The Effect of Medicaid Expansions on the Behavior of the Poor

by

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Abstract

An important form of compensation for welfare participants is health insurance, known as Medicaid. Any health insurance reform proposal would surely have effects on the welfare rolls. This thesis studies several avenues where Medicaid could affect the participant's behavior, specifically the decision to work, the decision to marry and the decision to bear children.

In Chapter One, I assess the impact of Medicaid, which is means-tested, on labor market outcomes. I identify these effects through a series of health insurance expansions for children. These expansions severed the traditional link between AFDC eligibility and Medicaid eligibility. They reduced the marginal tax rate for earning more than the AFDC breakeven point, which created room for a mother's earnings to increase without losing health insurance for her children. The expansions create treatment and control groups in three dimensions to identify Medicaid's effect: within a state, across states and over time. The expansions are unique in that they condition eligibility on the child's birthday. They consequently have different income effects on otherwise similar looking recipients with children of slightly different ages. I estimate that moving the income eligibility limit by 25 percent of the federal poverty level from its current level will reduce the probability of AFDC participation by 4.61 percent and increase the probability of working by 3.32 percent. Further, annual hours worked would increase by roughly five weeks. Finally, simulation results suggest that means-testing health insurance at 185% of the federal poverty level would result in annual AFDC savings of $410 per family and increase of $273 in tax revenue per family.

In Chapter Two, I explore whether extending Medicaid to children in two-parent families encourages marriage for their mothers. This might occur since welfare programs condition eligibility and benefits on residing in a non-traditional family, which translates into a female headed household with children under 18 present in practice. The expansions which relaxed the income margin in the first chapter also severed the link to family structure. This allows identification of Medicaid's effect on marriage, even within a state at a point in time. I find that extending Medicaid to all children in a family is associated with an increase in the probability of marriage of 2.61 percent. I also show that the expansions not only offered new incentives to marry, but created some incentives to get divorced. After controlling for this second effect, known as the "independence effect", I find that Medicaid's effect increases the probability of marriage by 4.36 percent. The results on family structure are stronger than previous findings on the effect of AFDC cash benefits, possibly because the potential husband is more easily able to substitute wage earnings for cash benefits than employer provided health insurance for Medicaid.

In Chapter Three, I examine the role of health insurance on fertility by utilizing the fact that the expansions provided income effects to only certain families in the form of health insurance. Since the expansions conditioned eligibility on a child's birthday, they allow identification of Medicaid's effect within a state by utilizing variation in the age distribution of older children. I construct a valuation of this income effect by using health expenditure data from the *National Medical Expenditure Survey*, which is linked to the *Current Population Survey* to assess the effect on fertility. My primary conclusion is that while the income effect of health insurance coverage does indeed significantly increase fertility, the economic magnitude is small. Increasing the value of Medicaid health insurance by $1,000 leads to a rise in fertility of no more than one-third of one percent, or less than a five percent increase in the fertility rate. This stands in contrast to work using the RAND Health Insurance Experiment (HIE), which found increases of nearly thirty percent in the fertility rate. A possible explanation for the difference between the two findings is that the HIE had an anticipated deadline, which might change the timing of births, while the Medicaid expansions were viewed as more permanent coverage.

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Preface

In the past decade Medicaid benefits have become an increasingly important component in the welfare compensation package. By 1991, Medicaid expenditure exceeded AFDC expenditure for welfare recipients. This growing importance of health insurance led me to examine Medicaid's potential impact on economic decision making. The main finding of the dissertation is that Medicaid distorts several margins: the labor supply decision, the marriage decision and the fertility decision. The finding suggests that health insurance reform might additionally be viewed as welfare reform, by severing the tie of Medicaid eligibility from welfare eligibility more generally.

In Chapter One, I examine the effect of Medicaid on labor supply and welfare participation. As long as the recipient earns less income than the "AFDC break-even level" then her family receives Medicaid. If she earns any more than this, she loses the Medicaid coverage entirely, which can be thought of an exceedingly high marginal tax rate for earning extra income.

In the past this has been an exceedingly difficult question because Medicaid eligibility has been contingent on AFDC eligibility. The effects of Medicaid are therefore confounded with the effects of cash benefits. One important contribution of this chapter is utilizing a series of health insurance expansions for children which severed the link to AFDC eligibility, allowing independent variation in Medicaid eligibility. The expansions relaxed the income limit where the recipient lost Medicaid for her children. While Medicaid coverage is still means-tested, the income limit can be much higher than under AFDC. In some states, the expansions allow a recipient to double or triple her previous earnings and still retain this coverage.

These expansions also condition the eligibility and generosity based on arbitrary cut-offs of a child's birthday, which creates "treatment" and "control" groups that are identical in many respects except in Medicaid expansion coverage. By arguing that the groups are similar in many respects except in eligibility, I am able to gauge Medicaid's effect without the need to value health insurance.

Using the *Current Population Survey*, I find that increasing the income eligibility limit by 25 percent of the Federal poverty level reduces the probability of AFDC participation by nearly 5 percent and increase the probability of working by more than 3 percent. Since recipients move off the welfare rolls and into the labor force from the expansions, this has two revenue consequences. By leaving the welfare rolls, the government saves money on AFDC expenditure. By increasing labor supply, the government receives more money in taxes. I find that means-testing the Medicaid insurance at 185 percent of the FPL results in an average annual savings of $410 per family from reduce AFDC expenditure and results in an average annual increase of $273 in tax revenue per family.

In Chapter Two, I examine the impact of Medicaid on family structure. The past three decades have witnessed drastic changes in the composition of families, with large increases in the incidence of female headship and out-of-wedlock births. These changes coincide with changes in the generosity and scope of the U.S. welfare system. In particular, Medicaid and food stamps were introduce in the middle of the 1960s.

A distinguishing feature of AFDC and Medicaid is their categorical nature.

Besides satisfying income requirements, a recipient typically has had to reside in a non-traditional family to be eligible. The loss of the welfare benefits could be construed as a tax on marriage.

I improve on previous work in three important respects. First, I examine the effect of health insurance on family structure rather than cash benefits. It is difficult for cash benefits to explain the time series trends because cash benefits have declined in real value while headship rates have increased. One the other hand, real expenditure on Medicaid has increased. Second, I examine the decision to marry rather than the decision to get divorced. There is no compelling reason to expect that benefits would have effects that are equal and opposite in size for these decisions, because the costs and benefits of moving between the two states may be asymmetric. Third, the Medicaid expansions again allow for a control group, even within a state at a point in time.

The expansions severed the link to AFDC along two dimensions: the marriage margin and the income margin. While severing the link to family structure leads to increases in the incentive to marry, relaxing the income margin has the potentially perverse effect of increasing the incentive to divorce, since new opportunities are available on the single woman's budget set.

I find that extending Medicaid to all children in a family is associated with an increase in the probability of marriage of 2.61 percent. After controlling for outflows from marriage due to the "independence effect", Medicaid increases the probability of marriage by 4.36 percent. I find larger effects for Medicaid than previous work has found for AFDC cash benefits, possibly because the potential husband is more easily able to substitute wage earning for cash benefits than employer provided health insurance for Medicaid.

