies on the effect of Medicaid **eligibility** on **birth** outcomes, relatively little is known about Medicaid's effectiveness in improving the health of infants and children from poor and near-poor families. Furthermore, findings from past studies of the effect of Medicaid on infant health are diverse, and as a result, do not provide sufficient information to guide current and future policy decisions in this area. Therefore, it appears that the large amount of resources spent on the Medicaid program has been motivated primarily by the perceived effectiveness of the program, and not by empirical evidence related to health outcomes.[1]

This study is an attempt to fill the gap in our knowledge about the effectiveness of Medicaid in improving the health of infants and children. In this analysis, we examine the effect of Medicaid **participation** on both **infant** and **child** health. Virtually all previous studies have focused only on Medicaid eligibility, and consequently produce estimates of the effect of Medicaid on child health that are confounded by decisions to participate in the program. In addition, our study is the first to examine the effect of Medicaid participation on health outcomes of children over age one. There is growing evidence that medical care (e.g., prenatal care) has a relatively minor effect on determining birth outcomes since the clinical scope of such an intervention is limited. Thus, restricting the analysis to birth outcomes, as has been the case in most past studies, may provide a misleading view of the effect of Medicaid on child health. In this study, we examine health outcomes of both infants and children. Child outcomes include nutritional status (i.e., standardized height and weight), activity limitations, presence of chronic

[1] There is a large literature that examines the effect of Medicaid on child health care utilization. In most studies, Medicaid is found to increase utilization relative to the uninsured. See the studies by Long and Marquis (1996), Currie and Thomas (1995) and St. Peter et al. (1992) for more on the effect of Medicaid on health care utilization.

and acute conditions, and maternal assessments of child health. A final contribution of our study is that we focus on poor and near-poor infants and children, the target population of the Medicaid program. In most past studies, researchers have used the entire population of infants and children to examine the effect of Medicaid, even though only a fraction of the population is affected by the program.

B. Review of Previous Research

Several recent papers have examined the effect of the Medicaid program on infant and child health. The earliest study was by Grossman and Jacobowitz (1981), who examined the effect of Medicaid statutes and eligibility rules on neonatal mortality using county-level data for the year 1971. These authors found that Medicaid eligibility rules had no effect on infant mortality. In a similar study that used county-level data from 1977, Corman and Grossman (1985) also found that the Medicaid eligibility had little effect on infant mortality. One important feature of both of these studies is the specification of the empirical model. In both papers, a variable measuring the proportion of women that were in poverty was included in the model, and Medicaid policy variables were interacted with the poverty measure. The importance of this specification can be illustrated with reference to a simple model taken from Grossman and Jacobowitz (1981). First, specify the total infant mortality rate (D_T) as a weighted average of the infant mortality rate of poor (D_p) and non-poor women (D_n):

(1) $$D_T = kD_p + (1-k)D_n ,$$

where k is the proportion of births to poor women. Medicaid only affects poor women since non-poor women are not eligible. Thus, the infant mortality rate of poor and non-poor women may be written as

(2) $$D_p = \alpha_0 + \alpha_1 M ,$$

(3) $$D_n = \beta_0 ,$$

where M is the proportion of poor women who participate in Medicaid. Substituting Equations (2) and (3) into (1) yields a relationship between total infant mortality and Medicaid:

(4) $$D_T = \beta_0 + (\alpha_0 - \beta_0)k + \alpha_1 kM .$$

Equation (4) illustrates the point that it is necessary to interact Medicaid variables with the fraction of births to poor women, and to include the fraction of births to poor women in the model, in order to obtain an estimate of the effect of Medicaid on the affected population. This specification yields an estimate of the effect of Medicaid policy variables on the infant mortality rate of women affected by Medicaid even though the dependent variable is aggregate infant mortality.

More recently, Currie and Gruber (1996a), examined the effect of Medicaid eligibility on child health status using individual level data from the National Health Interview Surveys from 1984 to 1992. The authors found that being eligible for Medicaid had no effect on a mother's evaluation of her child's health (e.g., activity limitations). In the same paper, however, Currie and Gruber (1996a) examined the effect of Medicaid eligibility on child (ages 1-4 and 5-14) mortality using state-level data from vital statistics. In contrast to their other finding, they found that Medicaid eligibility reduces child mortality. It is noteworthy that Currie and Gruber (1996a) found a significant effect in the aggregate data, but not the individual-level data since Medicaid affects only a fraction of the aggregate population.[2] Random variation in aggregate child mortality will tend to obscure the effect of changes in Medicaid eligibility that affect only a small portion of the aggregate population. In individual-level data, however, the empirical relationship is more direct since both eligibility and child health are measured at the individual level. Furthermore, in contrast to the papers by Grossman and his colleagues, Currie and Gruber (1996a) do not include the appropriate poverty measure in their analysis that uses aggregate data.

[2] The authors imply that the differences in results are due to the fact that the micro-level data contains only subjective measures of child health.

Thus, their empirical specification does not yield an estimate of the effect of Medicaid on child mortality of families affected by Medicaid.

In a related study, Currie and Gruber (1996b) used state-aggregate data to examine the effect of Medicaid eligibility on birth weight and infant mortality. The authors found that broad expansions in Medicaid that occurred in the latter half of the 1980s had no effect on infant health, but that earlier expansions that were targeted at small groups of poor women not receiving AFDC did reduce infant mortality and the incidence of low birth weight. The pattern of their findings is unexpected since the targeted expansions affected a smaller percentage of all families than did the broad expansions, and the study examines total infant mortality. Currie and Gruber (1996b) do not include the appropriate poverty measure that would yield estimates of the effect of Medicaid on the affected population as opposed to the total population. Finally, the magnitude of the estimates in this paper is implausibly large. For example, estimates related to infant mortality imply that the number of infant deaths declined by 80 percent among women who were made eligible and who participated in the expanded Medicaid program.[3]

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[3] Aggregate infant mortality may be written as $\frac{D}{B}=\frac{D_e}{B}+\frac{D_i}{B}+\frac{D_n}{B}$, where D is the number of infant deaths, B is the number of births in thousands, subscript e indicates that mother of child is poor and eligible for Medicaid, subscript i indicates mother of child is poor and currently ineligible for Medicaid, and subscript n indicates mother of child is not poor and ineligible for Medicaid. Expanding Medicaid coverage to poor and ineligible women will change infant mortality as follows: $\frac{\partial(D/B)}{\partial E}=\frac{\partial(D_i/B)}{\partial E}$. Currie and Gruber (1996b) report the following regression estimate (p. 1277, Table 3, column 4): $\frac{\partial(D/B)}{\partial E}=\frac{\partial(D_i/B)}{\partial E}=-3.031$. Assume that $\partial E=.30$ and $B=3612$ (1980 figure), and solve for ∂D_i. This yields a change in the number of infant deaths to poor mothers who were previously ineligible for Medicaid of 3283 in response to a 30 percentage point increase in Medicaid eligibility. Assume that $D=45526$ (1980 figure), and that $\frac{D_i}{D}=.3$, and thus $D_i=13658$. Therefore, a 30 percentage point increase in Medicaid eligibility lowered the number of infant deaths among poor women previously ineligible for Medicaid by 24 percent (3283/13658). Assume that one-third of the women made eligible for the program actually participated, and that they accounted for one-third (4097) of infant deaths among the poor and previously ineligible group. Thus, the number of infant deaths had to fall by **80 percent** for women who became eligible for Medicaid and who participated in the program.

Two other studies used national samples to examine the impact of Medicaid eligibility on infant health. Cole (1995) used state-aggregate data to study the effects of Medicaid eligibility on birth weight, but unlike Currie and Gruber (1996b), allowed the effect of Medicaid eligibility to differ by demographic characteristics correlated with Medicaid participation (e.g., marital status). The results from this study indicated that the recent broad eligibility expansions decreased the rate of low birth weight by between two and three percent for groups most likely to be affected by changes in eligibility (e.g., unmarried). Kenney and Dubay (1995) used county-level data for the period 1986 to 1990 to estimate the impact of Medicaid statutes and eligibility rules on low birth weight. Similar to Grossman and his colleagues, Kenney and Dubay (1995) include the relevant poverty measure in the model and found that Medicaid eligibility rules had no effect on birth weight.

In addition to studies that use national data, several other studies have examined the effect of Medicaid on infant health in a particular state. Piper, Mitchel and Ray (1994) used individual-level data and a before-and-after analysis to examine the impact of Medicaid eligibility on birth weight in Tennessee. The authors found that expanding Medicaid income eligibility from 45 to 100 percent of poverty had no effect on birth weight. Using a similar methodology, Haas, Udvarhelyi, Morris and Epstein (1993) examined the effect of expanded Medicaid eligibility (i.e., Healthy Start Program) for low-income pregnant women in Massachusetts, and found insignificant effects of the expansion on infant health outcomes. Finally, Long and Marquis (1995) studied the effect of Medicaid participation on birth outcomes in Florida. These authors found that Medicaid participation had no significant effect on birth outcomes.

In summary, past estimates of the effect of the Medicaid program on child health are mixed, and for this reason additional study of the issue is warranted. Furthermore, our brief review of the literature has documented three deficiencies in past studies. First, there is only one prior study that has examined the effect of Medicaid **participation**, as opposed to Medicaid eligibility, on infant and child health, and past studies' focus on eligibility is problematic for two reasons. As several recent analyses have demonstrated, many women and children eligible for Medicaid decline to participate.[4] Thus, examining the effect of Medicaid eligibility on infant and child health results in a downward biased estimate of the effect of Medicaid **participation** on infant and child health. In addition, Medicaid eligibility has sometimes been measured with substantial error. For example, Currie and Gruber (1996a) assign eligibility based on reported annual income in the past year, but actual Medicaid eligibility is determined based on a family's current monthly income. As a result, eligibility is often incorrectly assigned. Yazici (1996) shows that between 20 and 25 percent of a sample of low income women drawn from either the National Maternal and Infant Health Survey (NMIHS) or the Current Population Survey (CPS) report that they are covered by Medicaid even though their annual income is greater than the eligibility threshold. Other studies use even cruder measures of Medicaid program participation. In Haas, Udvarhelyi, Morris and Epstein (1993), birth outcomes of uninsured women are compared to those of women with private insurance. These authors argue that being uninsured is a proxy for Medicaid (i.e., Healthy Start) eligibility. Similarly, Kenney and Dubay (1995), Grossman and Jacobowitz (1981), and Corman and Grossman (1985) use Medicaid rules as proxy measures for eligibility or participation. In contrast to these past studies, we examine the effect of Medicaid **participation** on infant and child health outcomes. This strategy allows us to directly measure the impact of Medicaid and avoids the measurement error problem that has plagued some previous studies.

A second problem with past studies is that they frequently examine the effect of the Medicaid program using aggregate data on **all** infants and children (e.g., Currie and Gruber 1996a, 1996b; Kenney and Dubay 1995; Corman and Grossman 1985). As Cole (1995) notes, this strategy makes it difficult to identify any effect of Medicaid since only a fraction of the total population is eligible for Medicaid, and only a portion of the eligible population actually

[4] See Currie and Gruber (1996a), Cutler and Gruber (1996), Dubay and Kenney (1996a), Shore-Sheppard (1996a) and Yazici (1996) for evidence on the take-up rate for Medicaid.

participate in the program. Studies such as those by Grossman and his colleagues, attempt to overcome this problem by including a measure of the fraction in poverty in the model, but are nevertheless unsatisfactory because of the empirical difficulties associated with estimating the correctly specified model.[5] In this study, we use individual-level data and restrict the sample to infants and children from families headed by unmarried women, age 19 or older who have 12 or less years of education. This sample is characterized by high rates of poverty and Medicaid participation, and it represents a significant portion of the target population of recent changes in Medicaid policy.

Finally, there has been only one past study of the effect of Medicaid on health outcomes of children over age one, and that study examined the effect of Medicaid eligibility on child mortality using a sample of all families (Currie and Gruber 1996a). Thus, there is limited information about the effectiveness of the Medicaid program in improving the health of older children. Studies that focus on birth outcomes may not be relevant to child outcomes because of the relatively limited ability of medical interventions (i.e., prenatal care) to affect birth outcomes (Huntington and Connell 1994; Guyer 1990). In this study, we examine health outcomes of both infants and children. Child outcomes will include nutritional status (i.e., standardized height and weight), activity limitations, presence of chronic and acute conditions, and maternal assessments of child health.

C. Research Design and Methods

1. Theoretical Model

Our analytic framework is motivated by Grossman's (1972) model of the demand for health. The key feature of the Grossman (1972) model is the recognition that health is a commodity that cannot be purchased, but is instead self-produced using a combination of market

[5] See Corman and Grossman (1985) for a further discussion of this point.

inputs such as medical care, and own time. Thus, embedded in this model is a health production function that describes the relationship between health inputs and health output (i.e., health status). The Grossman (1972) model can be applied to an analysis of child health by noting that child health outcomes are largely determined by the decisions of the family. In this case, we specify parental utility in period t as a function of child health (H) and other consumption (C):

(5) $$U_t = f(H_t, C_t).$$

Child health at age k and time t is a function of past investments in medical care (M), other market inputs (Y), and parental time (L):

(6) $$H_t = g(M_t, M_{t-1}, \ldots, M_{t-k}, Y_t, Y_{t-1}, \ldots, Y_{t-k}, L_t, L_{t-1}, \ldots, L_{t-k} : \varepsilon, \upsilon).$$

Note that in Equation (6), child health depends on the cumulative amount of inputs as well as the child's health endowment (ε), and a production efficiency parameter (υ). The demand function for child health that results from this model has the following general form:

(7) $$H_t = g'(p_t, p_{t-1}, \ldots, p_{t-k}, w_t, w_{t-1}, \ldots, w_{t-k}, I_t, I_{t-1}, \ldots, I_{t-k} : \varepsilon, \upsilon, \theta),$$

where p is the price of child health care services, w is the price of parental time input, I is family income, ε is the child's health endowment, υ is an efficiency parameter associated with the child health production function, and θ is a taste parameter. Our empirical analysis focuses on estimating Equation (7), or the parameters of the demand function for child health. The distinguishing aspect of the analysis of child health outcomes, as opposed to adult health, is the prominent role that the family plays in determining child health.

Medicaid affects the demand for child health through its effect on the price of health care. Medicaid virtually eliminates out-of-pocket costs of care and insurance premium payments, but may increase time costs of care since access to providers may become limited.[6] Thus, the net effect of Medicaid on the price of health care depends on which component of price is most

[6] Medicaid can decrease insurance costs even if insurance is received through employment. Implicit in this argument is that there is shifting of insurance costs to wages.

affected. For most families that are eligible for Medicaid, it is likely that the elimination of out-of-pocket and insurance costs dominates, implying a lower price of health care. We assume that a decrease in the price of health care will improve child health because children will receive more health care services. Accordingly, our null hypothesis is that Medicaid participation, relative to being uninsured, improves the health of children.

2. Empirical Model

Based on the theoretical considerations outlined in the previous section, we specify the following linear-in-parameters demand function for child health:

$$(8) \qquad H_{it} = \alpha_i + \delta_1 f_1(\sum_0^t MC_{it}) + \delta_2 f_2(\sum_0^t P_{it}) + \delta_3 f_3(\sum_0^t I_{it}) + \sum_{m=4}^{M} \delta_m f_m(\sum_0^t X_{itm}) + \varepsilon_{it} \quad ,$$

where H is a measure of child health status, MC is a dummy variable indicating that the child is on Medicaid, P is a dummy variable indicating that the child has private insurance, I is a measure of family income, and i and t index individuals and time respectively. X_k is a vector of other independent variables including race, sex and age of child, mother's education, mother's marital status, and mother's health status. These independent variables are included to control for differences in the price of parental time, health production efficiency and family preferences. Note that unobserved individual effects (e.g., endowment differences) are identified by the inclusion of individual (α_i) specific intercepts in the model.

The specification of Equation (8) is intended to illustrate the point that child health in period t is a function of the entire history of prices, incomes and other factors. That is why we have included lagged values of insurance status and income (e.g., $f_1(\sum_0^t MC_{it})$).[7] Investments in child health are made throughout the child's life and as prices of child health-production inputs

[7] The model could be made more general by allowing parameters to vary over time.

change in real terms, parents alter the mix of inputs as well as the level of child health demanded.

3. Econometric Issues

a) Mis-specification of the Demand or Production Function

In most empirical applications, current child health is specified as a function only of current variables. As Equation (8) illustrates, this strategy could lead to seriously biased estimates if the effects of past child health-production inputs are important and prices or circumstances have changed in real terms. For example, relating current child health to current insurance status may not be appropriate if health care does not have an immediate effect on child health. It is easy to imagine a situation where a previously uninsured child, who currently receives Medicaid, may be in poor health not because of a lack of current care, but because the child's health had deteriorated during the period they were uninsured.

One solution to this problem that we pursue is to examine infant health outcomes since the relevant history is short (i.e., gestation). This may be one reason most previous studies have been limited to the analysis of birth weight and infant mortality. A second solution is to use longitudinal data and include the entire history of prices (i.e., insurance status) and income. In this study, we use data from the National Longitudinal Survey of Youth (NLSY) which contains the longitudinal information necessary to estimate a modified version of Equation (8).

b) Selection Bias

A second statistical problem associated with Equation (8) is selection bias. Estimates of the effect of Medicaid on health status may be biased if unobserved factors that affect Medicaid participation also affect health status. A similar caveat applies to participating in a private health insurance plan. Families choose a type of insurance plan based on a variety of factors such as income, the level of risk aversion, and the child's health endowment. These same factors may also determine the child's health status. Researchers, however, only have access to a limited number of observable characteristics. Thus, any attempt to compare health status among children who differ by type of health care insurance, must address this "selection" issue in order to separate the effect of insurance from the effect of unobservable factors.

We use three different strategies to address the selection problem. Each of the approaches we use exploits a different aspect of our data and is based on different underlying assumptions.

(1) Selection on Observable Characteristics

Our first strategy is primarily heuristic and we use it mainly to provide insight into the magnitude of the selection problem. The basic idea of this approach is to use the available information in our data to 1) minimize the extent of the selection problem, and 2) to gauge the magnitude of the selection problem. Toward achieving the first goal, we limit the analysis sample to infants and children of unmarried women, age 19 or older, who have 12 or less years of education. This selection criteria will tend to reduce the heterogeneity in the sample and narrows the sample to predominantly low-income families who are a primary target of the Medicaid program. In addition, we estimate Equation (8) on two separate sub-samples based on insurance status: infants and children with Medicaid or private insurance, and infants and children who are uninsured or who have Medicaid.[8] The separation of the sample according to insurance status further reduces the heterogeneity in the samples and facilitates the use of an instrumental variables procedure since there is only one endogenous right hand side variable.

The second goal of this approach, gauging the magnitude of the selection problem, is achieved by estimating two different versions of Equation (8): one that includes a limited number of right hand side variables, and a second that includes a larger set of explanatory variables. Insight into the magnitude of the selection problem may be obtained by examining the changes in

[8] We delete from the sample the small number (2.5-3.5%) of families covered by other types of public insurance such as CHAMPUS.

the estimated effect of Medicaid participation as the number of observable characteristics included in the analysis are increased. If the inclusion of additional observable variables that significantly affect child health outcomes has little effect on the estimate of the effect of Medicaid participation on child health, it is reasonable to conclude that the selection problem may not be of practical significance. The validity of this procedure depends on the strength of the correlation between observed and unobserved characteristics, and the unobserved characteristics and child health status. If these correlations are strong, then this procedure is an effective way to identify and control for selection bias. On the other hand, if these correlations are weak, it suggests that the correlation between unobserved factors and insurance status may be insignificant.[9]

(2) Instrumental Variables

The fundamental statistical problem associated with estimating Equation (8) is that Medicaid participation may be correlated with the error. Appropriate sample selection, and the inclusion of a large set of control variables may or may not eliminate this correlation. Furthermore, there is no definitive way to test the validity of this strategy. An alternative solution is to use an instrumental variables (IV) procedure. The IV solution involves finding an instrument that is correlated with Medicaid participation, but uncorrelated with infant and child health outcomes. One potential set of instruments are state dummy variables. States differed in the timing, magnitude and implementation (e.g., presumed eligibility) of their expansions of the Medicaid program, and this state variation may be used to predict Medicaid participation.[10]

The validity of the IV procedure depends on whether state dummy variables affect infant and child health outcomes independently of Medicaid participation status and other variables

[9] Another possibility is that the correlation between the observed variables added to the model and health status is weak.

[10] Another instrument that we use is labor market characteristics such as the local unemployment rate. Since private health insurance is primarily offered through employment, areas with better employment opportunities will have higher rates of participation in private insurance plans.

included in Equation (8). One aspect of our study that makes the assumption underlying the use of state dummy variables as instruments reasonable is the relatively homogenous sample we use in the analysis. There will be less unmeasured state heterogeneity in a sample of families from similar socioeconomic backgrounds than in a sample with a broader range of families. Furthermore, unmeasured state factors are most likely to affect child health because of differences in the provision of publicly financed care (i.e., Medicaid). Our empirical model, however, controls for Medicaid participation, and state differences in the quality or accessibility of care will be reflected in differences in state participation rates.

In analyses that use one year of cross-sectional data, we will not be able to adequately test whether state dummy variables are valid instruments since the empirical model will include just one additional instrument. When we pool cross-sectional data, however, we can include state and year dummy variables in the model, and use state-year interactions as instruments.[11] The problem with this latter procedure is that state-year interactions may not have enough explanatory power to make them useful instruments (Bound et al. 1995). We plan to test this proposition by examining the significance of the excluded instruments in the first-stage regressions.

(3) Fixed Effect Estimates

Our last approach to the selection bias problem is to estimate fixed-effects models. As previously noted, we use data from the National Longitudinal Survey of Youth, which has longitudinal information on child health outcomes and other variables of interest. An important feature of longitudinal data is that it enables the researcher to control for time-invariant unmeasured characteristics that affect the outcome of interest. Therefore, a fixed-effects estimator may be a solution to the selection problem if unobserved characteristics that affect both participation in Medicaid and child health outcomes are time invariant. To implement the fixed

[11] We also exploit variation in state laws by age of child by including state-age and year-age interactions as instruments.

effects model, we estimate Equation (8) using data on individual children in the NLSY. We transform the data into deviations around child specific means to control for time-invariant unobserved effects.

c) Unobserved Health Endowment

Another statistical problem relates to the presence of the child's health endowment in the demand function (Equation (8)). This unobserved factor may lead to biased estimates of Equation (8) since the endowment may be correlated with other right hand side variables such as Medicaid participation. To address this issue, we use proxy measures such as child's birth weight and mother's health status to control for the effect of the child's health endowment. In addition, analyses that use longitudinal data and a fixed-effects estimation procedure control for the effect of time-invariant characteristics such as the child's unobserved health endowment.

D. Data

We use three different data sources to carry out our analysis since there is no single data set that is ideal for our purposes. Each of our data sources has a particular advantage that we exploit. Using all three data sets we are able to address several of the statistical issues raised above, and answer all of the questions of interest.

1. The 1988 National Maternal and Infant Health Survey

We begin our analysis by examining the effect of Medicaid on infant health. In particular we focus on birth weight and the incidence of low birth weight. For this analysis we use the live birth file of the 1988 National Maternal and Infant Health Survey (NMIHS). The NMIHS is a national sample of live births in which vital records (i.e., birth certificates) are linked with maternal questionnaires. The purpose of the NMIHS was to study factors related to poor pregnancy outcomes.[12] To this end, the maternal questionnaires provide supplementary information about family demographic characteristics and maternal behaviors such as receipt of prenatal care, use of tobacco, alcohol and drugs, and labor market experiences. The NMIHS is an excellent source of data to study infant health outcomes because of the high quality of the health measures and the extensive social and demographic information. In total, there were 9,440 observations that had information from both vital statistics records and from maternal questionnaires. Information on birth weight comes from vital statistics while data on insurance status and other characteristics comes from the maternal questionnaire.[13]

We limit the sample to infants of unmarried women, age 19 or older who have 12 or less years of education. These sample restrictions reduce the heterogeneity in the sample and result in a sample of infants from predominantly poor and near-poor families with high rates of Medicaid participation. This group of families is a primary target of the Medicaid program. Descriptive statistics for the sample are contained in the appendix.

2. National Health Interview Survey

In order to study the effect of Medicaid on child health outcomes we use the 1989 and 1992 National Health Interview Surveys (NHIS). The NHIS contains an extensive set of self-reported health measures and is a large national sample. In 1989 and 1992, the NHIS included a supplement on health insurance. Thus, for these two years, we can link information about child health to data on health insurance status. In addition to information about health and health

[12] The NMIHS is not a simple random sample. Low- and very-low birth weight infants are over sampled as are black families. The consequences of the NMIHS sample design for empirical analyses have been investigated by Korn and Graubard (1995). Following their conclusions, all models are estimated using weighted least squares.

[13] For approximately 5 percent of the sample, dual insurance coverage was indicated. For these cases, we assigned coverage based on the following hierarchy: if private insurance was one of the categories, we assigned that person to private insurance; and if Medicaid and other insurance were indicated, we assigned that person to Medicaid. We followed this assignment strategy for all three data sets used in the analysis.

insurance, the NHIS contains data on social and demographic characteristics of families. We limit the analysis to children between the ages of 2 and 12 who come from families headed by unmarried women, age 19 or older who have 12 or less years of education. Descriptive statistics for this sample are presented in the appendix.

It is fortuitous that the NHIS included health insurance supplements in the years 1989 and 1992. During this period, the Medicaid program was greatly expanded to cover higher income families and older children. This significant variation across states and over time in Medicaid program eligibility will enhance the efficacy of the instrumental variables procedure in which state-year interactions are used as instruments to predict Medicaid participation.

A drawback of the NHIS data is that all health measures are maternal self-reports. These measures include the mother's rating (excellent-poor) of her child's health, whether the child has any activity limitations, the number of general and acute conditions that result in activity limitation or bed days, and the number of restricted activity or bed days. Maternal self reports may be poor measures of actual child health that may decrease the precision of our parameter estimates, and under some conditions may result in biased estimates. In preliminary analyses, however, all of the child health measures in the NHIS were significantly correlated with the number of physician visits. This suggests that maternal reports of child health do in fact measure actual child health. In addition, all empirical analyses control for a family income and a variety of maternal characteristics that may be correlated with any measurement error in maternal reports of child health. Consequently, our estimates of the effect of Medicaid participation on child health may be relatively unaffected by measurement error.

3. The National Longitudinal Survey of Youth

The third data set we use is the National Longitudinal Survey of Youth (NLSY) which is a national probability sample of young adults who were between the ages of 14 and 21 in 1979 (Center for Human Resources 1994). The respondents have been interviewed on a yearly basis since 1979. The NLSY contains detailed information about the respondent on the following subjects: marital history, schooling, labor force experience, health, fertility, child rearing practices and geographic mobility. Most importantly, the NLSY contains information about the health and health insurance status of all children born to female respondents. Information about child health and health insurance status was obtained in 1986, 1988, 1990, and 1992 for all children born by those dates. Similar to the NHIS, the measures of child health contained in the NLSY are maternal reports of the child's health including overall health rating (excellent-poor), the number of illnesses that required medical attention, whether the child is limited in play or school activities, whether the child has a limitation that requires medical attention, and the child's weight and height. Approximately half of the measures of weight and height were actual measurement. Similar to the other analysis samples, we limit the NLSY sample to children between the ages of 1 and 8 from families headed by unmarried women, age 19 or older who have 12 or less years of education.

The longitudinal nature of the NLSY provides two benefits. First, it allows us to construct cumulative measures of Medicaid program participation, private health insurance coverage and family income. In order to do so, however, we had to limit the sample to children born after 1984. The 1984 cutoff was used because information on health status and health insurance was first available in 1986. Therefore, we could not construct cumulative measures of program participation and health insurance coverage for children born prior to this date. The longitudinal nature of the NLSY also allows us to estimate fixed-effects models of the demand for child health. The fixed-effects procedure requires that we have at least two observations for each child, and thus, the sample was restricted to meet this criterion. As noted above, the fixed-effects procedure is a potential solution to the selection bias problem associated with Medicaid participation. Descriptive statistics for this sample are listed in the appendix.

E. Results

1. Infant Health

We begin this section by reviewing estimates of the effect of Medicaid participation on infant birth weight and the incidence of low-birth weight using data from the National Maternal and Infant Health Survey. Medicaid participation is measured by whether the mother's prenatal care was covered by Medicaid, and for those women who did not receive care, whether their delivery was covered by Medicaid. Only five percent of the sample did not receive care, and since there is no presumptive eligibility for Medicaid at time of delivery, use of the delivery insurance status is reasonable. A woman enrolled in Medicaid at delivery must have been enrolled in Medicaid prior to delivery.[14] Similar procedures were used to define private insurance coverage. Information on birth weight is obtained from birth certificates. The analysis is restricted to infants from families headed by unmarried women, age 19 or older who have 12 or less years of education.

We estimate a modified version of Equation (8) by ordinary least squares methods using the entire sample, and two sub-samples: infants who are covered by Medicaid or who are uninsured, and infants covered by either Medicaid or private insurance. Separating the sample according to health insurance status reduces unmeasured heterogeneity and facilitates the use of instrumental variables procedures that address the selection bias issue associated with Medicaid participation. As noted above, analyses of infant health outcomes do not require longitudinal data and the construction of cumulative measures of program participation. Accordingly, we estimate a model in which birth weight depends on prenatal care insurance coverage, family income in the last year, child race and sex, and current family characteristics such mother's age and marital

[14] There were 140 women who did not receive prenatal care: 27% were uninsured at delivery, 7% were insured by private insurance at delivery and the remaining had Medicaid insured deliveries. To test the sensitivity of our results to this assignment procedure, we dropped these women from the sample and re-estimated all models. The results obtained using this sample were very similar to those reported in the text.

status, mother's and father's education, and mother's and father's body mass index (weight/height^2). All estimates are obtained using weighted least squares since the NMIHS over samples low-birth weight infants and birth weight is our dependent variable (Korn and Graubard, 1995).

Table 1 lists the results for the analysis of infant birth weight measured as the natural logarithm of weight in grams. The first column presents the estimates obtained using the entire sample. These estimates indicate that both Medicaid and private insurance coverage had no significant effect on birth weight, although the estimate of the effect of private insurance coverage is positive and approaches commonly accepted levels of significance (p=.14). In fact, tests of the hypotheses that both the effect of Medicaid and private coverage were jointly zero could not be rejected at the .05 level of significance. Thus, there is only weak evidence that infant health is improved by having some type of private insurance coverage. The estimate associated with private insurance indicates that infants born to mothers with private insurance are two percent heavier than those born to mothers who are uninsured.

To test the sensitivity of the results in column 1 to the inclusion of additional variables, we re-estimated the model including measures of maternal smoking, alcohol consumption and drug use.[15] As we argued above, inclusion of additional observable variables correlated with birth weight may mitigate the selection bias problem if these variables are also correlated with insurance status. Estimates, which are not reported, of the effect of Medicaid and private insurance coverage obtained from these expanded specifications were very similar to those reported in column 1. This is particularly noteworthy given that smoking and cocaine use had large and statistically significant effects on birth weight. These results suggest that the selection

[15] The inclusion of these variables in a demand function for child health is justified by noting that these variables are choice variables and theoretically would have cross-price effects . Using the quantity instead of the prices of these commodities results in conditional demand functions. In these models, it is appropriate to treat the other consumption variables as endogenous. We ignore this problem because of data limitations and a lack of appropriate instruments.

problem may not be of much practical importance.

To further investigate the selection problem, we obtain estimates of the effect of Medicaid and private insurance on birth weight using an instrumental variables procedure. The instruments used to predict Medicaid participation and private insurance coverage are state dummy variables and a dummy variable indicating the mother worked prior to delivery. State dummy variables are effective instruments because of the significant cross-sectional variation in Medicaid eligibility rules and program implementation.[16] Tests of overidentifying restrictions could not reject the null hypothesis that these instruments were valid. The instrumental variables estimates are found in column 2. The estimate associated with Medicaid remains negative and is somewhat larger than its counterpart in column 1. The estimate associated with private insurance coverage is also larger than the OLS estimate in column 1, but is not statistically significant at the .05 level.[17] The instrumental variables estimates are consistent with the OLS estimates: relative to the uninsured, Medicaid has no statistically significant effect on birth weight, and private insurance has a positive, but insignificant effect on birth weight. Tests of exogeneity could not reject the null hypothesis that both Medicaid and private insurance are exogenous.

In columns 3 and 4, estimates of the effect of Medicaid are obtained using women who are either uninsured or covered by Medicaid. The primary reason we separated the sample in this way is to facilitate the use of an instrumental variables procedure. The collinearity that is created when the same set of instruments are used to predict two right-hand side endogenous variables is avoided when we separate the sample into groups that have only two insurance categories. In this way, our instrumental variables estimates will be more precise. Column 3 presents estimates from an ordinary least squares (OLS) regression, and in column 4, instrumental variables

[16] F-tests of the joint significance of the instruments in first-stage regressions had p-values of 0.001.

[17] The grouped nature of the instrumental variable, and the potential intra-class correlation within states, has been taken into account when calculating the standard errors of the second stage estimates.

estimates are presented. The OLS estimate of the effect of Medicaid is negative, very small and not statistically significant. This result is nearly identical to that in column 1. The instrumental variables estimate is negative and somewhat larger, but insignificant. In this case, tests of over-identification restrictions reject the null hypothesis that the instruments were valid, but tests of exogeneity could not reject that Medicaid participation was exogenous. Overall, the estimates in columns 3 and 4 are consistent with those in columns 1 and 2, and indicate that Medicaid has no effect on birth weight. Furthermore, in supplemental analyses that included maternal use of cigarettes, alcohol and drugs, OLS estimates of the effect of Medicaid were very similar to those in column 3.

The last two columns of Table 1 list estimates of the effect of Medicaid participation on birth weight among a sample of women either covered by Medicaid or private insurance. Similar to the results in column 1 and 2, the estimates in columns 5 and 6 indicate that women covered by private insurance have slightly heavier infants than those covered by Medicaid. Based on the OLS estimates, the differential is approximately two percent. The instrumental variables estimates have the same sign as the OLS, but are somewhat larger. Tests of over-identifying restrictions and of exogeneity could not reject the null hypotheses. Finally, when maternal use of cigarettes, alcohol and drugs were added to the model, the estimate of the effect of Medicaid participation was -0.011 and statistically insignificant. Taken together, these results suggest private insurance coverage improves infant health only slightly compared to Medicaid coverage. Indeed, the difference is usually not statistically significant.

The second measure of infant health we examine is the incidence of low-birth weight. Health insurance coverage may have a larger impact on the incidence of low-birth weight than on mean birth weight. Furthermore, differences in the incidence of low-birth weight have much larger effects on infant mortality than differences in mean birth weight.

Estimates of the effect of Medicaid participation on the incidence of low-birth weight are reported in Table 2. The estimates were obtained by OLS and instrumental variables procedures.[18] The basic pattern of results in Table 2 is very similar to that found in Table 1. Medicaid participation has no effect on the incidence of low-birth weight relative to being uninsured, and having private insurance lowers the incidence of low-birth weight relative to being uninsured or covered by Medicaid. Estimates in columns 2 and 5 indicate that private insurance coverage has a relatively large effect on the incidence of low-birth weight. For example, estimates in column 5 indicate that the incidence of low-birth weight is 2.5 percentage points lower among infants born to mothers covered by private insurance than among infants born to mothers covered by Medicaid.

The estimates related to private insurance, however, are not robust to model specification. When maternal cigarette, alcohol and drug use were added to the model in column 5, the estimate of the effect of Medicaid on the incidence of low-birth weight was reduced from 0.025 to 0.012, and it was no longer significant. This result suggests that there is some unobserved selection process that is affecting the estimates of the effect of private insurance, relative to Medicaid coverage, on infant health. One possibility is that unobserved differences in lifestyle explain differences in birth outcomes between women covered by private insurance and women covered by Medicaid. Alternatively, the prenatal care received by women covered by private insurance may be more effective at altering lifestyle choices than the prenatal care received by women covered by Medicaid. In summary, the results in Table 2 suggest that private insurance coverage lowers the incidence of low-birth weight relative to being uninsured or covered by Medicaid, but the difference is usually not significant, and that there is no significant difference in the incidence of low-birth weight between infants whose mothers are covered by Medicaid and infants whose mothers are uninsured.

Health insurance coverage can improve infant health because of differences in the

[18] For the OLS estimates, standard errors were corrected for heteroscedasticity using the method proposed by White (1980) and Heckman and MaCurdy (1985). For the IV estimates, we use Huber (1967) standard errors that account for the grouped nature of the instrumental variable.

quantity and quality of health care that women who are covered by insurance receive relative to those who are uninsured. Thus, our finding that women with private health insurance coverage have somewhat healthier infants than women with Medicaid coverage or women who are uninsured implies that these women are receiving more or better prenatal care. We test this hypothesis by examining the effect of health insurance status on the timing and number of prenatal care visits. The analysis of prenatal care parallels our analysis of birth weight. We examine two outcomes: the month that prenatal care was initiated, and the number of prenatal care visits conditional on the month care was started. For women who did not receive prenatal care we assigned them a value of 10 for the month of initiation of care.