In Chapter Three, I examine the effect of the expansions on fertility. Since the expansions conditioned eligibility on a child's birthday, they allow for within state variation in the value of the income effect through variation in the age distribution of older children. To analyze the effect on fertility, I link the *Current Population Survey (CPS)* with health care expenditure information from the *National Medical Expenditure Survey (NMES)* to construct an exogenous valuation of the expansions for each family. I find that the expansions have a statistically significant effect on fertility, confirming some simple predictions of the theory. On the other hand, the economic importance is small. In the preferred specification, raising the value of Medicaid by $1,000 leads to an increase in the probability of having a child of no more than one-third of one percent, or leading to less than a five percent increase in the fertility.

Chapter One

The Medicaid Notch, Labor Supply and Welfare Participation: Evidence From Eligibility Expansions

1. Introduction

The welfare package consists of three main benefits: cash assistance through Aid to Families with Dependent Children (AFDC), health insurance coverage through Medicaid, and food subsidies through Food Stamps.[1] In the past decade Medicaid benefits have become more important as medical costs have soared while cash benefits have failed to keep up with inflation. In fiscal year 1991, Medicaid expenditures of $21.9 billion on 12.6 million AFDC recipients exceeded cash payments of $20.9 billion to this group (Green Book, 1993). This paper investigates the hypothesis that losing Medicaid coverage is a large deterrent to leaving welfare. If this is true, then current debates on extending health insurance could additionally be viewed as welfare reform, since the new policies, unlike past Medicaid eligibility, would condition on neither income nor family structure.[2]

Traditionally, eligibility for Medicaid has been contingent on eligibility for AFDC -- that is, one simultaneously qualifies for Medicaid and AFDC by having net income under a state's payment standard. The health insurance is untaxed as long as the AFDC recipient earns less than the "AFDC break-even level", the point where AFDC benefits are lost. It is then entirely lost from earning additional income, which can be thought of as a marginal tax rate in excess of 100%.

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[1] Moffitt (1992) presents a thorough and readable discussion of the welfare system.

[2] Health insurance has been found to distort behavior in other contexts. Madrian (1992) finds that "employment-tested" health insurance causes a 25% reduction in job mobility. Gruber and Madrian (1993) show that COBRA health insurance continuation coverage raises the probability of early retirement among older males by 15%.

It is difficult to separate out the effect of Medicaid from the effect of AFDC cash assistance on outcomes like labor force participation, hours of work, and welfare participation because eligibility for the programs was collinear. This could be why very little work has been done on Medicaid's distortions relative to the work done on AFDC's distortions. If Medicaid has a substantial effect on welfare and labor force participation, then previous estimates AFDC's effect could be overstated by omitting Medicaid's influence. Three previous studies, Blank (1989), Winkler (1991) and Moffitt and Wolfe (1992) have tried to identify Medicaid's effect, but have not arrived at a clear consensus. Blank (1989) proxies for the value of Medicaid with the average expenditure per recipient in each state and finds that Medicaid has insignificant effects on welfare participation. Winkler (1991) also proxies for the value of Medicaid with the average expenditure per recipient in each state and finds that Medicaid has a statistically significant, but small effect on labor force participation and no effect on hours of work. Finally, Moffitt and Wolfe (1992) proxy for the value of Medicaid by developing a family-specific health index and find that the value of Medicaid has a large negative effect on labor force participation and large positive effect on welfare participation.

This paper will explore some recent Medicaid expansions for children which sever the link between Medicaid eligibility and AFDC eligibility and generate sizable exogenous shocks to the budget set for only some potential welfare recipients. These expansions condition eligibility and generosity on the child's birthday, which creates plausibly identical "treatment" and "control" groups in many respects except for eligibility for the expansions. Using the expansions and recent data, I offer new evidence on Medicaid's distortions and arrive at different conclusions from some previous work. One contribution of this paper will be to offer a way to gauge the effect of Medicaid without having to individually value health insurance, in order to predict the direction of labor market responses. If one accepts that the "treatment" and "control" groups are identical, so that the value of Medicaid is identical for different groups, then we can predict the change in labor force participation, AFDC participation and hours of work by moving the income eligibility limit for Medicaid. No valuation of health insurance is necessary because the recipient will get the same health insurance package after the eligibility expansion, only means-tested at a higher level. Using the *Current Population Survey (CPS)*, I find that increasing the income eligibility limit by 25% of the Federal poverty line from its current level will reduce the probability of AFDC participation by 4.61% and increase the probability of labor force participation by 3.32%. Further, annual hours would increase by roughly five weeks. Finally, simulation results suggest that means-testing health insurance at 185% of the Federal poverty level would result in annual AFDC savings of $410 per family and increases of $273 in tax revenue per family, since families would exit welfare and enter the labor force.

The paper is arranged as follows: Section 2 describes the legislative changes used to identify Medicaid's effects. Section 3 sets up the theoretical framework to analyze Medicaid's effects. Section 4 describes the data extract, various years of the March Current Population Survey. Section 5 provides reduced form evidence of Medicaid on labor force participation and AFDC participation. Section 6 presents policy simulations from changing the income limits for Medicaid eligibility. Section 7 concludes.

2. Legislative History and Identification

2.1 Overview of Legislation from 1986 to 1990

The Medicaid expansions used in this paper were legislated in response to the Omnibus Budget Reconciliation Act (OBRA) 1981, which severely reduced access to health care services for the poor by placing heavy restrictions on AFDC eligibility. As a result, nearly 40 percent of working AFDC families were removed from the welfare rolls and roughly two million people (nearly 500,000 families) lost Medicaid eligibility between 1979 and 1983. Starting in 1984, and especially from 1986 onward, Congress attempted to increase access to health care for pregnant women, infants and children through a series of Medicaid expansions.[3] The ensuing legislation severed the link between Medicaid eligibility and AFDC eligibility by eliminating Medicaid eligibility criteria related to the family structure and, more importantly for labor supply, an individual state's AFDC payment standard.

The legislation allowed Medicaid coverage to be means-tested to some percentage of the federal poverty level (FPL), usually 100% or 133%. For a family of three in 1989, 133 percent of the federal poverty level was $13,380. Table 1 shows the budget set facing a family of three in Pennsylvania. Medicaid is "cashed-out" at the state average expenditure per AFDC family, which was $2,304 in Pennsylvania. For the entire country, average expenditure was $682 per AFDC child and $1,290 per AFDC adult. Medicaid is lost between $9,000 and $10,000 of earnings, and total income does

[3] While pregnant women only receive services related to the pregnancy or complications from the pregnancy, infants and children receive full coverage equivalent to that received by the categorically needy.

not reach its previous level again until gross earnings nearly double at $17,000. Losing this health insurance by increasing earnings is know as the "Medicaid notch." In this case, the expansions move the Medicaid notch from $9,000 to $13,380. From this table, we can also see that Medicaid is a substantial component of the welfare package.

The expansions provided health insurance coverage to young children which was conditioned on the child's birthday and state of residence. If the decision to have a child was unrelated to the incentives that these expansions offered, then eligibility for coverage could be viewed as being randomized by the child's birthday, state of residence and date of implementation. The expansions therefore create plausibly identical "treatment" and "control" groups to gauge the effects of moving the income eligibility limit for Medicaid to a much higher level. The treatment group is families with children fortunate to be born after the birthday cut-off, while the control group is families with children who were born before the birthday cut-off.[4]

Several pieces of legislation expanded access to health care for children.[5] OBRA 1986 and 1987 gave states the *option* to cover young children, the option to choose a range of children's ages which they could cover, and the option on what income level to move eligibility for Medicaid to. The Family Support Act (FSA) 1988 gave families twelve months of transitional Medicaid coverage for those leaving AFDC due to earnings or hours increases. Previously the coverage had been four months. OBRA 1989 and 1990 *mandated* Medicaid coverage to a larger group of children, for a longer transitional

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[4] This identification strategy is similar to Krueger and Pischke (1992) who examine the effect of Social Security on retirement using differential generosity of the program across cohorts based on their year of birth.