The specification of the prenatal care demand models are similar to those used for the models of demand for child health. We omit the sex of child and parental body mass indices. In addition, in analyses of the number of prenatal care visits, the month of prenatal care initiation is included as an explanatory variable.[19] Estimates of the effect of Medicaid participation and private insurance coverage on prenatal care are listed in Table 3. The top panel of Table 3 lists the results for the month of initiation of care and the bottom panel presents the results for the number of prenatal care visits conditional on the month of initiation. Focusing on the top panel first, women covered by Medicaid begin prenatal care at the same time as women who are uninsured, and those covered by private insurance begin prenatal care earlier than either uninsured women or women covered by Medicaid. Women covered by private insurance begin care between 0.72 (column 1) and 0.94 (column 5) months earlier than other women. These estimates are relatively robust to methods of estimation and model specification. The instrumental variables estimates tend to be somewhat larger than the OLS estimates, but tests of exogeneity never rejected the null hypothesis. Finally, the inclusion of maternal cigarette, alcohol

[19] Including the month of prenatal care initiation in the analysis of the number of prenatal care visits may be problematic since the month of initiation is endogenous. We ignore the endogeneity since there is no easy solution and this analysis is not the focus of our paper.

and drug use in the model had little effect on the estimates.

In regard to the number of prenatal care visits, the estimates in Table 3 indicate that women covered by either private insurance or Medicaid had the same number of prenatal care visits than uninsured women. Alternative estimation procedures or model specifications do not alter the general nature of the findings.

Estimates of the effect of Medicaid on prenatal care are consistent with estimates of the effect of Medicaid on birth weight. Women covered by Medicaid had infants of equal birth weight and equal rates of low-birth weight than uninsured women. Consistent with these outcomes, women covered by Medicaid also received about the same amount of prenatal care. On the other hand, women covered by private insurance had marginally heavier infants and lower rates of low-birth weight than women covered by Medicaid or women who were uninsured. Women covered by private insurance also received significantly more prenatal care than women on Medicaid or women who were uninsured.

2. Child Health

As noted in the introduction, there has been very little previous study of the effect of the Medicaid program on child health. Virtually all previous studies have focused on birth outcomes. Medicaid coverage, however, may have very different effects on child health than it does on infant health. For example, the scope for health care intervention may be much wider with regard to child health than it is for infants since in the latter case it is primarily restricted to prenatal care. Accordingly, we examine the effect of Medicaid on a variety of maternal reports of child health. We use two different data sets for this purpose: the National Health Interview Survey (NHIS) and the National Longitudinal Survey of Youth (NLSY).

a) Analyses Using the National Health Interview Survey (NHIS)

We use two years of the NHIS surveys: 1989 and 1992. These two years were chosen because in both of these years, supplemental questions related to health insurance status were included in the survey, and during the intervening period between 1989 and 1992, the Medicaid program was greatly expanded. Thus, we can link information on child health insurance coverage to maternal reports of child health status, and use the time variation in Medicaid eligibility rules to instrument for Medicaid participation. We limit the sample to children between the ages of 2 and 12 from families headed by an unmarried women, age 19 or older who has 12 or less years of education.

The specification of the child health demand model is very similar to the one we used for infant health. We use several measures of child health: whether the mother rated her child's health as excellent or good, whether the child had an acute condition that limits activity, whether the child had a restricted activity day in the past two weeks, whether the child had a restricted bed day in the past two weeks and the number of restricted bed days in the past 12 months. We focus on acute conditions, defined as occurring in the past two weeks, because they are more amenable to health care interventions than chronic conditions which include many congenital illnesses. In addition to these health outcome measures, we also examine the effect of Medicaid on the number of doctor visits in the past 12 months. For these analyses, we include the number of chronic and acute conditions affecting the child as a control for health status.

Our explanatory variables include health insurance coverage and the following: child characteristics – age, race and sex; mother characteristics – age, education, marital status, and health status; family income in the past year; a dummy variable indicating urban residence; and year and state dummy variables. Mother's health status is included in the model to control for unobserved tastes and genetic differences in health. One drawback of the NHIS data is that is cross-sectional and therefore we are unable to construct cumulative measures of Medicaid program participation, private insurance coverage and family income. This may lead to some measurement error bias.

Table 4 lists the estimates of the effect of Medicaid and private insurance coverage on child health, and each row in Table 4 corresponds to a different model and measure of child health. A complete set of estimates for a representative model are contained in the appendix. Models in which the dependent variable is dichotomous are estimated with a correction for heteroscedasticty.

Columns 1 and 2 of Table 4 list the estimates obtained by OLS procedures using the entire sample.[20] There is only one significant estimate associated with private insurance coverage. Mothers of children with private insurance coverage are more likely to rate their child's health as excellent or good than mothers of children without insurance coverage. In the case of Medicaid, mothers of children covered by Medicaid are more likely to report that their child has had more restricted bed days in the last 12 months than mothers of children without insurance. In addition, mothers of children covered by Medicaid report more doctor visits in the last 12 months than mothers of children without insurance.

To test the sensitivity of these findings to differences in model specification, we re-estimated all models related to columns 1 and 2 excluding measures of mother's health status (e.g., self reported health, height and weight). The estimates obtained from these models were very similar to those reported in Table 4 even though mother's health status is a highly significant and numerically important determinant of child health status. This result suggests that there may not be a significant selection bias problem in this sample.

In columns 3 and 4 of Table 4, the sample is restricted to children who are either covered by Medicaid or uninsured. Column 3 lists the estimates from OLS regressions and column 4 lists the estimates from an instrumental variables procedure. The estimates in column 3 are very similar to those in column 1, and this was also true for models that omitted mother's health status. In column 4, however, we notice one significant change. Instrumental variables estimates of the effect of Medicaid on child health indicate that children covered by Medicaid are more likely to

[20] We do not present the instrumental variables estimates that correspond to the estimates in column 1 because they are qualitatively similar to other estimates presented in Table 4.

be rated in excellent or good health by their mothers than children without insurance coverage.

The final set of estimates of the effect of Medicaid on child health using the NHIS are found in columns 5 and 6 of Table 4. In this case, the analysis is restricted to children covered by either Medicaid or private health insurance. The OLS estimates in column 5 are similar to those in column 1, and are not very sensitive to model specification as estimates changed little when mother's health status was omitted from the model. Medicaid coverage is associated with a smaller probability of being rated in good health, and more reported activity limitations, restricted bed days and doctor visits. The instrumental variables estimates in column 6 suggest that some of the OLS estimates may be subject to selection bias, although the imprecision of these estimates makes it hard to evaluate this hypothesis. In particular, the instrumental variables estimates of the effect of Medicaid on whether the mother rates the child to be in excellent or good health, and the number of restricted bed days in the past 12 months have signs and magnitudes that are the opposite of the OLS estimates.

Overall, the estimates in Table 4 suggest that child health is not greatly affected by health insurance. Children who are uninsured appear to be in as good health as those with health insurance, although in most cases mothers of uninsured children rated their child to be in worse health than mothers of children covered by either Medicaid or private insurance. The magnitude of the effects was not large. For example, mothers of children covered by private insurance rated their children to be in good or excellent health about five percent more than mothers of uninsured children. On the other hand, mothers of children covered by Medicaid were more likely to report that their child had a greater number of restricted bed days than mothers of uninsured children or mothers of children covered by private insurance. This latter result, however, was sensitive to estimation procedures as instrumental variables estimates indicated no adverse health consequences associated with Medicaid coverage. Finally, children covered by Medicaid tended to have higher rates of utilization than uninsured children. The results are difficult to summarize. The absence of a consistent set of results or results that accord with measures of health care utilization (i.e., doctor visits) raise questions about the benefits of health insurance on child health.

b) Analyses using the National Longitudinal Survey of Youth (NLSY)

Our final analysis also examines the effect of Medicaid on child health, but in this case we use data from the NLSY. One advantage of using the NLSY is that we have two or more observations of the same child for a relatively large number of children. Therefore, we can estimate fixed-effects models that control for unobserved, time-invariant factors that affect child health. If the selection problem is a result of these omitted time-invariant factors, the fixed effect procedure is a solution. Another advantage of the NLSY is that we can construct cumulative measures of Medicaid program participation, private insurance coverage and family income. As argued above, specifying a demand for child health model that depends only on contemporaneous measures may introduce a significant amount of measurement error into the analysis. The NLSY sample consists of children between the ages of 1 and 8, from families headed by an unmarried woman, age 19 or older who has 12 or less years of education. The sample was limited to those with at least two observations.

The NLSY has a limited number of child health outcomes, and we use the following: whether the child had an illness that required medical attention in the last year, the number of illnesses that required medical attention in the last year, and child nutritional status as measured by weight and height. All of these measures are maternal reports, except for child weight and height which are actual measurements in approximately half the cases. The explanatory variables in the model include insurance status, child age, race, sex and birth weight, mother's age, education and marital status, family income, a dummy variable indicating residence in an urban area, and a dummy variable indicating whether the mother has a health problem that limits her activity. An important aspect of the NLSY is that we are able to construct cumulative measures of program participation, insurance coverage and family income. Thus, Medicaid participation is measured as the proportion of survey years that the mother reported the child was covered by Medicaid. A similar definition is used for private insurance coverage, and family income is the average annual family income from wages and salary during the child's life.

Estimates of the effect of Medicaid and private insurance on child health are listed in Table 5. Each row of the table represents a different model and measure of child health. A complete set of results for a representative model is contained in the appendix. We focus on the fixed-effects estimates in columns 3 and 4 since the fixed-effects model is theoretically superior to OLS in this problem. The fixed-effects procedure controls for unobserved time-invariant factors that affect child health and may be a solution to the selection bias problem. There is only one significant result related to child health outcomes. Children covered by Medicaid are heavier than uninsured children. With respect to health care utilization, however, both children covered by Medicaid and those with private insurance are more likely to have had a doctor visit in the last 12 months. This result is conditional on the child's health status as measured by whether the child has an activity limitation. The results in Table 5 confirm our earlier findings related to the effect of health insurance on child health. Children covered by Medicaid and those covered by private insurance do not appear to be in significantly better health than children who are uninsured.

F. Summary and Conclusion

In this study, we have extensively examined the effect of Medicaid on infant and child health. We used a variety of data sources and several different statistical procedures to investigate this question. Overall, our results suggest that Medicaid has little effect on infant and child health. Indeed, there was only limited evidence that private insurance coverage has a positive effect on child health. Private insurance coverage improved infant health by lowering the incidence of low-birth weight. Otherwise, private insurance coverage had little effect on child health.

One explanation of these results is that uninsured children may be receiving adequate health care. Parents of uninsured children in poor and near-poor families may be paying for essential and effective health care out-of-pocket, or these children may receive the necessary care at hospitals and clinics that treat the uninsured free of charge. In fact, children covered by private insurance may face the most expensive health care if there are significant co-payments associated with their insurance plan.

A second explanation of our findings is that our measures of infant and child health do not adequately reflect the benefits of having Medicaid or private insurance. Health insurance may have very specific effects on child health that are manifest only in regard to certain types of infant and child illnesses. Furthermore, except for birth weight and child height and weight, all measures of child health used in this analysis were maternal reports. These subjective reports may measure actual child health with significant error. Random measurement error would decrease the likelihood of finding significant effects, and non-random measurement error may bias estimates of the parameters of interest. Furthermore, some measures of child health were also measures of utilization. For example, the number of illnesses that required medical attention may reflect differences in utilization as well as differences in child health. It may be the case that children with private insurance or Medicaid went to the doctor more often when sick, and this would result in a greater number of illnesses that required medical attention. Therefore, children covered by private insurance or Medicaid may be healthier than uninsured children, but because of differences in utilization we find no measured health benefits of insurance, or even health deficits.

Taken at face value, however, our results suggest that publicly financed health care does not lead to significant improvements in the health of infants and children from poor and near-poor families. These results suggest that the justification for continued support of the Medicaid program should be made on another basis. Perhaps the revenue stream that Medicaid provides to the health care industry allows providers to offer free care to the uninsured. Cutting back or eliminating this source of revenue may result in a significant worsening of the health of uninsured children. Indeed, an analysis of the effect of Medicaid subsequent to such a policy change may find that Medicaid had significant health benefits, as the health of uninsured children deteriorated because of inadequate care. Thus, the results of our study do not necessarily support a reduction in publicly financed health insurance. Moreover, publicly financed health care may provide an income subsidy that frees poor and near-poor families from the financial stress that burdens their lives. Eliminating this income subsidy may create other social problems not strictly health related as families face greater financial hardship.

What our results do suggest, however, is that we need to question the presumption that health insurance is the cure for adverse health outcomes among poor and near-poor children. Our results, and the results of most past research, contradict this presumption. There needs to be additional research that examines directly the relationship between health insurance status and health outcomes. Too much prior research has been content with showing that health care utilization increases with insurance coverage and then assuming that health also improves. In addition, there needs to be more research of the financial benefits for the health care industry of Medicaid financed care. Although Medicaid payments may enable providers to care for uninsured children, the cross-subsidy implicit in this arrangement may be inefficient. Alternative financing schemes may provide the same level of health at a reduced cost.

Table 1

The Effect of Health Insurance Status on Birth Weight Using NMIHS Data (Birth Weight is Expressed in Natural Logarithm)

	OLS Medicaid, Private Insurance and Uninsured	2SLS Medicaid, Private Insurance and Uninsured	OLS Medicaid and Uninsured	2SLS Medicaid and Uninsured	OLS Medicaid and Private Insurance	2SLS Medicaid and Private Insurance
Medicaid Participation	-0.005	-0.019	-0.007	-0.029	-0.023+	-0.035
	(0.012)	(0.040)	(0.012)	(0.036)	(0.013)	(0.036)
Private Insurance	0.022	0.039	-	-	-	-
	(0.015)	(0.057)				
Household Income ($1000)	0.0004	0.0001	-0.0002	-0.0003	0.0004	0.0003
	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Black	-0.069**	-0.066**	-0.070**	-0.068**	-0.063**	-0.062**
	(0.011)	(0.011)	(0.012)	(0.011)	(0.012)	(0.012)
Hispanic	0.009	0.010	0.018	0.016	0.00002	0.001
	(0.015)	(0.021)	(0.016)	(0.021)	(0.0163)	(0.026)
Male	0.044**	0.044**	0.044**	0.045**	0.048**	0.048**
	(0.010)	(0.009)	(0.011)	(0.011)	(0.011)	(0.010)
Mother's Body Mass Index	0.006**	0.006**	0.006**	0.006**	0.006**	0.006**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's Age	0.014	0.013	0.014	0.014	0.018+	0.018+
	(0.009)	(0.011)	(0.010)	(0.011)	(0.010)	(0.010)
Mother's Age-Squared	-0.0003	-0.0002	-0.0003	-0.0003	-0.0003	-0.0003
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Mother Has Never Been Married	-0.016	-0.013	-0.011	-0.010	-0.033**	-0.031*
	(0.011)	(0.010)	(0.012)	(0.012)	(0.012)	(0.013)
Mother Has Completed High School	0.014	0.011	0.025*	0.024+	0.014	0.013
	(0.010)	(0.012)	(0.011)	(0.013)	(0.012)	(0.012)
Number of Previous Still Births	-0.139**	-0.135*	-0.116**	-0.116*	-0.122**	-0.120*
	(0.041)	(0.060)	(0.041)	(0.055)	(0.046)	(0.053)
Number of Previous Miscarriages	-0.012	-0.011	-0.011	-0.010	-0.014	-0.013
	(0.010)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)
Father's Body Mass Index	0.0008	0.0008	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Father's Education	-0.0002	-0.0003	-0.0014	-0.0013	0.0002	0.0001
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Residence in Metropolitan Area	-0.010	-0.010	-0.009	-0.009	-0.008	-0.008
	(0.012)	(0.013)	(0.013)	(0.014)	(0.013)	(0.013)

Notes: In all models, the sample is restricted to unmarried women aged 19 or above with a high school degree or less. In the 2SLS regressions state dummy variables and a dummy variable indicating mother worked prior to delivery are used as instruments. Data are weighted to be nationally representative. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .10$

Table 1, Continued

The Effect of Health Insurance Status on Birth Weight Using NMIHS Data
(Birth Weight is Expressed in Natural Logarithm)

	OLS Medicaid, Private Insurance and Uninsured	2SLS Medicaid, Private Insurance and Uninsured	OLS Medicaid and Uninsured	2SLS Medicaid and Uninsured	OLS Medicaid and Private Insurance	2SLS Medicaid and Private Insurance
F-statistic: Medicaid = Private	4.062*	2.131	-	-	-	-
F-statistic: Medicaid = Private = 0	2.073	1.107	-	-	-	-
F-statistic: Overidentifying Restrictions	-	1.343	-	1.432*	-	0.872
F-statistic: Exogeneity	-	0.398	-	0.679	-	0.124
Dependent Variable Mean	8.050	8.050	8.037	8.037	8.051	8.051
R-Squared	0.057	0.057	0.049	0.049	0.065	0.065
N	2588	2588	2126	2126	2048	2048

Notes: In all models, the sample is restricted to unmarried women aged 19 or above with a high school degree or less. In the 2SLS regressions state dummy variables and a dummy variable indicating mother worked prior to delivery are used as instruments. Data are weighted to be nationally representative. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .10$

Table 2

The Effect of Health Insurance Status on Low Birth Weight Using NMIHS Data (Birth Weight ≤ 2500 Grams)

	OLS Medicaid, Private Insurance and Uninsured	2SLS Medicaid, Private Insurance and Uninsured	OLS Medicaid and Uninsured	2SLS Medicaid and Uninsured	OLS Medicaid and Private Insurance	2SLS Medicaid and Private Insurance
Medicaid Participation	0.008 (0.014)	0.007 (0.037)	0.008 (0.014)	0.015 (0.032)	0.025+ (0.014)	0.042 (0.044)
Private Insurance	-0.014 (0.015)	-0.076 (0.052)	-	-	-	-
Household Income ($1000)	-0.0007* (0.0003)	-0.0003 (0.0005)	-0.0007 (0.0005)	-0.0006 (0.0005)	-0.0006+ (0.0004)	-0.0005 (0.0005)
Black	0.056** (0.012)	0.051** (0.011)	0.059** (0.015)	0.058** (0.011)	0.049** (0.014)	0.048** (0.011)
Hispanic	-0.016 (0.018)	-0.020 (0.024)	-0.022 (0.021)	-0.021 (0.029)	-0.005 (0.021)	-0.006 (0.024)
Male	-0.033** (0.011)	-0.032** (0.011)	-0.035** (0.013)	-0.036* (0.015)	-0.039** (0.013)	-0.039** (0.012)
Mother's Body Mass Index	-0.003** (0.001)	-0.003** (0.001)	-0.004** (0.001)	-0.004** (0.001)	-0.003** (0.001)	-0.003** (0.001)
Mother's Age	-0.017 (0.011)	-0.014 (0.010)	-0.020 (0.014)	-0.021+ (0.012)	-0.016 (0.013)	-0.015 (0.009)
Mother's Age-Squared	0.0004+ (0.0002)	0.0003 (0.0002)	0.0004+ (0.0003)	0.0004+ (0.0002)	0.0004 (0.0002)	0.0004 (0.0002)
Mother Has Never Been Married	0.010 (0.012)	0.004 (0.009)	0.005 (0.015)	0.004 (0.011)	0.015 (0.014)	0.013 (0.012)
Mother Has Completed High School	-0.041** (0.012)	-0.034** (0.011)	-0.049** (0.014)	-0.049** (0.011)	-0.036** (0.014)	-0.034* (0.015)
Number of Previous Still Births	0.213** (0.073)	0.205* (0.087)	0.200** (0.073)	0.200* (0.087)	0.163* (0.076)	0.160* (0.079)
Number of Previous Miscarriages	0.010 (0.012)	0.009 (0.009)	0.001 (0.014)	0.0007 (0.011)	0.012 (0.015)	0.011 (0.010)
Father's Body Mass Index	0.0003 (0.001)	0.0004 (0.001)	0.0007 (0.002)	0.0007 (0.002)	0.0004 (0.002)	0.0004 (0.002)
Father's Education	0.0001 (0.003)	0.0005 (0.003)	0.0008 (0.003)	0.0008 (0.003)	0.001 (0.003)	0.001 (0.003)
Residence in Metropolitan Area	0.003 (0.013)	0.004 (0.014)	0.004 (0.016)	0.004 (0.016)	0.004 (0.015)	0.004 (0.016)

Notes: In all models, the sample is restricted to unmarried women aged 19 or above with a high school degree or less. In the 2SLS regressions state dummy variables and a dummy variable indicating mother worked prior to delivery are used as instruments. Data are weighted to be nationally representative. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .10$

Table 2, Continued

The Effect of Health Insurance Status on Low Birth Weight Using NMIHS Data
(Birth Weight ≤ 2500 Grams)

	OLS Medicaid, Private Insurance and Uninsured	2SLS Medicaid, Private Insurance and Uninsured	OLS Medicaid and Uninsured	2SLS Medicaid and Uninsured	OLS Medicaid and Private Insurance	2SLS Medicaid and Private Insurance
F-statistic: Medicaid = Private	2.586	2.760+	-	-	-	-
F-statistic: Medicaid = Private = 0	1.298	1.397	-	-	-	-
F-statistic: Overidentifying Restrictions	-	0.622	-	0.670	-	0.490
F-statistic: Exogeneity	-	1.140	-	0.040	-	0.148
Dependent Variable Mean	0.109	0.109	0.121	0.121	0.109	0.109
R-Squared	0.036	0.031	0.033	0.033	0.034	0.033
N	2588	2588	2126	2126	2048	2048

Notes: In all models, the sample is restricted to unmarried women aged 19 or above with a high school degree or less. In the 2SLS regressions state dummy variables and a dummy variable indicating mother worked prior to delivery are used as instruments. Data are weighted to be nationally representative. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .10$

Table 3

The Effect of Health Insurance Status on Timing of Prenatal Care Using NMIHS Data

	OLS Medicaid, Private Insurance and Uninsured	2SLS Medicaid, Private Insurance and Uninsured	OLS Medicaid and Uninsured	2SLS Medicaid and Uninsured	OLS Medicaid and Private Insurance	2SLS Medicaid and Private Insurance
Medicaid Participation	0.123	-0.094	0.128	-0.038	0.939**	0.890**
	(0.102)	(0.377)	(0.107)	(0.424)	(0.110)	(0.378)
Private Insurance	-0.717**	-1.341**	-	-	-	-
	(0.122)	(0.445)				
F-statistic: Medicaid = Private	58.920**	14.853**	-	-	-	-
F-statistic: Medicaid = Private = 0	30.538**	8.005**	-	-	-	-
F-statistic: Overidentifying Restrictions	-	1.822**	-	2.263**	-	1.942**
F-statistic: Exogeneity	-	1.351	-	0.916	-	0.032
Dependent Variable Mean	3.466	3.466	3.735	3.735	3.424	3.424
R-Squared	0.093	0.077	0.038	0.038	0.113	0.087
N	2491	2491	2047	2047	1975	1975

Notes: In all models, the sample is restricted to unmarried women aged 19 or above with a high school degree or less. In the 2SLS regressions state dummy variables and a dummy variable indicating mother worked prior to delivery are used as instruments. Data are weighted to be nationally representative. Standard errors are in parentheses. ** p < .01, * p < .05, + p < .10

Table 3, Continued

The Effect of Health Insurance Status on Number of Prenatal Care Visits Using NMIHS Data

	OLS Medicaid, Private Insurance and Uninsured	2SLS Medicaid, Private Insurance and Uninsured	OLS Medicaid and Uninsured	2SLS Medicaid and Uninsured	OLS Medicaid and Private Insurance	2SLS Medicaid and Private Insurance
Medicaid Participation	0.009 (0.197)	0.217 (1.524)	0.051 (0.198)	0.258 (1.785)	-0.083 (0.214)	-0.410 (0.906)
Private Insurance	-0.009 (0.238)	0.385 (1.273)	-	-	-	-
F-statistic: Medicaid = Private	0.007	0.073	-	-	-	-
F-statistic: Medicaid = Private = 0	0.004	0.164	-	-	-	-
F-statistic: Overidentifying Restrictions	-	3.375**	-	3.060**	-	3.372**
F-statistic: Exogeneity	-	0.025	-	0.261	-	0.425
Dependent Variable Mean	9.945	9.945	9.492	9.492	9.993	9.993
R-Squared	0.422	0.422	0.435	0.435	0.427	0.427
N	2266	2266	1871	1871	1812	1812

Notes: In all models, the sample is restricted to unmarried women aged 19 or above with a high school degree or less. In the 2SLS regressions state dummy variables and a dummy variable indicating mother worked prior to delivery are used as instruments. Data are weighted to be nationally representative. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .10$

Table 4

The Effect of Health Insurance Status on Health of Children Aged 2 to 12 Using NHIS Data

Dependent Variable	OLS Medicaid, Private Insurance, and Uninsured		OLS Medicaid and Uninsured	2SLS Medicaid and Uninsured	OLS Medicaid and Private Insurance	2SLS Medicaid and Private Insurance
	Medicaid (1)	Private (2)	Medicaid (3)	Medicaid (4)	Medicaid (5)	Medicaid (6)
Child is in Excellent or Very Good Health	0.022 (0.018)	0.045* (0.019)	0.013 (0.019)	0.162* (0.078)	-0.031+ (0.017)	0.021 (0.086)
F-statistic: Overidentifying Restrictions	-	-	-	1.012	-	1.228*
F-statistic: Exogeneity	-	-	-	4.248*	-	0.317
Child Had Acute Condition Such As Infectious Diseases, Injuries, Respiratory Ailment	0.002 (0.012)	0.003 (0.013)	-0.002 (0.012)	0.027 (0.043)	-0.005 (0.011)	-0.091 (0.064)
F-statistic: Overidentifying Restrictions	-	-	-	1.088	-	1.231*
F-statistic: Exogeneity	-	-	-	0.317	-	1.793
Child Had Restricted Activity Days in Past Two Weeks	-0.014 (0.013)	-0.016 (0.015)	-0.011 (0.013)	-0.035 (0.051)	-0.003 (0.013)	-0.079 (0.051)
F-statistic: Overidentifying Restrictions	-	-	-	1.029	-	0.972
F-statistic: Exogeneity	-	-	-	0.207	-	1.261
N	5084	5084	3582	3582	4278	4278

Notes: In all models, the sample is restricted to children of unmarried women aged 19 or above with a high school degree or less. All regressions include the following set of explanatory variables: dummy variables for ages 3 through 12, race, and sex; mother's age and age squared; dummy variables indicating mother is never married, or has high school degree; dummy variables for level of income; mother's health status, weight and height, indicator for residence in a MSA, or in an urban area, state dummy variables, and a dummy variable for year. In the regression of number of doctor visits in past 12 months, child's number of chronic or acute conditions are added as a regressor. In the 2SLS regressions state-year, state-age, and year-age interactions are used as instruments, where dummy variables for the age groups are 2-3, 4-6, and 7 or above. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .10$

Table 4, Continued

The Effect of Health Insurance Status on Health of Children Aged 2 to 12 Using NHIS Data

Dependent Variable	OLS Medicaid, Private Insurance, and Uninsured		OLS Medicaid and Uninsured	2SLS Medicaid and Uninsured	OLS Medicaid and Private Insurance	2SLS Medicaid and Private Insurance
Child Had Restricted Bed Day in Past Two Weeks	-0.005 (0.011)	-0.011 (0.012)	-0.002 (0.011)	-0.038 (0.043)	-0.001 (0.010)	-0.017 (0.051)
F-statistic: Overidentifying Restrictions	-	-	-	1.192+	-	1.258*
F-statistic: Exogeneity	-	-	-	0.748	-	0.112
Number of Restricted Bed Day in Past 12 Months	2.931** (1.140)	1.295 (0.931)	3.004* (1.303)	-0.794 (3.260)	1.828 (1.123)	-16.759* (6.746)
F-statistic: Overidentifying Restrictions	-	-	-	0.398	-	0.717
F-statistic: Exogeneity	-	-	-	0.428	-	6.396*
Number of Doctor Visits in Past 12 Months	0.473+ (0.254)	0.106 (0.300)	0.480+ (0.253)	1.650* (0.790)	0.390 (0.276)	-0.526 (1.304)
F-statistic: Overidentifying Restrictions	-	-	-	3.422**	-	4.020**
F-statistic: Exogeneity	-	-	-	1.711	-	0.349
N	5084	5084	3582	3582	4278	4278

Notes: In all models, the sample is restricted to children of unmarried women aged 19 or above with a high school degree or less. All regressions include the following set of explanatory variables: dummy variables for ages 3 through 12, race, and sex; mother's age and age squared; dummy variables indicating mother is never married, or has high school degree; dummy variables for level of income; mother's health status, weight and height, indicator for residence in a MSA, or in an urban area, state dummy variables, and a dummy variable for year. In the regression of number of doctor visits in past 12 months, child's number of chronic or acute conditions are added as a regressor. In the 2SLS regressions state-year, state-age, and year-age interactions are used as instruments, where dummy variables for the age groups are 2-3, 4-6, and 7 or above. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .10$

Table 5

The Effect of Health Insurance Status on Health of Children Aged 1 to 8 Using NLSY Data

	OLS Medicaid, Private Insurance, and Uninsured		Fixed Effects Medicaid, Private Insurance, and Uninsured	
	Medicaid (1)	Private (2)	Medicaid (3)	Private (4)
Child Had Illnesses that Required Medical Attention in the Past Year [N = 1497]	-0.013 (0.044)	0.051 (0.053)	0.091 (0.104)	0.112 (0.112)
Number of Illnesses Child Had in the Past Year [N = 1493]	0.066 (0.191)	0.163 (0.202)	-0.240 (0.337)	0.444 (0.444)
Child's Weight in Kilograms [N = 1396]	0.450 (0.381)	1.000* (0.462)	2.148* (0.934)	0.966 (0.906)
Child's Weight is Below the 10th Percentile of Standard Distribution [N = 1396]	-0.073 (0.044)	-0.100+ (0.053)	-0.130 (0.080)	-0.061 (0.082)
Child's Height in Meters [N = 1432]	0.004 (0.008)	0.017+ (0.010)	0.025 (0.020)	-0.012 (0.023)
Child's Height is Below the 10th Percentile of Standard Distribution [N = 1432]	-0.092* (0.044)	-0.152** (0.052)	0.050 (0.089)	0.002 (0.101)
Child's Body Mass Index (Grams per Meters Squared) [N = 1356]	0.546 (0.540)	0.520 (0.599)	-1.224 (0.988)	1.363 (1.357)
Child's Body Mass Index is Below the 10th Percentile of Standard Distribution [N = 1356]	-0.029 (0.041)	-0.096+ (0.052)	0.098 (0.092)	0.156 (0.097)
Child Had a Doctor Visit for Checkup in the Past Year [N = 1449]	0.003 (0.040)	0.035 (0.046)	0.230** (0.086)	0.192+ (0.099)

Notes: In all models, the sample is restricted to children of unmarried women aged 19 or above with a high school degree or less. OLS regressions include the following set of explanatory variables: dummy variables for ages 2 through 8, race, origin, and sex; child's birth weight, mother's age and age squared; dummy variables indicating mother is never married, or has high school degree; average of wage income for each family; dummy variable indicating whether mother has limitations to work, indicator for residence in a central SMSA, or in an urban area, dummy variables for regions, and year dummy variables. Fixed-effect regressions include mother's age, average wage income for each family, dummy variable indicating whether mother has limitations to work , and year dummy variables. In the regression of child had a doctor visit for checkup in the past year, child's activity limitation is added as a regressor. Number of observations are in brackets. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .10$

Table A1

Descriptive Statistics for NMIHS Data

	Mean	Standard Deviation
Infant Birth Weight Measured as the Natural Logarithm of Weight in Grams	8.050	0.276
Incidence of Low Birth Weight (Birth Weight $\leq$ 2500 Grams)	0.109	0.344
Month of Prenatal Care Initiation	3.117	1.961
Number of Prenatal Care Visits	9.953	5.097
Medicaid Coverage	0.533	0.551
Private Health Insurance Coverage	0.239	0.471
No Health Insurance Coverage	0.228	0.463
Household Income ($1000)	14.724	17.043
Black	0.371	0.533
Hispanic	0.170	0.415
Female	0.462	0.551
Mother's Body Mass Index (Weight /Height^2)	22.964	5.321
Mother's Age	24.643	5.377
Mother Has Never Been Married	0.583	0.545
Mother Has Completed High School	0.611	0.539
Number of Previous Still Births	0.013	0.129
Number of Previous Miscarriages	0.163	0.522
Mother Smoked Cigarette in the Year Prior to Delivery	0.366	0.532
Mother Drank Alcoholic Beverage in the Year Prior to Delivery	0.190	0.434
Mother Smoked Marijuana in the Year Prior to Delivery	0.038	0.212
Mother Used Cocaine in the Year Prior to Delivery	0.018	0.146
Mother Worked in the Year Prior to Delivery	0.585	0.544
Father's Body Mass Index (Weight /Height^2)	24.819	4.573
Father's Education	11.428	2.666
Residence in Metropolitan Area	0.767	0.467
N	2588	

Notes: The sample is restricted to unmarried women aged 19 or above with a high school degree or less. Data are weighted to be nationally representative.

Table A2

Descriptive Statistics for NHIS Data

	Mean	Standard Deviation
Child is in Excellent or Very Good Health	0.674	0.469
Child Had Acute Condition Such As Infectious Diseases, Injuries, Respiratory Ailment	0.096	0.295
Child Had Restricted Activity Day in Past Two Weeks	0.113	0.317
Child Had Restricted Bed Day in Past Two Weeks	0.069	0.254
Number of Restricted Bed Days in Past 12 Months	5.234	31.787
Number of Doctor Visits in Past 12 Months	2.873	6.647
Medicaid Coverage	0.546	0.498
Private Health Insurance Coverage	0.295	0.456
No Health Insurance Coverage	0.158	0.365
Family Income ($1000)	10.440	8.283
Child's Age	7.086	3.196
Child Had Acute or Chronic Condition	0.385	0.738
Black	0.425	0.494
Hispanic	0.209	0.407
Female	0.509	0.500
Mother's Age	31.836	6.752
Mother Has Never Been Married	0.358	0.479
Mother Has Completed High School	0.573	0.494
Mother's Health (0 = poor, 5 = excellent)	2.469	1.093
Mother's Weight in Pounds	153.664	37.829
Mother's Height in Inches	63.975	2.835
Residence in Central City	0.528	0.499
Residence in Urban Area	0.287	0.452
N	5084	

Notes: The sample is restricted to unmarried women aged 19 or above with a high school degree or less. All means are unweighted.

Table A3

Descriptive Statistics for NLSY Data

	Mean	Standard Deviation
Child Had Illness that Required Medical Attention in the Past Year	0.333	0.472
Number of Illnesses Child Had in the Past Year	0.782	1.843
Child's Weight in Kilograms	17.797	6.384
Child's Weight is Below the 10th Percentile of Standard Distribution	0.137	0.344
Child's Height in Meters	1.023	0.176
Child's Height is Below the 10th Percentile of Standard Distribution	0.198	0.398
Child's Body Mass Index (Weight /Height^2)	17.050	5.455
Child's Body Mass Index is Below the 10th Percentile of Standard Distribution	0.180	0.384
Child Had a Doctor Visit for Checkup in the Past Year	0.770	0.421
Percent of Time Covered by Medicaid	0.568	0.435
Percent of Time Covered by Private Health Insurance	0.267	0.391
Percent of Time Not Covered by Any Health Insurance	0.165	0.298
Average Family Wage Income ($1000)	6.256	9.364
Child's Age	4.379	2.007
Birth Weight Measured in Ounces	111.679	21.971
Child Had Conditions that Limited Activity or that Required Medical Care in the Past Year	0.109	0.311
Black	0.490	0.391
Hispanic	0.189	0.500
Female	0.484	0.500
Mother's Age	28.557	2.948
Mother Has Never Been Married	0.493	0.500
Mother Has Health Limitations	0.066	0.249
Mother Has Completed High School	0.634	0.482
Residence in Central City	0.211	0.408
Residence in Urban Area	0.506	0.500
Residence in North East Region	0.141	0.348
Residence in North Central Region	0.252	0.435
Residence in South Region	0.421	0.494
N	1647	

Notes: The sample is restricted to unmarried women aged 19 or above with a high school degree or less. All means are unweighted. Sample size is lower when dependent variables are included in the regression.