[5] Appendix I provides a detailed account of each piece of legislation.

period, and to a higher percentage of the FPL than previous legislation. The later mandates were binding in almost every state. Another piece of legislation, the Medicare Catastrophic Care Act, mandated that states could not cut their AFDC benefit or eligibility levels in order to finance the Medicaid expansions. In particular, states which implemented an expansion before December 22, 1987 were prohibited from lowering their AFDC standards in effect on April 17, 1986. States implementing expansions after this date were prohibited from lowering their AFDC standards in effect on July 1, 1987. Without this, it might be more difficult to tell the effect of Medicaid on labor supply, since transitional health insurance coverage and reduced cash benefits both tend to increase labor force participation and decrease AFDC participation.

Table 2 illustrates the generosity of the expansions at two points in time. OBRA 1986 gave states the option to implement the expansions to children under 2 years of age up to 100% of the FPL. Within one year of its implementation, half of the States had expanded eligibility to 100% of the FPL. Within two years, 44 States and the District of Columbia had expanded. OBRA 1987 gave states further options, by letting them implement expansions for children up to age 8 to 100% of the FPL, who were born after September 30, 1983. It also increased the income eligibility limit even more for infants. Eighteen states used these options to raise the threshold for infants above the poverty level, most to the upper limit of 185%. Then OBRA 1989 mandated that states cover children under age 6 to 133% of the FPL, by April 1, 1990. The impact of this law was felt more widely by the states. Thirty-two States did not have thresholds at 133 percent of the FPL for infants and were required to adjust incrementally, most from 100 percent to 133 percent. A much larger effect surrounded the mandated coverage of children, however. Only 14 States were already covering children to 6 or 7 years of age. Twenty-five states were phasing-in coverage of children from 2 to 5 years of age, and 12 States covered only infants to 1 year of age under the newly eligible groups. Finally OBRA 1990 mandated that states cover all children under age 19 to 100% of the FPL who were born after September 30, 1983. Thus, all poor children under age 19 would be eligible for Medicaid by the year 2002.

2.2 Parameterization of Health Insurance Expansions

The law changes create three dimensions of variation along which to identify Medicaid's effect: mothers with different aged children within a state at a point in time, mothers across states at a point in time and mothers within a state over time. The most intriguing dimension is *within* state. Consider a state that implemented the OBRA 1987 provisions to the maximum extent possible, as soon as possible.

OBRA 1987 options	
After July 1, 1988	After October 1, 1988
The state *may* extend Medicaid coverage to any age up until 5 if:	The state *may* extend Medicaid coverage to any age up until 8 if:
• Child's age<5 *(state can choose 1, 2, 3, 4 or 5).*	• Child's age< Age limit *(state can choose 6, 7 or 8).*
• Family income less than 100% of FPL *(state can choose any level < 100%).*	• Family income less than 100% of FPL *(state can choose any level < 100%).*
• Child born *after* September 30, 1983.	• Child born *after* September 30, 1983.

In that case the following mother's with children of different birthdays get the following "treatment" on July 1, 1988:

Child's birthday	Child covered by expansion?	Length of coverage
12/25/88	Yes	8 more years
10/1/83	Yes	3.25 more years
9/30/83 or before	No	0 years

In addition to variation in the eligibility and duration of coverage margins, the laws generate variation in the *earnings (or percentage of FPL)* margin, the income limit where the recipient now loses coverage. It means that after the expansions, the new "Medicaid notch" depends on the child's age. For instance, after July 1, 1991, the mother could face the following earnings schedule for losing Medicaid coverage, due to the binding Federal mandates, OBRA 1989 and 1990 (conditional on her children being eligible for the expansions):

Child's age	Age 0	Ages 1 to 5	Ages 6 to 18	Ages 19 and over
Percentage of FPL	185%	133%	100%	0%

In other words, a mother with a 5 year old can earn up the 133% of the FPL before losing Medicaid, while a mother with a 6 year old can only earn up to 100% of the FPL. The differences in generosity of the new "Medicaid notch" are primarily generated by infants (for whom the mother gets a higher percentage). After April 1990, however, a federal mandate (OBRA 1989) generated differences for mothers with children between ages five and six.

With the heterogeneity of treatment in the expansions, some parameterization of the expansions was necessary to create an independent variable. One possibility is a dummy variable for eligibility for the expansion. Since the expansions may not have much impact in a state, however, this misses an important aspect of the expansion. In California, where welfare benefits are quite generous, the expansion will have little impact and a dummy variable will not capture that. Another possibility is using the new income eligibility limit. That is, if California implemented an expansion to 100% of the FPL (which I will call NEWNOTCH%), then we might consider using the new income limit as the measure. This measure suffers a similar problem to the dummy variable in that it does not capture the change in opportunities, however. That is, if Alabama implements a similar expansion to 100% of the FPL, it should have more impact there because the budget set is more drastically changed.

The measure I incorporate captures the essence of the expansions: how much the point of the Medicaid notch moves The measure used in estimation is:

$$(1) \quad GAIN\% = \max(NEWNOTCH\% - OLDNOTCH\%, 0)$$

It is the change in the income limit above which the recipient loses Medicaid, which is the change in opportunities on the budget set. It indicates the *incremental* gain to leaving welfare and getting a job due to the expansions, where all variables are measured as a percentage of the federal poverty level. OLDNOTCH% is the income level where the recipient previously lost her Medicaid coverage. Measuring NEWNOTCH% is straightforward: it is equal to OLDNOTCH% (so that GAIN% equals zero) if the recipient is ineligible for the expansion (because of an older child or the state not implementing an expansion), while it equal some percentage of the FPL, usually 75%, 100%, 133% or 185% if the recipient qualifies for the expansion. There is no guarantee that NEWNOTCH% will be greater than OLDNOTCH% (the expansion may have no "bite"), even for recipients who qualify for the most generous expansions, because after the appropriate institutional detail is accounted for, the income levels that recipients can earn from OLDNOTCH% may be quite high. This explicitly accounts for the fact that the expansions should have less impact in a generous welfare state, like California, (through OLDNOTCH%) since the recipient could have worked and earned some amount of money before the expansion. Furthermore, taking a maximum is appropriate, since we do not want to "penalize" a mother who is in a state that provides an austere expansion. Her opportunities on the budget set opportunities remain the same rather than being reduced, so assigning zero is appropriate.