Table A4

Representative Model of the Effect of Health Insurance Status on Health of Children Aged 2 to 12 Using NHIS Data - First Column of Table 4

Dependent Variable: Child is in Excellent or Very Good Health		
Medicaid Coverage	0.022	(0.018)
Private Health Insurance Coverage	0.045*	(0.019)
Black	-0.056**	(0.017)
Hispanic	-0.065**	(0.020)
Female	0.026*	(0.012)
Mother's Age	-0.009	(0.007)
Mother's Age-Squared	0.0001	(0.0001)
Mother Has Never Been Married	0.004	(0.014)
Mother Has Completed High School	0.029	(0.020)
Mother's Health	-0.170**	(0.006)
Mother's Weight	-0.001	(0.003)
Mother's Height	0.0001	(0.008)
Interaction of Mother's Weight and Height	0.00002	(0.00005)
Family Income is Between 5,000 and 7,999	0.029	(0.020)
Family Income is Between 8,000 and 10,999	-0.013	(0.021)
Family Income is Between 11,000 and 14,999	0.052*	(0.023)
Family Income is Between 15,000 and 19,999	0.104**	(0.024)
Family Income is Between 20,000 and 29,999	0.081**	(0.027)
Family Income is Between 30,000 and 39,999	0.020	(0.048)
Family Income is Between 40,000 and above	0.169**	(0.036)
Indicator of Missing Family Income	0.029	(0.021)
Residence in Central City	-0.003	(0.019)
Residence in Urban Area	0.017	(0.019)

Notes: In all models, the sample is restricted to children of unmarried women aged 19 or above with a high school degree or less. Dummy variables for state, child's age and year are included in the regression. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .10$

Table A5

Representative Model of the Effect of Health Insurance Status on Health of Children Aged 1 to 8 Using NLSY Data - First Column of Table 5

Dependent Variable: Child Had Illnesses that Required Medical Attention in the Past Year	
Percent Time Covered by Medicaid	-0.013 (0.044)
Percent Time Covered by Private Health Insurance	0.051 (0.053)
Birth Weight	0.0003 (0.0006)
Black	-0.082 (0.041)
Hispanic	-0.243** (0.037)
Female	-0.062* (0.025)
Mother's Age	-0.028 (0.074)
Mother's Age-Squared	0.0006 (0.0013)
Mother Has Completed High School	0.072** (0.026)
Mother Has Health Limitations	0.071 (0.052)
Mother Has Never Been Married	0.006 (0.028)
Residence in Central City	0.136+ (0.037)
Average Family Wage ($1000)	0.0005 (0.0020)
Residence in Urban Area	0.025 (0.031)
Residence in North East Region	0.058 (0.050)
Residence in North Central Region	0.007 (0.044)
Residence in South Region	0.014 (0.043)

Notes: In all models, the sample is restricted to children of unmarried women aged 19 or above with a high school degree or less. Dummy variables for year and child's age are included in the regression. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .10$

Essay III

Did Medicaid Expansions Affect Labor Supply Decisions of Female Heads?

A. Introduction

Historically, publicly subsidized health insurance coverage was available only to individuals receiving welfare. Beginning in the mid-1980s, the link between Aid to Families with Dependent Children (AFDC) and Medicaid was severed by significant expansions in the Medicaid program. With the legislation contained in OBRA-89, states were required to cover pregnant women and children with family incomes below 133 percent of poverty level. Furthermore, by 1992 many states extended their benefits up to 185 percent of poverty level for pregnant women. The proportion of pregnant women and children receiving Medicaid increased by over 100 percent as eligibility for the public program expanded between the years 1987 and 1992 (Currie and Gruber 1996b).

Medicaid expansions, in addition to providing new opportunities for health insurance coverage, may have potentially affected labor supply and welfare participation decisions of female heads (Yelowitz 1995b). In particular, women who were initially receiving AFDC benefits had the option to leave welfare, increase their labor supply, and keep publicly subsidized health insurance coverage for their children. As women on welfare availed themselves of these opportunities, the number of AFDC recipients would decrease significantly, resulting in lower poverty rates and less financial burden on local and federal governments.

Given the importance of this issue it is surprising that there is only one prior study that examined the effect of Medicaid income eligibility expansions on labor force and AFDC participation (Yelowitz 1995b). In this study, the author found that recent expansions in the Medicaid program were associated with lower welfare rolls and higher labor force participation rates for unmarried women with children. These findings are promising, for they suggest that the

Medicaid expansions encouraged women to leave welfare for work. The impact of Medicaid expansions on women's labor supply is, however, more complex and needs to be analyzed with further considerations.

In this paper, I build upon and extend the previous research on the effects of the Medicaid expansions on welfare participation and labor supply decisions of female heads in several ways. First, I use longitudinal data in the analysis. These data enable me to examine changes in welfare participation and labor supply decisions of the same individuals before and after the expansions. Previous research estimated these effects using repeat cross-sectional data. Moreover, I exploit the longitudinal nature of the data to control for unobserved person-specific effects that may potentially be correlated with the policy variable. Finally, I conduct separate analyses for women who were initially on welfare and those who were initially off welfare. This is an important improvement over the previous research, for labor supply responses of these two groups of women may be in opposite directions. Evaluating the effect of Medicaid expansions on labor market experiences of all female heads may obscure these different effects.

B. Review of Previous Research

Even though a wide range of studies exist on the effect of AFDC on work incentives, there is little research examining the effect of Medicaid, especially the effect of recent Medicaid expansions on labor supply decisions.[1] Medicaid was closely linked to the cash assistance program prior to the expansions and most of the previous research evaluated labor supply effects of these programs under AFDC. The earliest study that examined the impact of the Medicaid program on labor supply and AFDC participation for female heads was by Blank (1989). Using a state-specific measure of Medicaid benefit generosity for each household and individual-level

[1] For a detailed literature review of the effects of the Aid to Families with Dependent Children and the Food Stamp programs on labor supply and family structure see Moffitt (1992).

data for the year 1980, the author found little effect of Medicaid on hours of work and AFDC participation. Winkler (1991) extended the study of Blank (1989) by examining the effect of Medicaid benefit generosity on labor force participation, hours of work, and AFDC participation decisions. The author, using individual-level data for the year 1986, found that Medicaid benefit generosity had a weak negative effect on labor force participation, but no effect on hours of work and on AFDC participation for female heads.

In a related study, Moffitt and Wolfe (1992) examined the effect of the Medicaid program on labor force and AFDC participation decisions of female heads using individual-level data for the year 1986. In contrast to the two previous studies, Moffitt and Wolfe (1992) used a family-specific Medicaid variable in their analyses. The authors found a significant decrease in the employment probability and a significant increase in the likelihood of AFDC participation associated with an increase in Medicaid benefits. This highly significant effect was in contrast with estimates obtained by Blank (1989) and Winkler (1991). Moffitt and Wolfe (1992) attributed these differences to use of state-specific measures in the previous studies. The authors argued that state level variation may not be the appropriate measure to identify the effects of Medicaid, since valuation of the Medicaid program differed at the individual family level rather than at the state level.

In summary, some of the studies that analyzed the effect of Medicaid benefit generosity reported evidence of a decrease in the likelihood of labor force participation and an increase in the likelihood of welfare participation for female heads. Even though these studies examined the effect of the Medicaid program, they were unable to provide information about the effect of the Medicaid income eligibility expansions on labor force and welfare participation. The legislative changes in the mid-1980s severed the link between the cash benefit program and Medicaid, and created new incentives for individuals receiving welfare. Women who were previously on AFDC gained the opportunity to leave welfare, increase their labor supply, and continue to receive Medicaid benefits for their children. Moreover, poor and near-poor pregnant women and children who were initially off welfare gained access to Medicaid coverage.

Montgomery and Navin (1996) examined the impact of Medicaid on labor supply decisions of female heads using repeat cross-sectional data from 1988 through 1993. Unlike studies by Blank (1989), Winkler (1991), and Moffitt and Wolfe (1992), this study examined the effect of Medicaid benefit generosity on labor supply during the period of expansions. They used policy measures based on average state Medicaid expenditures in the analysis. Their measures of Medicaid benefit generosity captured both increases in the number of recipients and in the generosity of the program. The authors included controls for state effects to avoid any spurious inverse correlation between labor supply and Medicaid benefits as is suggested by Moffitt (1994) and Hoynes (1995) in evaluating the effects of public programs. Montgomery and Navin (1996) found that labor supply decisions of unmarried women were insensitive to increases in Medicaid benefit generosity.

Evaluating the effects of Medicaid benefit generosity and Medicaid income eligibility expansions on labor supply and welfare participation may indeed be quite different. Using variables that capture the increase in Medicaid eligibility thresholds from their initial AFDC level for each individual may differ substantially from using average per capita Medicaid state- level expenditure. There is only one prior study that has examined labor supply and welfare participation effects of changing the income eligibility threshold measured using individual characteristics of female heads (Yelowitz 1995b).

Yelowitz (1995b) using repeat cross-sectional data from 1989 through 1992, found that expanding Medicaid income thresholds increased the probability of employment by 0.9 percentage points and decreased the probability of AFDC participation by 1.2 percentage points for single mothers. The author included dummy variables for state, time, youngest child's age, state-time, and child's age-time, to account for unobserved economic conditions that may affect welfare and labor force participation.

The policy variable used in this analysis is defined using state, time and age of the youngest child. Using this variable in labor force and welfare participation equations to measure the effect of the Medicaid expansions may be problematic. Individuals may have younger children because of their relative preferences for leisure.[2] Thus, they may be less likely to participate in the labor force and more likely to enroll in the welfare program. The author addressed this issue using dummy variables for age of the youngest child in all regressions. Age of the youngest child, however, may be an endogenously determined variable. Expansions in the Medicaid program may indeed induce childbearing.[3] Including dummy variables for youngest child's age may not eliminate this bias. The author is unable control for these unmeasured individual factors due to use of repeat cross-sectional data in this study.

In the same study, Yelowitz (1995b) noted that Medicaid expansions may have affected labor supply decisions of women who were initially on welfare and those who were initially off welfare in opposite ways. In the subsequent analyses, however, the author evaluated all female heads without classifying women previously on welfare as a separate category. This limitation arose from use of repeat cross-sectional data. These data do not provide information on the same individual's welfare and labor force participation before and after the expansions. They rather give information on a group of individuals prior to the expansions and on another group of individuals after the expansions. For example, analyzing the effect of the expansions on labor supply decisions of all female heads may result in an underestimate of the true effect for women initially off welfare. A potential reduction in labor supplies of women initially off AFDC may obscure the increase in labor supplies of women previously on AFDC.

Overall, findings of the study by Yelowitz (1995b) were interesting, for they suggested that Medicaid expansions increased the likelihood of labor force participation and decreased the

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[2] Children are consumption goods produced with non-market time, thus individuals with relative preferences for leisure are more likely to have younger children (Becker 1981).

[3] Findings of the previous research on the effect of the Medicaid program on childbearing suggested that there may be associations between fertility and Medicaid (Schultz 1994; Joyce and Kaestner 1996).

likelihood of AFDC participation. Additional research on the issue is warranted, because the inability to control for unobserved person-specific effects and the inability to distinguish between women previously on welfare and those who were off welfare may have plagued estimates of this study.

The novel aspect of my research is use of longitudinal data to analyze the effect of Medicaid expansions on labor supply and AFDC participation decisions. This type of data has several advantages over repeat cross-sectional data. First, using observations from the same individuals over time enables me to control for unmeasured person-specific factors that may simultaneously affect welfare and labor force participation and the policy relevant variables. Second, longitudinal nature of the data permits me to determine separate group of individuals depending on their welfare statuses prior to the expansions. It is possible to identify, for example, whether an individual who was on welfare in 1989, left AFDC and participated in the labor force in 1992. This is an important improvement over the past research. It allows me to test the validity of the theoretical discussions on the different effects of Medicaid on labor supply decisions of women who were initially on welfare and those who were initially off welfare.

Another distinguishing aspect of this study is that, in addition to labor force participation, I use hours of work as one of the measures to determine the effect of Medicaid expansions on labor supply decisions for female heads. As discussed by Yelowitz (1995b), expanding Medicaid income eligibility thresholds may affect hours of work decisions, but not the labor force participation of women who were initially working.

C. Research Design and Methods

1. Theoretical Model

Medicaid expansions, by eliminating the link between the cash assistance and public health insurance programs, created new labor market incentives for single women with children.

As is discussed by Yelowitz (1995b), the expansions may have induced an increase in labor supply and a decrease in AFDC participation for female heads who were initially on welfare. Prior to the Medicaid expansions, as women on welfare began to work, their AFDC benefits would be taxed away, and as they increased their hours of work further, they would loose their AFDC and Medicaid benefits. Yelowitz (1995b) argued that women who were initially on welfare and working may increase their hours of work, and those who were on welfare and not working may participate in the labor force after the expansions. Thus, expanded eligibility in Medicaid may induce a reduction in the AFDC participation and an increase in labor supply, since these women were given the opportunity to work, and at the same time to keep subsidized health insurance coverage for their children.

Besides these positive aspects of the Medicaid expansions for women initially on welfare, Yelowitz (1995b) discussed some possible effects of the public program on female heads who were initially off welfare. He argued that women may decrease their hours of work upon gaining eligibility in Medicaid after the expansions. The simple argument was that the Medicaid expansions created an income effect by making near-poor children eligible for the public program. The theoretical effect of the expansions is, however, more complex for women who were initially off welfare. The increase in the Medicaid income eligibility thresholds reduced the price of child health services for women who were previously ineligible for the public program. The net effect of the expansions on labor supply for this group may not be a decrease in hours of work.

It is possible to analyze potential labor supply effects of a price change in child health services with a model that incorporates child health in parental utility function. The analytical framework for this group of women is motivated by Becker's (1981) model of the demand for children. The Becker (1981) model introduced children as a commodity that cannot be purchased, but is instead produced with market and non-market inputs. I use this model to incorporate child's quality into parental utility, where quality is measured with good health. On that account, parental utility at period t is a function of child health (H) and other consumption goods (X):

(1) $U_t = U(H_t, X_t)$

Child health is produced using medical services (M) and parental time-input allocated for child health (N) in each period t. The health production function takes the following form:

(2) $H_t = f(M_t, N_t; \theta)$

Note that in Equation (2), child health is also a function of unobservable factors (θ) such as child's initial health endowment. Other consumption goods are, similarly, produced using purchased goods and services (C) and time allocated to non-market activities (Z):

(3) $X_t = g(C_t, Z_t)$

Letting (T) total time available and (L) time allocated to market work, the time constraint that each individual faces in period t is:

(4) $T_t = L_t + N_t + Z_t$

The budget constraint at any period t is a function of unearned income (V), wage (w), purchased goods and services (C), medical services for child (M), and the price for medical services (π):

(5) $C_t + \pi_t M_t = V_t + w_t L_t$

The price of purchased goods and services is normalized to one for simplicity.

The consumer's optimization problem is to choose N_k, M_k, C_k, and Z_k, $k = t, \ldots, T$, to maximize the expected present discounted value of utility, subject to Equations (1) through (5) and to non-negativity of the choice variables. The consumer will choose a new optimal plan each period due to new information coming in each period. It is assumed that the consumer operates in an environment of perfect certainty, and that at the beginning of period t the consumer knows the values of all exogenous variables dated t or earlier. Solving the maximization problem yields the following demand function for hours of market work:

(6) $L_t = L(w_t, \pi_t, \lambda)$

where π indicates the current price of child health services, w indicates the current price of parental time input, and λ indicates marginal utility of lifetime wealth. This demand function is referred as marginal utility of wealth (λ) constant demand function (MaCurdy 1981). It reveals a life-cycle view, and is preferred over a static labor supply function. Static models usually include unearned income as an exogenous variable in the analysis. Property income, for example, may be difficult to distinguish from work related transfer payments in the data available to the researcher. Unearned income may also be endogenous to labor supply decisions because of differences in individual taste for asset accumulation. In the marginal utility of wealth constant demand functions, however, except for the value of current wage, λ is embedded with all the information about lifetime wages and assets that is necessary for the consumer to determine his or her optimal current labor supply (MaCurdy 1981).

In the above specification, Medicaid expansions enter the model through their effects on the price of child health services for individuals who were initially off welfare. Expanding the Medicaid income thresholds reduces the price of health insurance coverage for women who were previously paying for their children's insurance coverage either through out-of-pocket expenses or through lower wages.[4] A reduction in the price for child health services has an ambiguous effect on hours of work decisions for these women.[5] First, hours of work may increase due to a substitution effect in health production. A lower price for medical services results in medical care being less expensive compared to the time component of health production. Thus, a substitution occurs from time input towards medical care leading to a decrease in time allocated to child's health production and an increase in market and/or non-market work.

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[4] For the discussion on the presence of a relationship between lower wages and health insurance costs see Cutler and Madrian (1996), Gruber (1994), Feldman (1993), and Summers (1989).

[5] In the current context, all price and wage changes are assumed to be anticipated, so there is no wealth effect.

The second effect of a lower price of child health services is substitution in consumption. After the expansions, child health became relatively cheaper to produce compared to other consumption goods. Thus, there is a substitution towards child quality and away from other consumption. If child health is relatively more time intensive than other commodities, then as health production increases, time allocated to produce health will increase and time allocated to market and non-market activities will decrease.[6] As a conclusion, the Medicaid expansions by lowering the price of child health services give rise to an ambiguous effect on hours of work decisions for women who were initially off welfare. The net effect will depend whether substitution in production is larger than substitution in consumption.

As is discussed by Yelowitz (1995b), however, a reduction in the price of child health services is unlikely to have an effect on labor force participation of women initially off welfare. These women always had the option of not working and obtaining Medicaid through the AFDC program. Thus, a lower price for medical services is unlikely to push women initially off welfare out of the labor force.

2. Empirical Model and Econometric Considerations

The specific functional form of Equation (6) incorporates an unobserved term, λ, which is a function of lifetime wages and assets. Following Browning, Deaton, and Irish (1985), MaCurdy (1981), and Heckman and MaCurdy (1980), this term can be specified in an additive fashion and be eliminated using the fixed-effects estimator. The empirical demand function is linear in its parameters and is specified by the following model:

(6a) $$LFP_t^* = \alpha_0 + \alpha_1 \ln w_t + \alpha_2 \pi_t + \alpha_3 Z_t + \alpha_4 \ln \lambda + e_t$$

where LFP_t^* is a latent underlying labor force participation variable, w_t is wage rate, π_t is the

[6] Following Grossman (1972), I will assume that health is a relatively more time intensive good compared to other consumption goods.

price of medical services for children, Z_t is a vector of personal characteristics, including age, education, race, number and ages of children, family size, and residence in an urban area, the α 's are parameters to be estimated, and e_t is a random error term.

Actual labor force participation is observed as a discrete outcome:

$$\text{(6b)} \quad LFP_t = \begin{cases} 1, & if \quad LFP_t^* \geq 0 \\ 0, & otherwise \end{cases}$$

In this study, Equation (6b) is estimated to measure the effect of Medicaid expansions on labor force participation decisions of female heads. Unfortunately, it is not possible to estimate all the parameters of this structural equation, for the price of child health services is in general unobserved. Past studies replace these prices with dummy variables that indicate health insurance status of the child (see second essay). Health insurance status, however, may be endogenously determined. A woman with relatively higher preferences for leisure, for example, may be more likely to participate in Medicaid. Expanded Medicaid income eligibility thresholds may induce her to have more children which in turn increases her probability of Medicaid eligibility and Medicaid participation. Because of the potential endogeneity of health insurance status, similar to the measure used by Yelowitz (1995b), I will include a variable that measures the increase in the Medicaid income eligibility thresholds relative to their initial AFDC level as an approximation for the price of medical services.

A last econometric consideration in regard to estimation of Equation (6b) is related to wage income. In analytical terms, wages may be treated as independent of the individual's choices. This, however, does not mean that wages should be treated exogenous in statistical terms. Indeed, wages may well be correlated with unobserved factors that affect labor supply. Especially, wages may be endogenous in a statistical sense, since they are calculated using annual hours of work. Assuming that wage earnings are a function of individual characteristics, I replace the wage variable with its determinants. In this study, woman's age and education will be used as the primary determinants of individual's wage. These measures are assumed to permit for the implicit effect of the wages on the labor supply.

D. Income Eligibility Expansions of the Medicaid Program

Medicaid is a federal-state matching program providing medical assistance for (1) low-income aged, blind, or disabled individuals, (2) members of families with dependent children receiving welfare, and (3) low-income pregnant women and children. In the 1980s, Medicaid eligibility was greatly expanded for the third group. Starting with the Deficit Reduction Act of 1984 (DEFRA), Congress entitled pregnant women, who met AFDC income and resource standards, in two-parent families with an unemployed principal breadwinner to Medicaid coverage.[7] The Consolidated Omnibus Budget Reconciliation Act of 1985 (COBRA) further required the eligibility of pregnant women in two-parent families with an employed principal breadwinner.

With the Omnibus Budget Reconciliation Act of 1986 (OBRA 86), for the first time, states had the option to sever the historical tie between AFDC and Medicaid by expanding Medicaid eligibility to pregnant women and infants with family income below 100 percent of Federal poverty level (FPL). One year later, Omnibus Reconciliation Act of 1987 (OBRA 87) optionally expanded Medicaid coverage to pregnant women and infants with family income below 185 percent of FPL. Omnibus Reconciliation Act of 1989 (OBRA 89) required states to offer Medicaid coverage to pregnant women and children under age 6 born after September 30,

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[7] To be eligible for AFDC payments, a family must pass a gross income test and a countable income test. A family with gross income exceeding 185 percent of the State's need standard are ineligible for AFDC. Additionally, a family's gross income less certain disregards, countable income, must be less than State's need standard. Disregards are monthly child care costs up to \$175 (\$160 between 1984 and October 1, 1989) per child, and a standard earned income allowance of \$90 (\$75 between 1984 and October 1, 1989) per month. AFDC applicants meeting the income criteria must also pass a resource test. The value of a family's property cannot exceed \$1,000 per family unit.

1983 with family incomes below 133 1/3 of FPL. Finally, Omnibus Reconciliation Act of 1990 (OBRA 90) extended coverage for children under age 19 who were born after September 30, 1983 with family incomes below 100 percent of FPL.

In summary, beginning in 1984, Congress extended Medicaid coverage to low-income pregnant women and children with no ties to the AFDC program, and raised the eligibility income thresholds up to 185 percent of FPL. As a result, in 1991, all states had Medicaid eligibility thresholds at or above 133 percent of FPL. The legislative changes that took place nationwide aimed to ease access to health care for those with no health insurance coverage. An indirect response, however, was potential changes in labor supply decisions of poor and near-poor women with children. Medicaid expansions offered new opportunities for female heads who were initially receiving welfare and those who were off welfare.

E. Data

The data used in this analysis are the National Longitudinal Survey of Youth (NLSY) for the years 1989 and 1992. The longitudinal nature of the data provides a unique opportunity to examine the effect of Medicaid expansions on changes in labor supply and welfare participation decisions for female heads. The NLSY samples 6,283 young women who were born between 1957 and 1965, and who have been interviewed yearly since 1979 (Center for Human Resources Center 1994). It contains detailed information on labor force participation, hours of work, and welfare participation. For the purposes of this study, the sample is restricted to single women with at least one child under age 18 at the time of the 1989 interview. Descriptive statistics for the selected sample are reported in the appendix.

Dependent variables are selected out of a wide range of questions on labor supply and welfare participation that exist in the NLSY. These measures are available both at the time of the interview and in the last calendar year. Labor force and AFDC participation in last calendar year are used in the analyses that serve as a benchmark against the results of the previous studies. In other analyses, the number of hours worked and labor force participation during the week of the survey, and AFDC participation in the month of the survey, are used as dependent variables throughout the analysis.

It is more advantageous to use labor force and AFDC participation at the time of the interview than in the last calendar year. Medicaid rules differed substantially during the late 1980s and the beginning of the 1990s from one month to another. A two-year old child who lives in Ohio, for example, was subject to Medicaid expansions twice within a six-month period in 1990.[8] Thus using previous year's labor force and AFDC participation rates, to measure the effect of Medicaid expansions that often varied from month to month, may confound estimation results.[9]

Measuring Medicaid Expansions

In repeat cross-sectional data analyses, the effect of Medicaid income eligibility expansions is measured with a variable similar to that is used by Yelowitz (1995b). Age of the youngest child and state-established Medicaid rules are used as the main criteria to assign Medicaid eligibility thresholds to each female head. A value for the existing AFDC threshold and a value for the expanded Medicaid income threshold are assigned to each woman based on their state of residence and age of their youngest child. Then, the difference between the Medicaid income threshold and the AFDC income threshold is calculated and used as the policy variable (**ΔMEDICAID**) in this analysis. For individuals who live in non-expansion states or for individuals with children whose age are above the program cutoff, this measure takes the value of

[8] In January 1990, children at age 2 living with families who had incomes below 100 percent of poverty level were eligible for Medicaid. In April 1990, the Medicaid income eligibility threshold was raised to 133 percent of poverty level for children under age 6 (Hill 1992).

[9] Yelowitz (1995b) also acknowledges using labor force and welfare participation that corresponds to the previous year as a shortcoming in *footnote 14* of his paper.

zero.[10]

In longitudinal data analyses, the policy variable is redefined to measure the increase in Medicaid income thresholds from 1989 to 1992. If the individual lives in a state that expanded Medicaid thresholds and if the youngest child's age is below the program cutoff, then this person will be assigned to the **experimental** group. On the other hand, if state that the woman lives in did not expand Medicaid thresholds for the age group of her youngest child, then that woman will be assigned in a **non-experimental** group. Finally, if there was a decrease in the Medicaid income threshold assigned to a woman from 1989 to 1992 because her child grew older and lost eligibility for the public program, then that person will be in a non-experimental group. The following example will illustrate experimental and non-experimental groups more clearly.

Consider three unmarried women, A, B, and C, living in Massachusetts with one child in 1989. In 1989, woman A has a 2 years old child and the Medicaid income threshold assigned to her is 100 percent of poverty level. In 1992, the child is 5 years old and the Medicaid income threshold assigned to her increases to 133 percent of poverty level. Woman A is in the experimental group. In 1989, woman B has a 4 years old child and the Medicaid income threshold assigned to her is 100 percent of poverty level. In 1992, the child is 7 years old. Since Massachusetts expanded Medicaid eligibility only for children under 6 years old, the Medicaid

[10] The mean difference between Medicaid and AFDC income eligibility thresholds assigned to each individual are quite different in the study by Yelowitz (1995b), 0.0337, and in this study, 0.3208. Even though Medicaid income thresholds used to determine eligibility were the same in both studies, computation of AFDC income eligibility thresholds differed substantially. Yelowitz (1995b) included additional sources of income (e.g. child care) in the calculation of AFDC income limit that increased mean AFDC income eligibility threshold and decreased the mean difference between Medicaid and AFDC income eligibility thresholds (see Appendix B in Yelowitz's (1995b) study for the construction of the AFDC income eligibility limits). In this study, I used maximum AFDC benefits to calculate AFDC income thresholds assuming no other income such as child care and work expenses. It is, however, noteworthy that among AFDC recipients only a small percentage are working. Moffitt (1992), for example, reported that only 6 percent of all AFDC female heads worked in 1987, and only 33 percent of those who were working worked full-time. Thus, AFDC income thresholds calculated with additional sources of income may be overestimates of the true thresholds, since a majority of women on AFDC may not have child care and work expenses.

income threshold assigned to woman B decreases to its AFDC level, 87 percent of poverty level. Woman B is in the non-experimental group. In 1989,woman C has a 9 years old child and the Medicaid income threshold assigned to her is 87 percent of poverty level. In 1992, the child is 12 years old and the Medicaid income threshold assigned to her is still 87 percent of poverty level. Woman C is in the non-experimental group.

As the above example indicates experimental and non-experimental groups are used to identify individuals who would be affected by the Medicaid expansions from 1989 to 1992. This procedure allows me to determine changes in labor supply and welfare participation that occurred due to the expansions in the Medicaid program and changes that occurred due to other unobserved factors unrelated to the expansions. In sum, experimental and non-experimental groups will be used to measure the effect of the Medicaid expansions on labor supply and welfare participation decisions of female heads in the analyses that exploit the longitudinal nature of the data.

F. Empirical Analysis and Results

1. A Comparison of Estimates with Previous Studies

I begin this section by comparing estimates from the NLSY data used in this study and the CPS data used in the study by Yelowitz (1995b), the only previous research on the effect of Medicaid eligibility expansions on labor supply to my knowledge. The purpose of this comparison is to provide a benchmark against estimates of the previous study. Table 1 represents logit estimates of the effect of Medicaid expansions on changes in labor force and AFDC participation rates for female heads.[11] In order to control for unobserved factors that may affect

[11] The purpose of using a logistic distribution is to compare the results of this analysis to the fixed effects logit model represented in Table 2. Contrary to the logit model, the probit model does not lend itself to the fixed effects specification. This makes it a little harder to compare the results of this study to those of Yelowitz (1995b) who employed a probit model in the analysis.

labor force and AFDC participation, dummy variables for time, state, state-time interaction, age of the youngest child, and age-year interaction are included in the analysis. Labor force and AFDC participation in the year prior to the interview are used as dependent variables in this analysis.

Estimates of the policy variable from the first of row of Table 1 indicate that the Medicaid expansions had no significant effect on labor supply and AFDC decisions of female heads, although the estimate of the effect of labor force participation is positive and the estimate of the effect of AFDC participation is negative. These results are imprecise compared to highly significant estimates obtained by Yelowitz (1995b), while the sign of the coefficients are the same in both analyses indicating an increase in labor force participation and a decrease in AFDC participation as a response to the Medicaid expansions.

In order to compare the magnitude of the estimates from the two studies, I also used linear probability models.[12] Yelowitz (1995b) reported estimates of the linear probability models for labor force and welfare participation in columns (1) and (3) of Table VII. The coefficient of the policy variable is 0.2444 in the labor force equation and -0.3527 in the welfare participation equation. These estimates reflect changes in labor force and welfare participation in response to a 100 percentage point increase in the difference between Medicaid and AFDC eligibility thresholds. The mean value of this policy variable in Yelowitz's (1995b) study was 0.0337. Yelowitz (1995b) focuses on the effect of what he refers to as the fully phased-in expansion, or a six percentage point increase in relative Medicaid eligibility. In this case, the probability of labor force participation would increase by 0.0147 percentage points and the probability of welfare participation would decrease by 0.0212 percentage points. Note that a six percentage point increase in relative Medicaid eligibility is a large change, twice the mean for the sample.

[12] Another method to compare the magnitude of the estimates from the two studies would be by obtaining marginal effect of the policy variable on welfare and labor force participation. Yelowitz (1995b), however, does not report marginal effects of the estimates.

In the current analysis, estimates of the effect of increasing Medicaid eligibility thresholds above those of AFDC indicate that a 100 percentage point increase would change the probability of labor force participation by 0.072 and the probability of welfare participation by -0.046. In this study, however, the mean difference between Medicaid and AFDC eligibility is 0.3208: much larger than the mean value of Yelowitz's (1995b) measure. In my analysis, the fully phased-in expansion implies a 46 percentage point increase in the relative Medicaid eligibility threshold. A change of this magnitude in Medicaid eligibility would increase the probability of labor force participation by 0.0336 and would decrease the probability of welfare participation by 0.0214. This exercise indicates that estimates of the effect of Medicaid expansions on labor force and welfare participation are indeed similar in both studies. Significance of the point estimates of this study are, however, smaller than those reported in Yelowitz's (1995b) study. The difference in the significance level of the estimates may potentially be related to the small sample size, approximately 2,100, used in this analysis compared to a sample size of about 16,000 used in the study by Yelowitz (1995b).

As an additional analysis, I use fixed-effects logit model to measure the effect of Medicaid expansions on labor force and AFDC participation decisions of female heads. For cases where the dependent variable is dichotomous, Chamberlain (1980) suggested to construct a conditional likelihood function that uses only observations that change over time and to estimate a logit model with those observations.* The advantage of this procedure is that it enables me to account for the unobserved person-specific factors that may potentially be correlated with some of the explanatory variables. Its disadvantage, however, is the small sample size arising from deleting observations that do not vary over time. This approach is used to test whether including dummy variables for the youngest child age fully accounts for unmeasured individual factors that may affect both welfare and labor force participation and youngest child's age. Since age of the youngest child is one the determinants of Medicaid income eligibility measure, its possible endogeneity may plague the estimates of this policy variable.

Table 2 represents estimates of fixed-effects logit model for labor force and AFDC participation using a sample of female heads for the years 1989 and 1992. Estimates of the change in Medicaid income eligibility thresholds are positive and approach commonly accepted level of significance (p=.19) in the labor force participation equation. The estimate of the same variable, however, is considerably insignificant and positive in the AFDC participation equation. In addition, estimates for most of the other explanatory variables are insignificant, or the variables themselves are omitted due to no within-group variance in the model. A large reduction in the significance level of the explanatory variables as a group raises doubts about the validity of the model. The weakness of this analysis is mostly related to the considerably small sample size.

As is discussed in the data section, labor force and AFDC participation corresponding to last calendar year may not be the relevant measures for this analysis. Determining the effect of a contemporary variable (e.g., Medicaid eligibility based on child's age) on past year's labor force and AFDC participation may be problematic. Changes in past year's participation rates, for example, may occur prior to the expansions and erroneously appear as the outcome of a change in the contemporary policy variable. Moreover, labor force participation measured in a large span of time (e.g., 12 months period) may overstate the actual amount of work (Heckman 1980). Finally, there may be individuals who are working while off AFDC and not working while on AFDC, within a year period. Use of yearly measures may lead to misclassification of these individuals as working while on AFDC.

Therefore labor force participation in the survey week and AFDC participation in the survey month are used as dependent variables in Tables 3 and 4. As indicated in Table 3, estimate of the change in Medicaid income eligibility thresholds became highly significant and negative in the labor force participation equation. On the other hand, estimate of the policy variable remained insignificant, and became positive. Similarly, in the fixed-effects logit models as listed in Table 4, estimate of the change in Medicaid eligibility was negative in the labor force participation equation and positive in the AFDC equation, although it was insignificant in both equations. These findings suggest that using past year's measures in identifying the effect of policy variable based on contemporaneous determinants may indeed confound estimation results.

Estimates of the effect of Medicaid eligibility on labor force participation were dramatically sensitive to the way participation was measured, although estimates of the same variable on AFDC participation were not. This may be explained with the low correlation (0.54) between two labor force participation measures compared to the relatively high correlation (0.73) between the two measures of AFDC participation.

Another interesting aspect of this comparative analysis was that except for the policy variable, estimates of most of the other variables remained unchanged. Among estimates that were sensitive to the measurement of labor force participation were those of marital status. Estimates of marital status variables became smaller indicating that indeed labor force participation rates of divorced and separated women are not dramatically higher than those of never married women. In addition, estimates of residence variables changed. The estimate of rural residence became significant and negative suggesting that individuals living in rural areas are less likely to participate in labor force than those who live in central cities. The estimate of non-central city residence became insignificant indicating that there are not large difference in labor force participation among women living in non-central and central cities. Finally, the estimate of county unemployment lost its significance suggesting that women who are temporarily in the labor force are more likely to be affected by changes in unemployment than those who hold stable jobs are.

Overall, findings of this analysis indicated that estimates of the variable that measures the effect of Medicaid expansions were highly sensitive to the way participation variables were defined. In the analyses where past year's labor force and AFDC participation were employed, there was some evidence that Medicaid expansions increased labor supply and reduced welfare rolls. Replacing these measures with the arguably more preferable contemporary labor force and AFDC participation measures changed estimation results substantially. Estimate of the effect of

Medicaid expansions on labor force participation became negative and significant. Moreover, accounting for unobserved person-specific effects eliminated the effect of the Medicaid expansions on labor force participation altogether.

As is discussed in the theoretical section of this study, however, the effect of the expansions on labor supply decisions may be overshadowed by the differential impact of the Medicaid program on women initially on welfare and women initially off welfare. Therefore, in the next section of the paper, I will examine the labor supply effects of expanding the Medicaid program on these two groups of women, separately, using the longitudinal nature of the data.

2. Differences Between Welfare Women and Women Initially off Welfare

As discussed in Section III, labor supply effects of the Medicaid expansions may be quite different for women who were initially on welfare and receiving subsidized health insurance coverage and for women who were initially ineligible for the Medicaid program. The expansions may result in an increase in the former group's labor supply, while they may have an ambiguous effect on labor supply decisions for the latter group. Therefore, analyzing labor market experiences of female heads without distinguishing between these two groups of women may obscure these different effects. Estimates obtained using this sample may, for example, underestimate of the true effect of Medicaid for women initially on welfare. The increase in their labor supply may be overshadowed by the effect of the expansions on the labor supply decisions of women initially off welfare.

To address this issue, I define two different groups of individuals based on their AFDC and labor force participation at the time of the 1989 interview. The first group is women who were receiving AFDC benefits in the survey month. The second group is women who were working at the survey week with a family income below 200 percent of poverty level and who did not participate in the welfare program in the year 1989.[13] The latter group is restricted to low-income women in order to select individuals that would potentially be eligible for the Medicaid program after the expansions.

Table 5 lists changes in AFDC and labor force participation rates for the two groups of women who were potentially affected by the Medicaid expansions.[14] As indicated in the first row, between 1989 and 1992, welfare participation decreased and labor force participation increased for women initially on welfare and not working. There was a slight increase in AFDC participation and a small decrease in labor force participation in 1992 for women off AFDC and working.

Since the years 1989 and 1992 reflect the period of the Medicaid expansions, it is possible that the above changes in AFDC and labor force participation occurred due to the expansions. Some of the results indeed confirm theoretical predictions about the effect of the expanded eligibility thresholds. For example, there was a reduction in AFDC rolls for women previously on welfare. Moreover, there was an increase in labor force participation of women who were initially on welfare in the year that corresponds to the highest expansions in the Medicaid program.

It may, however, be misleading to attribute the entire change in AFDC and labor force participation to the Medicaid expansions. There may be group-specific effects as well as

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[13] Third group is selected based on those who did not receive AFDC benefits in the year 1989 rather than at the time of the interview. The reason was to avoid any spurious effect of including labor supply changes of women who were on welfare during the year, but not at the time of the survey.