While we cannot measure the actual Medicaid notch, which entails some method of valuing the health insurance, OLDNOTCH% measures the income level where the recipient loses her health insurance. It depends on a state's *payment standard*.[6] OLDNOTCH% also varies depending on how many months the recipient has been working (due to changes in the marginal tax rate and standard deduction) and because of family size (through the level of the payment standard), work expenses and child care expenses. OLDNOTCH% covers a longer time frame than the first four months of

[6] This analysis follows the Green Book (1993), p. 621.

work, because DEFRA 1984 implemented nine to fifteen extra months of transitional Medicaid coverage for recipients who lost Medicaid due to earnings or hours disqualifications related to the expiration of the "30 and 1/3 disregards." Furthermore, the Family Support Act affected the calculation of the notch in several ways. *Ceteris paribus,* the notch will be highest during the first four months of work and after the FSA was implemented. In some states, OLDNOTCH% can be well in excess of 100% of the FPL. After incorporating all this detail, OLDNOTCH% is calculated as:

$$(2) \quad OLDNOTCH\% = ((1.5 \cdot PAYMENT) + DISREGARD + WORKEXP + DAYCARE)/POV\$$$

where PAYMENT stands for the State's payment standard, DISREGARD for the standard deduction, WORKEXP for work expenses, POV$ for the dollar amount of the federal poverty level (appropriately adjusted for family size and inflated by 1.15 for Hawaii and 1.25 for Alaska), and DAYCARE for total child-care expense deductions.[7] DAYCARE is calculated as:

$$(3) \quad DAYCARE = (1 + \tfrac{1}{2} \cdot 1(post\ FSA)) \cdot (deduction/child) \cdot (children)$$

where 1(•) is defined as an indicator variable equal to one after the passage of the FSA,

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[7] The payment standards are taken from various issues of the Green Book. Since the Green Book only provides payment standards for a family size of three, several steps were taken to get a more complete account. First, in 42 of the 51 jurisdictions, the payment standard is equal to the maximum AFDC benefit (at zero income), which is presented for families of sizes 2 through 6 in the Green Book (as of January 1989, 1990, 1991 and 1992). For families larger than this, I imputed the payment standard by adding the difference between families of 5 and families of 6 to the payment standard of a family of 6. For the remaining 9 jurisdictions, the payment standard is equal to the need standard of the state. Using charts of need standards from the NGA and "State Characteristics of AFDC Programs" from HCFA, I obtained need standards for families of sizes 2 through 4 and used the same procedure to impute payment standards for larger families. The FPLs for different family sizes were also obtained from the Green Book. The regressions in Section 5 were rerun on family sizes of 2 to 6 to examine the sensitivity to this imputation for larger families, since the placement of OLDNOTCH% is completely accurate for smaller families. These are presented in an appendix.

zero otherwise.

An alternative method of identification, not taken in this paper, could be to use OLDNOTCH% itself to measure the effect of Medicaid. That is, we could ask whether welfare participation is higher in states that have a higher income cut-off at which the recipient loses Medicaid. The advantages of using the proposed legislative expansions, and hence GAIN%, instead of OLDNOTCH% include:

- Using GAIN% allows us to exploit within state variation, while identification with OLDNOTCH% hinges on cross state variation.
- It is more difficult to separate out the influence of AFDC *benefits* from the income level where the recipient loses Medicaid when using OLDNOTCH%. That is, a state with a high OLDNOTCH% most likely also has high AFDC benefits. In the estimation, the effect of AFDC benefits are controlled for with dummy variables for state fixed-effects and interactions of state and time effects. In addition, the benefits derived from food stamps are not incorporated into the analysis. There is little cross sectional variation in food stamps (except in Alaska and Hawaii), so any changes in this program that affect either AFDC participation or labor force participation should be captured by dummy variables for each year, which are included in all specifications presented in the next section. Also, since Medicaid eligibility is in no way linked to the eligibility of food stamps (either before or after the expansions), the use of OLDNOTCH% to measure the income level where Medicaid is lost is appropriate.

2.3 Identification

In addition to within state variation based on the child's age, these expansions created *across state* variation and *over time* variation, because different states implemented the expansions at different times. Consider the following hypothetical example: Between 1988 and 1989 California instituted a Medicaid expansion for children up to age 5, while New York did not. Then we could examine "first differences," which uses mothers with older children in California as a control group. Let $\mathrm{OUTCOME}_{CA,89,5}$ stand for mothers with children less than six years old and $\mathrm{OUTCOME}_{CA,89,6}$ stands for mothers with children greater than or equal to six. The impact of the law change could then be measured by:

$$\mathrm{OUTCOME}_{CA,89,5} - \mathrm{OUTCOME}_{CA,89,6}$$

An objection to this "first-difference" is that the two groups may *not* be comparable. That is mothers with older children face lower child care costs, since their children attend school. This alone would induce mothers with older children to participate in the labor force, thereby understating the true effect of the expansions. Another "first-difference" we could consider to gauge the effect on different outcomes is following the mothers with young children over time. The impact of the law change could then be measured by:

$$\mathrm{OUTCOME}_{CA,89,5} - \mathrm{OUTCOME}_{CA,88,5}$$

The time series correlations alone might also not be entirely convincing, because labor market outcomes might be influenced by other events occurring at the same time, such as changes in general economic conditions, passage of the Family Support Act offering incentives to leave welfare independent of the Medicaid expansions, and a general trend increase in labor force participation among female headed households. Therefore we could combine these dimensions, using the previous year and mothers with older children to control for such factors in a "difference-in-differences" specification. The impact of the law changes is now measured by:

$$(\text{OUTCOME}_{CA,89,5} - \text{OUTCOME}_{CA,89,6}) - (\text{OUTCOME}_{CA,88,5} - \text{OUTCOME}_{CA,88,6})$$

While it is harder to indict the "difference-in-difference" estimator, one could still argue that other factors are driving the observational difference. Economic conditions may have affected mothers with older children more, the FSA gave some nationally uniform exemptions from the work incentive programs for mothers with young children (under age 3), and the Earned Income Tax Credit gives extra exemptions to families with younger children, all of which could be the cause of any observational difference rather than the Medicaid expansions. If this is the case, then we can use New York, a state which did not implement expansions between 1988 and 1989, to control for nationally uniform shocks over time that affected mothers with younger children differently than those with older children.[8] The impact of the law changes is now measured by:

$$\{(\text{OUTCOME}_{CA,89,5} - \text{OUTCOME}_{CA,89,6}) - (\text{OUTCOME}_{CA,88,5} - \text{OUTCOME}_{CA,88,6})\}$$
$$- \{(\text{OUTCOME}_{NY,89,5} - \text{OUTCOME}_{NY,89,6}) - (\text{OUTCOME}_{NY,88,5} - \text{OUTCOME}_{NY,88,6})\}$$

Two points about this type of identification strategy deserve mention. First, one might argue that the law changes are endogenous. States that voluntarily adopted the expansions might have implemented them because labor force participation had been

[8] See Gruber (1993) and Gruber and Poterba (1993) for other examples of this type of identification strategy.

falling or AFDC participation had been increasing. This should not bias the within state estimation, however. One would further need to argue that the states knew the expansions would increase the labor force participation of mothers with younger children but would have no effect on mothers with older children. Second, one might argue that other factors beside the expansions were occurring at the same time which influence labor supply. In this case my control group should also be affected, so the estimates still give Medicaid's true effect.

3. Theoretical Effects of Medicaid

To analyze the effect of Medicaid on labor supply and welfare participation among potential welfare recipients, I use a variant of the static labor supply model which incorporates taxes and health status.[9] The consumer maximizes utility, U=u(Leisure, Other Goods;θ_{HEALTH}). Other goods can be thought of as a composite commodity and θ_{HEALTH} is the underlying health status of the individual, which can be viewed as an environmental parameter.[10] Assume that the potential recipient faces a constant pre-tax wage, $w^0(\theta_{HEALTH})$. Her wage depends on her productivity, which is affected by her health status. But her wage is assumed not to depend on the number of hours worked.

The welfare and tax system create non-linearities in the budget set. At zero

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[9] Hausman (1985) lays out the theoretical framework for incorporating taxation into the standard labor supply model. Both Blank (1989) and Winkler (1991) present careful theoretical expositions on how Medicaid affects labor supply.

[10] Alternatively, I could use an additional time constraint that includes time spent on health and sickness activities. Thus I could substitute $T_{health} + T_{labor} + T_{leisure} = 24$ into the utility function.

hours of work, the mother receives a certain level of AFDC, Food Stamp and Medicaid benefits, known as the "guarantee." Figure 1 illustrates the budget set. As she begins to work, her AFDC and Food Stamp benefits are taxed away at a high marginal tax rate, so that her after tax wage is $w^1=(1-\tau_{AFDC})*w^0$, where τ_{AFDC} is the marginal tax rate for earning income while on welfare.[11] Once she works more than $H_{break-even}$, the hours of work where the entire welfare benefit is taxed away, she loses her AFDC eligibility, and hence her Medicaid benefits, which creates a *dominated* part of the budget set, known as the "Medicaid notch." Once the recipient works more than this level of hours, she reenters the Federal tax code and faces an after tax wage of $w^2=(1-\tau_{FED})*w^0$, where τ_{FED} is the marginal tax rate in the standard tax code. To determine what region of hours worked is dominated, however, we would need to know the value of the benefits she received from Medicaid, which in turn depends on factors like health status, family size and degree of risk aversion.

As states have relaxed eligibility for Medicaid coverage by increasing the income levels to a higher level than what an AFDC recipient could earn, the notch has moved. That is, the coverage is *still* means-tested, but at a much higher level than the AFDC break-even level. The expansions, which will be used to identify Medicaid's effect on labor market outcomes, amount to a shift in "virtual income" for individuals located

[11] τ_{AFDC} is also known as the benefit reduction rate. It is meant to account for other taxes and subsidies as well, such as the Earned Income Tax Credit, state taxes and Federal taxes. Also note that non-labor and non-transfer income is excluded, since AFDC taxes this at 100% with no deductions.

along the non-welfare part of the budget constraint.[12] This change in virtual income yields several reduced form predictions for those eligible for expansions by changing the budget set as illustrated in figures 1 and 2. Since the following predictions rely *only* on revealed preference arguments, rather than functional form restrictions for the underlying preferences, the indifference curves can be omitted from the figures since they add no new insights.[13]

- Labor force participation unambiguously increases, since the new opportunities in the budget set occur where the woman participates. Either she does not change her behavior at all, or she moves to this new part of the budget set.
- AFDC participation unambiguously decreases, because the only new opportunities are where the woman leaves AFDC.
- AFDC participation decreases more than labor force participation increases. This occurs since some women will be located along the welfare part of the budget set (but not at zero hours of work) before the expansions, implying participation in both AFDC and the labor force. After the expansions these people could increase their hours and locate on the post-expansion part of the budget set, which we observe as exiting AFDC

[12] Since food stamps is not tied to the AFDC breakeven level, the budget set has more kinks than shown in the figures. This does not change any prediction, however, so it is not modeled in this section but is controlled for in the estimation with time dummies.

[13] Blank and Winkler also tried to identify Medicaid's effect from Medically Needy (MN) programs, which is most similar to the approach taken in this paper. The two studies tend to get coefficient estimates on MN programs opposite in sign from what theory predicts (and sometimes statistically significant from zero). One possible explanation for the weak result could be the small nature of the MN programs -- they offered little incentive to leave welfare. For example, as of January 1992, the Medically Needy income level was less than the AFDC categorically needy income level in Georgia, Kentucky, Maine, Minnesota, Oklahoma, South Carolina, Tennessee and Utah. In addition, the scope of covered services that states must provide to *categorically needy* recipients is much broader than the minimum scope of services for the *medically needy*.

but no effect on labor force participation. For women initially located at zero hours of work, the two effects should be the same, since the only *new* opportunity the expansion offers is to exit AFDC and enter the labor force. For women initially off welfare, their hours may decrease, but they will not participate in AFDC, which they could have already done. Therefore, in aggregate, the effect on AFDC participation is larger than the effect on labor force participation.

• The effect on total hours of work is ambiguous. Hours increase for women initially on the welfare part of the budget constraint, but could possibly decrease for women initially on the non-welfare part of the budget constraint.[14]

Previous work tries to identify Medicaid's effect by valuing health insurance. Valuing an in-kind benefit such as health insurance is a daunting task, because Medicaid's value should incorporate health status, risk aversion, scope of medical services offered, access to care, insurance copayments and deductibles. In addition, the presense of adverse selection in the insurance market implies that those who apply for insurance are likely to value it at more than its actuarial value. The two methods to value health insurance used in past studies were assigning the average expenditure in the state for the value and using an individual health status for the value.

Even that is difficult, however. Using individual health status measures to identify Medicaid's importance presents two potential confounding effects. Clearly, poor health increases the value of Medicaid because of increased utilization of health care

[14] In section 6, I will present some evidence on this "free-gift for working mothers" hypothesis when discussing the costs of these expansions.

services. As Wolfe and Hill (1993) explain, however, poor health lowers the potential wage the AFDC recipient could earn in deciding whether to work by lowering her marginal productivity. When the wage falls, the worker may reduce her hours or exit the labor force altogether. Poor health also increases the marginal disutility of work, making the indifference curves in {Leisure,Other Goods} space steeper, which also tends to reduce the number of hours worked. Since the entire utility maximization problem (both the budget constraint and preferences) changes, it is not clear how this approach measures the effect of the "Medicaid notch" on labor force and welfare participation. Using this measure, we mistakenly attribute these other two effects, which tend to decrease labor force participation, to Medicaid. Figure 3 illustrates how the budget set changes by employing variation in health status, which Moffitt and Wolfe (1992) used to identify Medicaid's effect. If poor health *only* increased the value of Medicaid, without changing the wage rate, $w^0(\theta_{HEALTH})$ or preferences, u(Leisure, Income,θ_{HEALTH}), then we could predict that AFDC participation increases with poor health. The changes in labor force participation and hours of work would be ambiguous without the restriction that leisure is a normal good. If poor health *also* lowers the wage the potential recipient faces, the figure shows that no predictions are possible, even for those initially on the welfare. The predictions remain ambiguous if we then include health as an argument in the utility function, which would make the indifference curves steeper (not shown in the figure).

A second potential source of variation, state average expenditure, which Blank (1989) and Winkler (1991) used. This measure may not generate meaningful variation since health expenditures are highly skewed -- many people have no medical expenses while a few have enormous expenses.[15] Variation in average expenditure can come from three different dimensions: differences in medical services or benefits, differences in prices or utilization of the same services across states, or differences in the underlying health status (and hence, utilization of services) of different states' populations. The value of actual *benefits* may not vary substantially across states because the most important services are either mandated by the Federal government or virtually every state has adopted the important optional benefits. There are 3 mandated services include and 33 optional benefits a state may provide. While the optional benefits could be utilized as instruments for the value of health insurance, most services are either minor in importance or aimed at the aged, blind and disabled, groups eligible for Medicaid other than AFDC recipients. One essential point is, however, if we see any labor supply response due to the breadth of services margin, surely we would expect larger responses due to the eligibility for health insurance margin.