[14] Sample size in Table 5 is smaller than in Table 3 that uses same measures of AFDC and labor force participation. First, in Table 5, women with family incomes above 200 percent of poverty level (N=112) and women off AFDC and not working (N=162) are excluded from the analysis. Including the observations for this group leads to a sample size of 1778 (889×2) for 1989 and 1992 combined. Second, 1989 cross-section allows for women unmarried in 1989, but married in 1992 (N=182) to be in the sample. Similarly, 1992 cross-section allows for women married in 1989, but unmarried in 1992 (N=247) to be in the sample. Thus, there are fewer observations in longitudinal data than in repeated cross-sections.

macroeconomic conditions that affect variation in welfare and labor force participation decisions. An increase in unemployment rate, for example, may induce growth in welfare rolls and have adverse effects on labor supplies of all female heads. Including controls for macroeconomic trends may not fully solve the problem. Presence of group-specific effects may also plague estimation results. For example, women initially on welfare may be coming from families highly dependent on welfare (Duncan, Hill, and Hoffman 1988). Even if economic conditions improve for all female heads, these women may find it difficult to get off AFDC. Therefore, I use differences-in-differences-in-differences (DDD) technique to separate the effect of Medicaid expansions from group-specific effects and time-varying effects that may also affect participation decisions.

The DDD estimator exploits changes in welfare participation and labor supply between women who would be affected by the Medicaid expansions and controls to account for time-varying shocks that impact treated women and controls equally. It also exploits changes among treated women and controls in experimental versus non-experimental groups (as previously defined) to eliminate group fixed-effects. For the purposes of this study, I define two different treatment groups: women on AFDC at the time of the 1989 survey; and women off AFDC in 1989 and working with a family income under 200 percent of poverty level. The control group is women who were off AFDC in 1989 and working with a family income above 200 percent of poverty level.

The DDD procedure incorporates two strong assumptions. First, the control group is assumed to be unaffected by policy changes. This is most likely true in our context, since these individuals have incomes well above the program cutoff. Second, it is assumed that unobserved time-varying factors affect treatment and control groups similarly. This problem is also unlikely to plague the results of this study inasmuch as female heads have relatively similar labor market experiences compared to other groups of individuals in the population (e.g., man).

As explained in the data section, experimental and non-experimental groups are based on youngest child's age and state-established Medicaid rules. A woman is in an experimental group, if she is subject to an increase in Medicaid income eligibility thresholds depending on the age of her youngest child. Similarly, a woman is in a non-experimental group if she is subject to a decrease or no change in the Medicaid income eligibility thresholds.

Tables 6a through 6c list results pertaining to the effect of Medicaid expansions on AFDC, labor force participation, and hours of work for women who were on welfare at the time of the 1989 interview. A focus on the first row of each top panel indicates that between 1989 and 1992, there was a reduction in welfare participation and an increase labor supply of this group. The likelihood of AFDC participation decreased by 28.2 percentage points and the employment probability increased by 7.7 percentage points for women initially on welfare. Furthermore, these women had 3.9 more hours of work per week in 1992 than in 1989.[15] Over time changes in welfare participation and labor supply of the control group, however, were in opposite direction to those of the treatment group. The results pertaining to the control group indicate that there was indeed a recessionary trend in the economy that led to an increase in AFDC rolls and an adverse effect on labor markets for all female heads. In sum, as the differences-in-differences estimates of the top panel show, after accounting for time varying changes in welfare and labor supply decisions, there was a decrease in welfare participation and an increase in labor supply of the treatment women between 1989 and 1992.

These changes, however, may have occurred due to group-specific effects rather than due to expansions in the Medicaid program. Bottom panels of Tables 6a to 6c report changes in welfare participation and labor supply of women in non-experimental groups. After accounting for macroeconomic trends that affect all female heads, the change in welfare participation and labor supply between 1989 and 1992 were larger for treatment women in non-experimental

[15] Hours of work models include observations from individuals with zero hours of work. I repeated the same analysis for women with positive hours of work in both 1989 and 1992 only. Sample sizes, however, decreased substantially for all groups of women. Appendix tables A4 through A7 present results of this exercise.

groups than those in experimental groups. The differences-in-differences estimates suggest that there were unobservable group-specific factors that caused a decrease in welfare participation and an increase in labor supply over time. Women who were initially on welfare, for example, may be conscientious about not to have additional children and they may be leaving AFDC rolls as their children grow older (Berrick 1995).[16] Another explanation is that these women may be using the cash benefit program to overcome a period of crisis, and may be leaving it within the few years following their entry into the program (Bane and Ellwood 1983). These and other factors that are unrelated to Medicaid expansions may have led to changes in welfare participation and labor supply of women in experimental and non-experimental groups.

As a result, the DDD estimates are insignificant in both welfare and labor supply models, indicating that it was the group-specific effects that led to a reduction in AFDC participation and an increase in labor supply for women initially on welfare. Medicaid expansions did not have any significant effect on welfare and labor supply decisions of these women.

A similar analysis for female heads who were initially off welfare is reported in Tables 7a through 7c. As indicated in the first row of each top panel, the likelihood of welfare participation increased by 11.3 percentage points and the likelihood of employment decreased for 32.5 percentage points from 1989 to 1992. Moreover, these women had 13.37 less hours of work per week in 1992 compared to 1989. The results pertaining to the control group indicate the presence of a recessionary trend in the economy that led to an increase in AFDC and a decrease in employment for all female heads. The differences-in-differences estimates of the top panel show

[16] In 1989, the NLSY women were 24 to 32 years old and only about 20 percent of them had a newborn between 1989 and 1992. Therefore, one of the reasons for the reduction in welfare participation may be that the children of these women grow older over the time period making it easier for them to leave welfare. As an exercise, I analyze welfare participation and labor supply decisions of women who did not have a newborn after 1989. Appendix Tables A2a to A2c indicate no significant differences between women with and without an infant after 1989. This result may partially be related to the fact that even though these women have new children, they are more experienced with child raising and are able to allocate more time to labor market activities. Alternatively, their older children may provide child care for the newborns while their mothers work.

that the probability of AFDC participation increased by 9.1 percentage points and the probability of employment decreased by 15.8 percentage points for the treatment group from 1989 to 1992. Adverse economic conditions accounted for almost half of the decrease in labor force participation and about 20 percent of the increase in welfare participation and decrease in working hours for the treatment group.

Changes in welfare participation and labor supply decisions of women in non-experimental groups were similar to those in experimental groups. The differences-in-differences estimates are reported at each bottom panel of Tables 7a to 7c. There was an upward trend in AFDC participation and a downward trend in labor supply for the treatment group. These changes, however, were less accentuated for women in non-experimental groups than those in experimental groups were. One of the reasons of larger changes in welfare and labor supply decisions of women in experimental groups may be that expanded Medicaid eligibility had adverse effects for women initially off welfare. As women in experimental groups inquire about their Medicaid eligibility, they may also find out that they are eligible for the welfare program and may enroll in AFDC (Council of Economic Advisers 1997). Another potential explanation follows from the theoretical section of this study. As discussed previously, Medicaid expansions reduced the price of child health services for women initially off welfare, thus made health production a cheaper commodity relative to other consumption goods. This relative price decrease may have encouraged women to have additional children and may have created incentives to participate in welfare.[17]

Even though Medicaid expansions seem to have marginally affected women initially off

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[17] Appendix Tables A3a through A3c replicate the same analysis for women who did not have a newborn between 1989 and 1992. As the results of each first panel indicate Medicaid expansions may have indeed had an effect on fertility decisions of women initially off welfare. The increase in AFDC participation for women in experimental groups who did not have a newborn after 1989 was about 35 percent less than for those who had an infant after 1989. This result may provide some evidence on fertility effects that the expansions in the Medicaid program may induce. The scope of the fertility effects of the Medicaid expansions is, however, a wider and needs to be evaluated in a separate study.

welfare, these effects disappear after accounting for group-specific and time-varying factors. The DDD estimates are insignificant in all models indicating that Medicaid expansions did not have any effect on AFDC and labor supply decisions for women initially off welfare. The estimate on hours of work model, however, approaches commonly accepted significance level ($p=.15$) suggesting that Medicaid expansions may indeed have a negative effect on hours of work for these women. A result that indicates a substitution towards child health production as the price of medical services for children fell with the Medicaid expansions. Overall, results of this table confirm earlier findings in this study that Medicaid expansions had little, if any, effect on welfare participation and labor supply decisions of female heads.

G. Summary and Conclusion

In this study, I have extensively examined the effect of expanding Medicaid income eligibility thresholds on labor supply and welfare participation decisions for female heads. Findings of this study showed that estimates of the effect of Medicaid expansions on welfare and labor force participation were highly sensitive to the way these variables were measured. Especially, there is some evidence that using employment status measured in the past calendar year would overstate the effect of the policy variable on labor force participation. This study also exploited potential behavioral differences between women who were initially on welfare and women who were initially off welfare. Overall, there was very little evidence that female heads changed their labor supply and welfare participation due to the expansions in the Medicaid program. Changes in AFDC participation and labor supply for female heads from 1989 to 1992 occurred for reasons unrelated to the expansions in the Medicaid income eligibility thresholds. The results of this study did not indicate any behavioral changes in AFDC participation and labor supply decisions for female heads due to Medicaid expansions.

These findings, however, are encouraging in that the expansions did not lower employment probabilities for women initially off welfare. A potential negative aspect of the expansions would have been lower labor force participation rates and reduced hours of work which could in turn increase probability of welfare dependency in the future. Results that pertain to women initially off welfare also verify findings of the first essay, which suggested that Medicaid expansions did not crowd out private health insurance. In sum, expanded Medicaid income eligibility thresholds appear to result in increased enrollment rates of children from working families with no employment-sponsored health insurance coverage without affecting their labor supply decisions.

Achieving a decrease in welfare rolls and an increase in labor force participation of women who were initially on AFDC is a more complex issue than just offering health insurance coverage for poor children. There is need for reforms in safe and affordable child care facilities, job training programs that will make an easier transition from welfare to labor markets, support for housing facilities, reforms to persuade fathers to pay for child support in a consistent manner. Although Medicaid coverage is a very valuable benefit, it is not sufficient by itself to decrease the AFDC caseload considerably. More structural welfare reforms are necessary to help poor families to overcome AFDC dependency.

Table 1

The Effect of Medicaid Expansions on Labor Force and AFDC Participation in the Past Calendar Year
(Logit Estimates Using Pooled Data for 1989 and 1992)

	Labor Force Participation	AFDC Participation
ΔMEDICAID	0.342	-0.145
	(0.345)	(0.333)
Age	-0.686*	0.656*
	(0.419)	(0.405)
Age-Squared	0.010+	-0.011*
	(0.007)	(0.007)
High School Graduate	0.814**	-0.899**
	(0.125)	(0.125)
Some College	1.913**	-1.796**
	(0.182)	(0.171)
College Graduate or More	2.787**	-2.660**
	(0.463)	(0.389)
Education Missing	-0.522	0.704
	(0.888)	(0.929)
Black	-0.419**	0.887**
	(0.139)	(0.136)
Divorced	0.804**	-0.533**
	(0.146)	(0.136)
Separated	0.709**	-0.439**
	(0.147)	(0.140)
Widowed	0.291	-1.572**
	(0.376)	(0.474)
Number of Kids Under 6	-0.461**	0.332**
	(0.137)	(0.135)
Family Size	-0.159**	0.143**
	(0.030)	(0.030)
Residence in a Non-SMSA City	0.115	-0.165
	(0.208)	(0.200)
Residence in a Non-Central SMSA City	0.263*	-0.310*
	(0.154)	(0.152)
County Unemployment Rate	-0.087**	0.103**
	(0.034)	(0.033)
N	2141	2137
Log-Likelihood	-1072.577	-1145.639

Note: The sample is restricted to unmarried women with at least one child under age 18 in the year 1989. Dummy variables for state, year, state-year interaction, age of youngest child, age of youngest child-year interaction are included in all regressions. Standard errors are in parentheses. ** p < .01, * p < .05, + p < .1

Table 2

The Effect of Medicaid Expansions on Labor Force and AFDC Participation in the Past Calendar Year (Fixed-Effects Logit Estimates Using Pooled Data for 1989 and 1992)

	Labor Force Participation	AFDC Participation
ΔMEDICAID	1.031 (0.795)	0.388 (0.629)
Age	-0.217 (0.863)	-0.300 (0.798)
Age-Squared	0.001 (0.014)	0.004 (0.013)
High School Graduate	2.843+ (1.629)	-0.190 (1.103)
Some College	-1.016 (1.526)	-
College Graduate or More	-	-
Education Missing	-	-
Divorced	-	-
Separated	-	-
Widowed	-	-
Number of Kids Under 6	-0.244 (0.418)	0.616+ (0.349)
Family Size	-0.085 (0.096)	0.067 (0.098)
Residence in a Non-SMSA City	2.096+ (1.108)	0.100 (1.041)
Residence in a Non-Central SMSA City	-0.019 (0.656)	-0.344 (0.692)
County Unemployment Rate	-0.151 (0.098)	0.121 (0.093)
N	372	342
Log-likelihood	-98.449	-100.729

Note: The sample is restricted to unmarried women with at least one child under age 18 in the year 1989. Parameter estimates are missing for some variables because values of those variables remained unchanged over time for the selected sample. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table 3

The Effect of Medicaid Expansions on Labor Force Participation in the Survey Week and AFDC Participation in the Survey Month (Logit Estimates Using Pooled Data for 1989 and 1992)

	Labor Force Participation	AFDC Participation
ΔMEDICAID	-0.698**	0.031
	(0.313)	(0.348)
Age	-0.770*	0.693*
	(0.370)	(0.420)
Age-Squared	0.013*	-0.012*
	(0.006)	(0.007)
High School Graduate	0.833**	-0.865**
	(0.120)	(0.128)
Some College	1.333**	-1.709**
	(0.150)	(0.177)
College Graduate or More	2.237**	-3.126**
	(0.312)	(0.503)
Education Missing	-0.326	0.413
	(0.933)	(0.900)
Black	-0.473**	0.704**
	(0.122)	(0.143)
Divorced	0.303**	-0.396**
	(0.125)	(0.144)
Separated	0.274*	-0.423**
	(0.130)	(0.146)
Widowed	0.418	-1.888**
	(0.348)	(0.585)
Number of Kids Under 6	-0.423**	0.395**
	(0.143)	(0.140)
Family Size	-0.130**	0.206**
	(0.029)	(0.031)
Residence in a Non-SMSA City	-0.414**	-0.272
	(0.187)	(0.209)
Residence in a Non-Central SMSA City	0.029	-0.460**
	(0.139)	(0.158)
County Unemployment Rate	-0.005	0.098**
	(0.031)	(0.034)
N	2141	2057
Log-Likelihood	-1314.140	-1064.270

Note: The sample is restricted to unmarried women with at least one child under age 18 in the year 1989. Dummy variables for state, year, state-year interaction, age of youngest child, age of youngest child-year interaction are included in all regressions. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table 4

The Effect of Medicaid Expansions on Labor Force Participation in the Survey Week and AFDC Participation in the Survey Month (Fixed-Effects Logit Estimates Using Pooled Data for 1989 and 1992)

	Labor Force Participation	AFDC Participation
ΔMEDICAID	-0.288	0.543
	(0.558)	(0.706)
Age	-0.659	-1.293
	(0.691)	(0.872)
Age-Squared	0.014	0.017
	(0.011)	(0.014)
High School Graduate	0.780	-
	(0.965)	
Some College	-1.352	-
	(0.956)	
College Graduate or More	0.074	-
	(3.075)	
Education Missing	-	-
Divorced	0.529	-2.426
	(1.837)	(1.581)
Separated	0.708	-1.982
	(1.788)	(1.378)
Widowed	1.755	-
	(2.266)	
Number of Kids Under 6	-0.794	0.246
	(0.413)	(0.380)
Family Size	-0.111	0.101
	(0.075)	(0.088)
Residence in a Non-SMSA City	-0.554	0.189
	(0.947)	(1.152)
Residence in a Non-Central SMSA City	-0.986+	-0.022
	(0.547)	(0.699)
County Unemployment Rate	-0.221	0.390
	(0.087)	(0.114)
N	492	338
Log-likelihood	-139.251	-92.7433

Note: The sample is restricted to unmarried women with at least one child under age 18 in the year 1989. Parameter estimates are missing for some variables because values of those variables remained unchanged over time for the selected sample. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table 5

Changes in Mean AFDC Participation, Labor Force Participation, and Hours of Work of Female Heads in 1992 for Groups of Female Heads Selected Based on their 1989 Characteristics

1989	1992	
	AFDC Participation	**Labor Force Participation**
AFDC Women		
AFDC = 1	0.706	0.237
LFP = 0.135	(0.026)	(0.024)
[N=325]		
Non-AFDC Women		
AFDC = 0	0.085	0.728
LFP = 1	(0.017)	(0.026)
[N=290]		

Note: AFDC represents participation in the survey month, LFP represents labor force participation at the survey week. Non-AFDC women are restricted to unmarried individuals with family incomes less than 200 percent of poverty level in 1989. Standard error of labor force participation for AFDC women in 1989 is 0.019. Number of observations are in square-brackets. Standard errors are in parentheses.