Three other points deserve mention. First, I do not model the possibility of employer provided health insurance or the use of emergency room facilities in the presentation of this structural model. The existence of the "notch" is predicated upon no feasible insurance alternatives for these women.[16] It may be reasonable to think that

[15] The *median* Medicaid expenditure is much lower than the mean Medicaid expenditure in all states.

[16] Short, Cantor and Monheit (1988) examine the dynamics of Medicaid enrollment. While 38% of the sample that was initially on welfare left over the next two years, only 43% were covered by private health insurance and 55% became uninsured. The remainder of their sample, 62% remained on Medicaid for the entire period. This should put an absolute upper bound on the availability of employer provided health insurance for this group, because of the standard self-selection argument: only those with the best wage and health insurance opportunities should leave welfare, so the 62% of the group that remained on welfare for the entire time should have even poorer wage

with the skill mix which the group possesses, the possibility of employer provided health insurance coverage is quite low. Unless there are systematic differences between my "treatment" and "control" groups in the incidence of employer provide health insurance coverage or emergency room use, then my results should still give the true effect of Medicaid. In trying to construct a structural model of labor supply with Medicaid and employer-provided health insurance, one encounters the same problems in imputing unobserved employer provided health insurance as with imputing the unobserved wage for those who do not participate in the labor force. Second, I do not model the possibility of "welfare stigma," which is the observation that some potentially eligible recipients do not actually participate. Moffitt (1983) shows that this stigma effect can be incorporated by adding the additional argument of "program participation" to the utility function, where participation would have a negative effect on utility. Stigma would tend to discourage families off welfare from taking up the coverage. Third, I abstract away from any dynamic labor supply or collective labor supply issues.

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and health insurance opportunities. In this case both the reservation wage, w^*, and the health insurance opportunities, HI^*, are unobserved for non-workers (people who did not leave Medicaid).

4. The Data Set

The data set, which consists of different cross sections over time, was constructed using the March Current Population Survey (CPS), from the years 1989, 1990, 1991 and 1992. These years cover the period when the Medicaid expansions occurred. The CPS is a timely, nationally representative survey interviewing a large number of households (approximately 57,000 per month). Its March Annual Demographic file contains information on demographic characteristics and household composition, as well as retrospective information on labor force participation, hours of work and welfare participation.[17] The sample used in the estimation contains 16,062 single[18] mothers between the ages of 18 and 55 with at least one child under 15 present.[19] I use a smaller range of children's ages (only from age 0 to age 14) than the previous studies (usually age 0 to age 18) for two reasons: First, during the four year period which my

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[17] Labor force participation is based on the answer to: "Did ... work at a job or business at any time during 19..?," which refers to the previous year. AFDC participation is based on the answer to: "Did ... receive AFDC or some other type of assistance?" Finally, annual hours of work is defined as the product of two variables: "During 19.., in how many weeks did ... work, even for a few hours?" AND "In the weeks that ... worked, how many hours did ... usually work per week?" This second question is top-coded at 99 hours per week, but this is not a binding constraint.

[18] Where "single" means: divorced, separated or never married. This criterion is not immune to the criticism that marital status itself is endogenous to the structure of the welfare system, so that the sample should consist of ALL women, not just single women. However, to more closely replicate Blank (1989), Winkler (1991) and Moffitt-Wolfe (1992), I consider only single women. In Yelowitz (1993), using the same expansions, I find that extending Medicaid to two parent families substantially increases the probability of marriage.

[19] In addition, I excluded any mother who was receiving Medicare (presumably her status was either the result of a coding error or some unusual qualifying event, like End Stage Renal Disease or disability, since very few people under age 65 qualify for Medicare). I also conditioned on the mother being a non-veteran, to exclude the possibility of CHAMPUS coverage. Finally, I exclude mothers who were in "ill-health" or had some "other personal handicap in finding a job" during the survey week of the CPS. These exclusions are rather trivial and resulted in only several deletions.

data spans, 1988 to 1991, the expansions never affected children over age 8, so using children up to age 14 should be an adequate control for within state variation in the benefit schedule. Second, and more importantly, I was concerned with the possibility that older teenage children may form their own families and collect welfare benefits independently of their mother so that modelling the joint labor supply decision would be more appropriate when older children are present. Further, when the youngest child reaches an age of 19, the family is automatically disqualified for welfare, so it would more difficult to identify reasons for labor force and welfare participation.

To each mother's record I linked the youngest child's age which, along with the time period and state of residence[20], is used to impute eligibility for and generosity of the expansions. I therefore compare labor market outcomes of mothers with any child eligible to mothers with no child eligible. The data concerning the Medicaid expansions was compiled from various issues of "Major Changes in State Medicaid and Indigent Care Programs," published by the Intergovernmental Health Policy Project which contains detailed information on the date of implementation, range of ages the expansion covered, the new "Medicaid notch", and any phase in schedule for the expansion.[21]

To assign eligibility it was necessary to impute a birth month and birth year to

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[20] I use all 50 states and the District of Colombia. Some studies exclude Arizona, because it has a non-traditional Medicaid program, a demonstration project authorized by HCFA. Since I am more interested in eligibility for Medicaid rather than the delivery of services, I include it. I also include Alaska and Hawaii, but take into account that these states have higher poverty levels (conditional on family size) than the other states. Altogether, these three states amount to 501 observations.

[21] Where possible, the data was cross-checked against the Green Book (various issues), the Yellow Book (1988) and "A Catalogue of State Medicaid Program Changes," published by the National Governors Association.

each child, since the CPS only asks the child's age as of March 1 of the survey year. To do this, I assigned each child a month, randomly drawn from the year in which they could have been born, based on a uniform distribution.[22] This random assignment is a compromise because it induces measurement error. This measurement error will be more important for children born in the year of some birthday cutoff, who then have a chance of being misclassified, while it is less important for children born more than one year above or below the cut off date.

Table 4 shows the means of the covariates. They correspond closely to the Winkler (1991) extract for the 1986 March CPS. My sample of mothers is younger, at 31.5, since I screen on a younger set of women. 40% of the sample collects Medicaid, while only 32% participate in AFDC. A smaller fraction, 36%, are covered by employer provided health insurance, though the labor force participation rate is 68%. The demographic makeup stays fairly stable across the years (not shown), but there are observable differences between never married, divorced and separated women. I include dummy variables for different marital states in all econometric specifications presented. The never married women tend to be younger and more likely to participate in welfare than divorced or separated women. 24% of the sample has education of less than 12

[22] Actual birth patterns across months are slightly non-uniform. According to *Vital Statistics*, the empirical birth distribution for 1987 was 8.01%, 7.44%, 8.33% ,8.08%, 8.39%, 8.43%, 8.83%, 8.70%, 8.77%, 8.57%, 8.04% and 8.41% for the months January through December, respectively. With a uniform distribution, I assign a probability of 8.33% per month. I randomly assign birth months instead of assigning all children of a certain age a birth month to avoid misclassifying an entire cohort. That is, if I assigned all children a birth month of October (which is the mid-point since age in the CPS is taken in March), I risk classifying too many people as eligible. It is not vital to know the day the child was born, because virtually all the expansions were implemented on the first of the month.

years, while 32% have more than 12 years.[23] Finally, Table 5 shows the independent variable of most interest, GAIN%, is greater than zero for approximately 15% of the sample (while 42% of the sample is eligible for some expansion coverage), and conditional on GAIN% being positive, its mean is 20.77%. When the expansions have "bite" the notch is moved up by 20.77% of the Federal poverty level, on average. The maximum GAIN% is 85%, which shows that the OLDNOTCH% can be quite important in reducing the generosity of the expansion. When considering policy implications of changing the notch, I will show simulations moving the notch by 10, 25, 50 and 100 points (as measured by the FPL). That is, I simulate moving the Medicaid notch from OLDNOTCH% to OLDNOTCH%+25%.

[23] The March 1992 CPS modified the classification of years of education, grouping together 1 to 4 years schooling, 5 to 6, and 7 to 8. In these cases, I assigned each observation largest number of years of schooling. The vast majority of the observations did not fall into these groupings, however, and the 1992 CPS still has disaggregated figures for grades 9 and above.

5. CPS Results

The primary evidence I present on the effect of the Medicaid notch comes from probits that model labor force and AFDC participation. The model is specified as

$$(4) \quad LFP_i^* = \beta_0 + \beta_1 GAIN\% + \beta_2 X_i + \beta_3 TIME_t + \beta_4 STATE_j + \beta_5 KIDAGE_i + \beta_6 FAMSIZE_i + \epsilon_i$$

where (4) is the underlying index function for the probit. A similar equation is used with AFDC* on the left hand side. In this case, GAIN% is the independent variable of primary interest (with β_1 hypothesized to be positive here and negative for AFDC participation) and X_i contains other covariates, including number of children under age 6, mother's age and its square, mother's education and its square, and dummies variables for black, divorced, separated and residence in a central city. In addition, all specifications include dummy variables for family size and youngest child's age. Finally, different interactions of STATE, TIME and KIDAGE are included in different specifications. In practice we do not observe the underlying value LFP*, but instead only the discrete outcome:

$$(5) \quad LFP_i = \begin{matrix} 1 \ \text{if}\ LFP_i^* \geq 0 \\ 0 \ \text{if}\ LFP_i^* < 0 \end{matrix}$$

Assuming $\epsilon \sim N(0,1)$, and denoting $\Phi(\bullet)$ as the cumulative normal function gives the following probability:

$$(6) \quad Prob(LFP_i) = \Phi(\beta_0 + \beta_1 GAIN\% + \beta_2 X_i + \beta_3 TIME_t + \beta_4 STATE_j + \beta_5 KIDAGE_i + \beta_6 FAMSIZE_i)$$

The estimates incorporate controls for time-specific and state-specific shocks, through the inclusion of TIME, STATE and STATE*TIME interactions. With all three, identification of Medicaid's effect, β_1, is solely from three sources of within state variation:

- Differences in the generosity of the new benefit schedule due to differences in ages of children at a fixed point in time within a state.
- The differential treatment of children within a state over time by adoption of new expansions (either voluntarily or by mandate).
- The changes in the calculation of OLDNOTCH% within a state due to the FSA's changes in the ordering of child-care disregards and changes in maximum allowable deductions.

The TIME indicator controls for time specific, nationally uniform shocks to all mothers that might affect the outcomes of interest, such as a recession or national welfare reform. The STATE indicator controls for heterogeneity across states in unobservable, time-invariant aspects that affect both mothers with younger and older children uniformly such as attitudes towards welfare participation, differential reporting standards and aggressivity of administration. It also controls for differences in wage opportunities, emergency room access and health insurance opportunities within a state that do not change over time. Finally, the STATE*TIME interaction controls for time-varying, state-specific differences such as *changes* in the aggressivity of administration within a state or *changes* in the wage, health insurance opportunities or scope of Medicaid services that could affect the outcomes facing the groups within a state independent of the expansions.

It is possible that not all of the within state variation used to predict labor force participation or AFDC participation should be attributed to the effect of Medicaid. First, the outcome measures may be biased because of large differences in child-care costs due to children starting elementary school. Second, the Earned Income Tax Credit, which is intended to increase labor force participation, has not only changed over time, but has recently given *extra* subsidies to families with younger children, so any change in labor force participation could be due to that instead of the Medicaid expansions. Further, the FSA gives several exemptions from the work training program to mothers with younger children. Including indicator variables for the youngest child's age (KIDAGE) controls for the fact that mothers with younger children may simply behave differently.[24] To control for nationally uniform, time-specific shocks that affect mothers with younger children differently than mothers with older children (like the EITC or FSA provisions), I include KIDAGE*TIME interactions.

Finally, the inclusion of FAMSIZE dummies for different family sizes is important because it shows that the results are not spuriously driven by the fact that mothers with more children (who, by revealed preference, might have a larger distaste for market work) work less and *also* receive a smaller GAIN% (larger OLDNOTCH%) due to the child-care expense deductions. As a further "specification-check" on family sizes, the coefficients were reestimated restricting the sample to families of 2 to 6 persons. This allows a more accurate measure of OLDNOTCH%, since no imputation

[24] One could include KIDAGE*STATE interactions to control for the effect of differential starting ages of elementary school on outcomes. These controls would be computationally cumbersome and remove many degrees of freedom. I present results for this difference-in-differences-in-differences specification for the five largest states in my sample, California, New York, Texas, Florida and Illinois.

is needed for different family sizes and also reduces the intra-state variation in GAIN% due to the child-care expense deduction.

Primary specification: All States

Table 6 presents the primary evidence on the effect Medicaid notch. Labor force participation and AFDC participation equations were each estimated using a probit model.[25] One can see that in all specifications, the effect of the Medicaid notch is large. The inclusion of state controls is shown to be extremely important: the effect of Medicaid falls by 50%, but the estimates are still significant. Including STATE*TIME interactions increases the estimates. With this in mind, notice that severing the link to AFDC eligibility has a much stronger effect on reducing AFDC participation than increasing labor force participation. As shown in section 2, this is predicted by the model because the AFDC and labor force participation decisions are not mutually exclusive decisions. The final column in this table controls for time, state and age-specific shocks. The results here indicate that a 25% increase in GAIN% (moving the notch up by 25% of the FPL) for everyone will increase labor force participation by 3.32% and decrease AFDC participation by 4.61%.[26] Recall that table 5 showed that this increase in GAIN% corresponds to a reasonable movement in the Medicaid notch

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[25] The coefficient estimates using either the logit model or the linear probability model were extremely similar in sign and statistical significance. These may be obtained from the author upon request.