Table 6a

The Effect of Medicaid Expansions on Mean AFDC Participation for Unmarried Women Initially On Welfare

Experimental

	1989	1992	Difference
Treatment Group	1.000	0.718	-0.282**
$AFDC_{89}$=1	(0.000)	(0.031)	(0.030)
	[221]	[216]	
Control Group	0.000	0.022	0.022
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.022)	(0.022)
$Income_{89}$ > 200% of Poverty	[47]	[46]	
	Differences-in-differences		**-0.304****
			(0.067)

Non-Experimental

	1989	1992	Difference
Treatment Group	1.000	0.683	-0.317**
$AFDC_{89}$=1	(0.000)	(0.046)	(0.046)
	[104]	[104]	
Control Group	0.000	0.032	0.032
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.022)	(0.023)
$Income_{89}$ > 200% of Poverty	[63]	[63]	
	Differences-in-differences		**-0.349****
			(0.061)
	Differences-in-differences-in-differences		**0.045**
			(0.091)

Note: Treatment group is defined based on AFDC participation in the month of the interview. Control group is defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table 6b

The Effect of Medicaid Expansions on Mean Labor Force Participation for Unmarried Women Initially On Welfare

Experimental

	1989	1992	Difference
Treatment Group	0.136	0.213	0.077*
$AFDC_{89}=1$	(0.023)	(0.028)	(0.036)
	[221]	[221]	
Control Group	1.000	0.833	-0.167**
$AFDC_{89}=0$; $LFP_{89}=1$	(0.000)	(0.054)	(0.054)
$Income_{89} > 200\%$ of Poverty	[48]	[48]	
	Differences-in-differences		**0.244****
			(0.081)

Non-Experimental

	1989	1992	Difference
Treatment Group	0.135	0.288	0.153**
$AFDC_{89}=1$	(0.034)	(0.045)	(0.056)
	[104]	[104]	
Control Group	1.000	0.921	-0.079*
$AFDC_{89}=0$; $LFP_{89}=1$	(0.000)	(0.034)	(0.034)
$Income_{89} > 200\%$ of Poverty	[63]	[63]	
	Differences-in-differences		**0.232****
			(0.077)
	Differences-in-differences-in-differences		**0.012**
			(0.113)

Note: Treatment group is defined based on AFDC participation in the month of the interview. Control group is defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table 6c

The Effect of Medicaid Expansions on Mean Hours of Work for Unmarried Women Initially On Welfare

Experimental

	1989	1992	Difference
Treatment Group	4.032	7.928	3.896**
$AFDC_{89}=1$	(0.746)	(1.072)	(1.306)
	[221]	[221]	
Control Group	38.917	35.646	-3.271
$AFDC_{89}=0$; $LFP_{89}=1$	(1.366)	(2.533)	(2.878)
$Income_{89}$ > 200% of Poverty	[48]	[48]	
	Differences-in-differences		**7.167***
			(3.107)

Non-Experimental

	1989	1992	Difference
Treatment Group	4.375	9.154	4.779*
$AFDC_{89}=1$	(1.200)	(1.651)	(2.041)
	[104]	[104]	
Control Group	43.952	38.937	-5.015*
$AFDC_{89}=0$; $LFP_{89}=1$	(1.654)	(1.879)	(2.503)
$Income_{89}$ > 200% of Poverty	[63]	[63]	
	Differences-in-differences		**9.794****
			(3.267)
	Differences-in-differences-in-differences		**-2.627**
			(4.483)

Note: Treatment group is defined based on AFDC participation in the month of the interview. Control group is defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table 7a

The Effect of Medicaid Expansions on Mean AFDC Participation for Unmarried Women Initially Off Welfare

Experimental

	1989	1992	Difference
Treatment Group	0.000	0.113	0.113**
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.026)	0.026
$Income_{89} \leq$ 200% of Poverty	[149]	[150]	
Control Group	0.000	0.022	0.022
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.022)	(0.022)
$Income_{89}$ > 200% of Poverty	[47]	[46]	
	Differences-in-differences		**0.091+**
			(0.048)

Non-Experimental

	1989	1992	Difference
Treatment Group	0.000	0.054	0.054**
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.020)	(0.020)
$Income_{89} \leq$ 200% of Poverty	[133]	[130]	
Control Group	0.000	0.032	0.032
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.022)	(0.023)
$Income_{89}$ > 200% of Poverty	[63]	[63]	
	Differences-in-differences		**0.022**
			(0.032)
	Differences-in-differences-in-differences		**0.069**
			(0.057)

Note: Treatment and control groups are defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table 7b

The Effect of Medicaid Expansions on Mean Labor Force Participation for Unmarried Women Initially Off Welfare

Experimental

	1989	1992	Difference
Treatment Group	1.000	0.675	-0.325**
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.038)	(0.038)
$Income_{89}$ ≤ 200% of Poverty	[154]	[154]	
Control Group	1.000	0.833	-0.167**
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.054)	(0.054)
$Income_{89}$ > 200% of Poverty	[48]	[48]	
	Differences-in-differences		**-0.158***
			(0.074)

Non-Experimental

	1989	1992	Difference
Treatment Group	1.000	0.784	-0.216**
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.036)	(0.036)
$Income_{89}$ ≤ 200% of Poverty	[134]	[134]	
Control Group	1.000	0.921	-0.079*
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.034)	(0.034)
$Income_{89}$ > 200% of Poverty	[63]	[63]	
	Differences-in-differences		**-0.137***
			(0.057)
	Differences-in-differences-in-differences		**-0.021**
			(0.093)

Note: Treatment and control groups are defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** p < .01, * p < .05, + p < .1

Table 7c

The Effect of Medicaid Expansions on Mean Hours of Work for Unmarried Women Initially Off Welfare

Experimental

	1989	1992	Difference
Treatment Group	39.357	25.987	-13.370**
$AFDC_{89}$=0; LFP_{89}=1	(0.801)	(1.617)	(1.804)
$Income_{89}$ ≤ 200% of Poverty	[154]	[154]	
Control Group	38.917	35.646	-3.271
$AFDC_{89}$=0; LFP_{89}=1	(1.366)	(2.533)	(2.878)
$Income_{89}$ > 200% of Poverty	[48]	[48]	
	Differences-in-differences		**-10.099****
			(3.610)

Non-Experimental

	1989	1992	Difference
Treatment Group	40.082	31.881	-8.201**
$AFDC_{89}$=0; LFP_{89}=1	(0.917)	(1.588)	(1.834)
$Income_{89}$ ≤ 200% of Poverty	[134]	[134]	
Control Group	43.952	38.937	-5.015*
$AFDC_{89}$=0; LFP_{89}=1	(1.654)	(1.879)	(2.503)
$Income_{89}$ > 200% of Poverty	[63]	[63]	
	Differences-in-differences		**-3.186**
			(3.178)
	Differences-in-differences-in-differences		**-6.913**
			(4.802)

Note: Treatment and control groups are defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table A1

Descriptive Statistics for the NLSY Data

	1989	1992
ΔMEDICAID	0.167	0.466
	(0.269)	(0.377)
AFDC Participation (Month of Interview)	0.315	0.302
	(0.465)	(0.459)
Labor Force Participation (Week of Interview)	0.509	0.523
	(0.500)	(0.500)
Hours of Work (Week of Interview)	19.966	20.619
	(21.253)	(21.283)
AFDC Participation (Last Calendar Year)	0.355	0.353
	(0.479)	(0.478)
Labor Force Participation (Last Calendar Year)	0.704	0.701
	(0.457)	(0.458)
Mother's Age	28.513	31.609
	(2.236)	(2.240)
Black	0.541	0.529
	(0.499)	(0.499)
Divorced	0.286	0.347
	(0.452)	(0.476)
Separated	0.203	0.213
	(0.402)	(0.409)
Widowed	0.015	0.024
	(0.122)	(0.154)
High School Graduate	0.510	0.505
	(0.500)	(0.500)
Some College Education	0.197	0.209
	(0.398)	(0.407)
College Graduate or More	0.032	0.044
	(0.175)	(0.206)
Education Missing	0.003	0.003
	(0.051)	(0.050)
Age of Youngest Child	5.285	6.965
	(3.461)	(4.052)
Number of Children Under Age 6	0.648	0.451
	(0.732)	(0.689)
Family Size	3.553	3.539
	(1.872)	(1.764)
Residence in a Non-SMSA City	0.207	0.206
	(0.405)	(0.405)
Residence in a Non-Central SMSA City	0.575	0.583
	(0.495)	(0.493)
County Unemployment Rate	5.602	8.000
	(1.982)	(2.355)
N	1133	1198

Note: Standard deviations are in parentheses. The sample is restricted to unmarried women with at least one child under age 18 in the year 1989.

Table A2a

The Effect of Medicaid Expansions on Mean AFDC Participation for Unmarried Women Initially On Welfare With No Newborns After 1989

Experimental

	1989	1992	Difference
Treatment Group	1.000	0.698	-0.302**
$AFDC_{89}$=1	(0.000)	(0.039)	(0.039)
	[140]	[139]	
Control Group	0.000	0.029	0.029
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.029)	(0.029)
$Income_{89}$ > 200% of Poverty	[35]	[35]	
		Differences-in-differences	**-0.331****
			(0.079)

Non-Experimental

	1989	1992	Difference
Treatment Group	1.000	0.680	-0.320**
$AFDC_{89}$=1	(0.000)	(0.046)	(0.046)
	[103]	[103]	
Control Group	0.000	0.032	0.032
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.022)	(0.022)
$Income_{89}$ > 200% of Poverty	[63]	[63]	
		Differences-in-differences	**-0.352****
			(0.062)
		Differences-in-differences-in-differences	**0.021**
			(0.100)

Note: Treatment group is defined based on AFDC participation in the month of the interview. Control group is defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table A2b

The Effect of Medicaid Expansions on Mean Labor Force Participation for Unmarried Women Initially On Welfare With No Newborns After 1989

Experimental

	1989	1992	Difference
Treatment Group	0.136	0.257	0.121*
$AFDC_{89}$=1	(0.029)	(0.037)	(0.047)
	[140]	[140]	
Control Group	1.000	0.886	-0.114*
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.055)	(0.055)
$Income_{89}$ > 200% of Poverty	[35]	[35]	
	Differences-in-differences		**0.235***
			(0.098)

Non-Experimental

	1989	1992	Difference
Treatment Group	0.136	0.291	0.155**
$AFDC_{89}$=1	(0.034)	(0.045)	(0.056)
	[103]	[103]	
Control Group	1.000	0.921	-0.079*
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.034)	(0.034)
$Income_{89}$ > 200% of Poverty	[63]	[63]	
	Differences-in-differences		**0.234****
			(0.077)
	Differences-in-differences-in-differences		**0.001**
			(0.124)

Note: Treatment group is defined based on AFDC participation in the month of the interview. Control group is defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table A2c

The Effect of Medicaid Expansions on Mean Hours of Work for Unmarried Women Initially On Welfare With No Newborns After 1989

Experimental

	1989	1992	Difference
Treatment Group	4.186	9.600	5.414**
$AFDC_{89}$=1	(0.955)	(1.441)	(1.728)
	[140]	[140]	
Control Group	38.514	38.314	-0.200**
$AFDC_{89}$=0; LFP_{89}=1	(1.792)	(2.717)	(3.255)
$Income_{89}$ > 200% of Poverty	[35]	[35]	
	Differences-in-differences		**5.614**
			(3.822)

Non-Experimental

	1989	1992	Difference
Treatment Group	4.417	9.243	4.826*
$AFDC_{89}$=1	(1.211)	(1.665)	(2.059)
	[103]	[103]	
Control Group	43.952	38.937	-5.015*
$AFDC_{89}$=0; LFP_{89}=1	(1.654)	(1.879)	(2.503)
$Income_{89}$ > 200% of Poverty	[63]	[63]	
	Differences-in-differences		**9.841****
			(3.281)
	Differences-in-differences-in-differences		**-4.227**
			(5.042)

Note: Treatment group is defined based on AFDC participation in the month of the interview. Control group is defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table A3a

The Effect of Medicaid Expansions on Mean AFDC Participation for Unmarried Women Initially Off Welfare With No Newborns After 1989

Experimental

	1989	1992	Difference
Treatment Group $AFDC_{89}$=0; LFP_{89}=1 $Income_{89} \leq 200\%$ of Poverty	0.000 (0.000) [104]	0.075 (0.026) [107]	0.075** (0.026)
Control Group $AFDC_{89}$=0; LFP_{89}=1 $Income_{89} > 200\%$ of Poverty	0.000 (0.000) [35]	0.029 (0.029) [35]	0.029 (0.029)
	Differences-in-differences		**0.046** **(0.048)**

Non-Experimental

	1989	1992	Difference
Treatment Group $AFDC_{89}$=0; LFP_{89}=1 $Income_{89} \leq 200\%$ of Poverty	0.000 (0.000) [133]	0.054 (0.020) [130]	0.054** (0.020)
Control Group $AFDC_{89}$=0; LFP_{89}=1 $Income_{89} > 200\%$ of Poverty	0.000 (0.000) [63]	0.032 (0.022) [63]	0.032 (0.022)
	Differences-in-differences		**0.022** **(0.032)**
	Differences-in-differences-in-differences		**0.024** **(0.056)**

Note: Treatment and control groups are defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table A3b

The Effect of Medicaid Expansions on Mean Labor Force Participation for Unmarried Women Initially Off Welfare With No Newborns After 1989

Experimental

	1989	1992	Difference
Treatment Group	1.000	0.706	-0.294**
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.044)	(0.044)
$Income_{89} \leq$ 200% of Poverty	[109]	[109]	
Control Group	1.000	0.886	-0.114*
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.055)	(0.055)
$Income_{89}$ > 200% of Poverty	[35]	[35]	
	Differences-in-differences		**-0.180***
			(0.083)

Non-Experimental

	1989	1992	Difference
Treatment Group	1.000	0.784	-0.216**
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.036)	(0.036)
$Income_{89} \leq$ 200% of Poverty	[134]	[134]	
Control Group	1.000	0.921	-0.079*
$AFDC_{89}$=0; LFP_{89}=1	(0.000)	(0.034)	(0.034)
$Income_{89}$ > 200% of Poverty	[63]	[63]	
	Differences-in-differences		**-0.137***
			(0.057)
	Differences-in-differences-in-differences		**-0.043**
			(0.098)

Note: Treatment and control groups are defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** p < .01, * p < .05, + p < .1

Table A3c

The Effect of Medicaid Expansions on Mean Hours of Work for Unmarried Women Initially Off Welfare With No Newborns After 1989

Experimental

	1989	1992	Difference
Treatment Group	40.110	27.349	-12.761**
$AFDC_{89}$=0; LFP_{89}=1	(0.926)	(1.936)	(2.146)
$Income_{89} \leq$ 200% of Poverty	[109]	[109]	
Control Group	38.514	38.314	-0.200**
$AFDC_{89}$=0; LFP_{89}=1	(1.792)	(2.717)	(3.255)
$Income_{89}$ > 200% of Poverty	[35]	[35]	
	Differences-in-differences		**-12.561****
			(4.215)

Non-Experimental

	1989	1992	Difference
Treatment Group	40.082	31.881	-8.201**
$AFDC_{89}$=0; LFP_{89}=1	(0.917)	(1.588)	(1.834)
$Income_{89} \leq$ 200% of Poverty	[134]	[134]	
Control Group	43.952	38.937	-5.015*
$AFDC_{89}$=0; LFP_{89}=1	(1.654)	(1.879)	(2.503)
$Income_{89}$ > 200% of Poverty	[63]	[63]	
	Differences-in-differences		**-3.186**
			(3.178)
	Differences-in-differences-in-differences		**-9.375+**
			(5.236)

Note: Treatment and control groups are defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** p < .01, * p < .05, + p < .1

Table A4

The Effect of Medicaid Expansions on Mean Hours of Work for
Unmarried Women Initially On Welfare With Non-zero Hours of Work in 1989 and 1992

Experimental

	1989	1992	Difference
Treatment Group	29.100	40.300	11.200*
$AFDC_{89}=1$	(3.822)	(1.317)	(4.043)
	[10]	[10]	
Control Group	38.975	42.775	3.800*
$AFDC_{89}=0$; $LFP_{89}=1$	(1.259)	(1.209)	(1.746)
$Income_{89} > 200\%$ of Poverty	[40]	[40]	
		Differences-in-differences	**7.400+**
			(4..026)

Non-Experimental

	1989	1992	Difference
Treatment Group	36.000	47.333	11.333
$AFDC_{89}=1$	(4.008)	(6.566)	(7.693)
	[6]	[6]	
Control Group	44.121	42.293	-1.828
$AFDC_{89}=0$; $LFP_{89}=1$	(1.783)	(1.296)	(2.204)
$Income_{89} > 200\%$ of Poverty	[58]	[58]	
		Differences-in-differences	**13.161+**
			(7.274)
		Differences-in-differences-in-differences	**-5.761**
			(8.220)

Note: Treatment group is defined based on AFDC participation in the month of the interview. Control group is defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table A5

The Effect of Medicaid Expansions on Mean Hours of Work for Unmarried Women Initially Off Welfare With Non-zero Hours of Work in 1989 and 1992

Experimental

	1989	1992	Difference
Treatment Group	40.019	38.481	-1.538
$AFDC_{89}=0$; $LFP_{89}=1$	(0.983)	(1.040)	(1.431)
$Income_{89} \leq 200\%$ of Poverty	[104]	[104]	
Control Group	38.975	42.775	3.800*
$AFDC_{89}=0$; $LFP_{89}=1$	(1.259)	(1.209)	(1.746)
$Income_{89} > 200\%$ of Poverty	[40]	[40]	
	Differences-in-differences		**-5.338***
			(2.551)

Non-Experimental

	1989	1992	Difference
Treatment Group	40.210	40.686	0.476
$AFDC_{89}=0$; $LFP_{89}=1$	(1.097)	(0.820)	(1.369)
$Income_{89} \leq 200\%$ of Poverty	[105]	[105]	
Control Group	44.121	42.293	-1.828
$AFDC_{89}=0$; $LFP_{89}=1$	(1.783)	(1.296)	(2.204)
$Income_{89} > 200\%$ of Poverty	[58]	[58]	
	Differences-in-differences		**2.304**
			(2.465)
	Differences-in-differences-in-differences		**-7.642***
			(3.579)

Note: Treatment and control groups are defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table A6

The Effect of Medicaid Expansions on Mean Hours of Work for Unmarried Women Initially On Welfare With No Newborns After 1989 and With Non-zero Hours of Work in 1989 and 1992

Experimental

	1989	1992	Difference
Treatment Group	31.875	40.750	8.875*
$AFDC_{89}$=1	(3.583)	(1.556)	(3.906)
	[8]	[8]	
Control Group	38.806	43.258	4.452*
$AFDC_{89}$=0; LFP_{89}=1	(1.549)	(1.523)	(2.172)
$Income_{89}$ > 200% of Poverty	[31]	[31]	
	Differences-in-differences		**4.423**
			(4.721)

Non-Experimental

	1989	1992	Difference
Treatment Group	36.000	47.333	11.333
$AFDC_{89}$=1	(4.008)	(6.566)	(7.693)
	[6]	[6]	
Control Group	44.121	42.293	-1.828
$AFDC_{89}$=0; LFP_{89}=1	(1.783)	(1.296)	(2.204)
$Income_{89}$ > 200% of Poverty	[58]	[58]	
	Differences-in-differences		**13.161+**
			(7.274)
	Differences-in-differences-in-differences		**-8.738**
			(8.918)

Note: Treatment group is defined based on AFDC participation in the month of the interview. Control group is defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

Table A7

The Effect of Medicaid Expansions on Mean Hours of Work for Unmarried Women Initially Off Welfare With No Newborns After 1989 With Non-zero Hours of Work in 1989 and 1992

Experimental

	1989	1992	Difference
Treatment Group	40.506	38.714	-1.792
$AFDC_{89}=0$; $LFP_{89}=1$	(1.089)	(1.323)	(1.714)
$Income_{89} \leq 200\%$ of Poverty	[77]	[77]	
Control Group	38.806	43.258	4.452*
$AFDC_{89}=0$; $LFP_{89}=1$	(1.549)	(1.523)	(2.172)
$Income_{89} > 200\%$ of Poverty	[31]	[31]	
	Differences-in-differences		**-6.244***
			(3.034)

Non-Experimental

	1989	1992	Difference
Treatment Group	40.210	40.686	0.476
$AFDC_{89}=0$; $LFP_{89}=1$	(1.097)	(0.820)	(1.369)
$Income_{89} \leq 200\%$ of Poverty	[105]	[105]	
Control Group	44.121	42.293	-1.828
$AFDC_{89}=0$; $LFP_{89}=1$	(1.783)	(1.296)	(2.204)
$Income_{89} > 200\%$ of Poverty	[58]	[58]	
	Differences-in-differences		**2.304**
			(2.465)
	Differences-in-differences-in-differences		**-8.548***
			(3.959)

Note: Treatment and control groups are defined based on no AFDC participation in 1989, and labor force participation at the week of the interview. Standard errors are in parentheses. ** $p < .01$, * $p < .05$, + $p < .1$

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