[26] The marginal probabilities were calculated as follows: For each observation, the predicted probability of AFDC or labor force participation was calculated at GAIN%=25 and then at GAIN%=0. The difference in these probabilities for each person was then averaged across the sample to obtain the marginal probability of increasing the notch by 25 points. The caseloads were then calculated as the change in participation divided by current participation (which is 68% for labor force and 32% for AFDC).

resulting from the expansions. This translates into a 14% decrease in the AFDC caseload and a 4.8% increase in the labor force pool. In relation to previous work, these estimates are larger than Blank and Winkler, who find very small effects, but less than the effects found by Moffitt and Wolfe. This is expected because the variation that Blank and Winkler use, average expenditure, may not capture much of the value of Medicaid to any specific family, while the health measures used by Moffitt and Wolfe attribute changes in preferences and changes in the wage that affect labor supply to Medicaid. The other covariates enter as expected: the number of children under age 6, the square of mother's age, black and residence in a central city usually are usually estimated in a labor force participation equation as negative and statistically significant. The mother's age, divorced, separated and the square of education enter into the labor force participation equation as positive and statistically significant. These covariates are of similar in sign and statistical significance to Winkler, who also used the CPS.[27] Table 7 shows both tobit estimation and least squares estimation (conditional on positve hours) of the effect of the expansions on annual hours worked. After controlling for time and state-specific shocks, the expansions lead to a substantial increase in hours. The tobit specification suggests that a 25% increase in the notch leads to 188 additional hours worked per year, roughly 5 extra weeks of full time work. This results will be utilized

[27] Several other checks are presented in the appendix. Table A.1 reestimates the coefficients using family sizes instead of family size dummies. The estimates are stronger in this case. Table A.2 shows estimates with family size dummies but uses youngest child's age instead of dummies. The results are strong in all specifications. Finally, Table A.3 restricts the sample to 15,802 mothers with families between 2 and 6 persons. This reduces the variation in OLDNOTCH% from the childcare deductions and also reduces the measurement error associated with imputing payment standards for larger family sizes. This restriction leads to smaller, but still significant estimates.

to forecast revenue effects for a policy simulation in section 6.

Difference-in-difference-in-difference specification: Five largest states

While it was computationally intractable to estimate the previous probit and tobit models in a difference-in-difference-in-differences (DDD) for all 50 states and D.C., I did estimate it for the five largest states in my sample, California, Florida, Illinois, New York and Texas. Restricting the sample to these states leaves 5,161 observations, approximately 32% of the entire sample. In this case, the DDD specification would control for both first order interactions, KIDAGE, STATE and TIME, and second order interactions, STATE*TIME, KIDAGE*TIME and KIDAGE*STATE. For the entire sample, I omitted KIDAGE*STATE because it would have added an additional 700 right hand side variables. By using several important states, we can gauge the impact of adding these additional controls. Table 8 presents probit estimates on AFDC participation and labor force participation and tobit estimates on annual hours of work. I include family size, number of children under age 6, mother's age and its square, mother's education and its square, and dummy variables for black, central city, divorced and separated in all specifications.[28] Column (1) presents estimates without any controls. However, youngest child's age is included linearly. As might be expected, the results in the restricted sample accord with the full sample. Moving the Medicaid income eligibility limit has a significant negative impact on AFDC participation and significant positive impact on labor force participation. The effect on AFDC

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[28] The estimates were also run replacing family size with family size dummies. In this case, the coefficients were somewhat weaker, though similar in sign and the DDD estimator is still significant.

participation is larger than the effect on labor force participation. In addition, increasing GAIN% has a large, positive, significant effect on total annual hours of work. Column (2) presents the difference-in-differences (DD) estimator. In this specification, I control for first order interactions by including KIDAGE, STATE and TIME controls. In comparison to column (1), the effect of Medicaid is significant for AFDC participation and annual hours of work, but is insignificant for labor force participation. The general pattern of larger coefficients on AFDC participation than labor force participation still holds. Column (3) controls for two of the three second order interactions, STATE*TIME and KIDAGE*TIME, which were also controlled for in the full sample. In this case, the coefficients on AFDC participation and annual hours of work are significant, while the coefficient on labor force participation is marginally significant. Finally, column (4) presents the DDD specification. This column controls for both first and second order interactions of KIDAGE, STATE and TIME. Quite remarkably, the coefficient estimates increase relative to column (2), the DD estimate and column (3). In this case, Medicaid has a significant negative impact on AFDC participation and significant positive impact on labor force participation. Finally, the impact on annual hours is positive and signicant.

Specification Checks on Entire Sample

I tried several checks, using more of the heterogeneity in the expansions. An indicator variable for eligibility yielded statistically insignificant results, although the coefficients are of the sign that theory predicts. This is not surprising, given the discussion in Section 2.2 on accurately characterizing the law change: the existence of the laws should not necessarily have a large effect, since it depends on the incentives that the entire welfare system offers. Next, GAIN% was interacted with an indicator variable for duration of health insurance coverage greater than some certain length, such as one or two years, with the reasoning that longer coverage should provide more of an incentive to leave welfare. The results of GAIN%*1(durat > t years) were similar to the coefficients reported in Table 6. Another attempt to use the duration of coverage margin was to exclude recipients with some fixed length of transitional coverage, for instance one year of coverage. The results, once again, were similar.

6. Policy Simulation

6.1 Revenue Consequences

This section presents the effects of moving the Medicaid notch from its old level to 185 percent of the Federal poverty level, corresponding to income of $18,611 for a family of three in 1989 dollars. This is the highest level that the notch moved to during the time covered by the CPS data.[29] Two effects can be estimated from moving the Medicaid notch. First, families will leave AFDC due to the expansions, so we can estimate the savings from reduced participation. Second, labor force participation will increase, thereby increasing the taxable base. To simulate these effects, I use the previous specifications that control for STATE, TIME and STATE*TIME interactions, corresponding to third column of Table 6 for labor force and AFDC participation and corresponding to the third column of Table 7 for annual hours of work.

This policy simulation is not binding for two percent of the sample, because OLDNOTCH% can be over 185%. For this group no behavioral response is expected as GAIN% is set equal to zero. The following equation is used to calculated the expected savings to the AFDC program:

$$(7) \quad \begin{matrix} AFDC \\ saving \end{matrix} = (Prob(AFDC)_{Oldnotch} - Prob(AFDC)_{185}) \ * \ maximum \ benefit$$

where the probabilities of AFDC participation are estimated at OLDNOTCH% and NEWNOTCH% respectively (which usually corresponds to 185%) and then multiplied

[29] As of July 1993, Minnesota moved the Medicaid notch to 275% of the FPL for all children under age 19. Other states have also expanded coverage using state-only funding.

by the maximum AFDC benefits in the woman's state taking into account family size.[30] The expected annual AFDC savings from this policy change is \$410.64 per family, averaged across all states. This annual saving ranges from \$185.16 in Connecticut to \$809.52 per family in Mississippi.

The second consequence of the expansions is increasing labor force participation and annual hours, thereby increasing the taxable base. To calculate the revenue implications from increased work, I predicted the increase in annual hours for each person from the Tobit specification presented in Table 10.[31] From this simulation, 87% of the sample were predicted to participate both before and after the policy change, 7% were predicted to change status from non-worker to worker after the change, and 6% were predicted to remain non-workers. The predicted change in annual hours is 466 hours for the first group, 284 hours for the second group, and zero for the third group, resulting in an average change of 428 hours. To gauge the revenue consequences of this, I conservatively assumed a wage rate, w, of \$4.25 (the minimum wage), and a marginal tax rate, τ, of 15%. Then the estimated increase in revenue is:

This results in an increase in tax revenue of \$273.15 per family per year (an increase in

[30] This overestimates the savings for those who are both on AFDC and working, since these recipients do not receive the maximum benefits.

[31] The change in annual hours depends on the person's labor force participation status evaluated at OLDNOTCH% and NEWNOTCH% (=185%). For those with $ANNHRS^*$ positive evaluated at both points, the change in hours is simply ($ANNHRS_{NEWNOTCH}$ - $ANNHRS_{OLDNOTCH}$), predicted using the individual's characteristics. For individuals in the sample with predicted $ANNHRS_{OLDNOTCH}$ negative and predicted $ANNHRS_{NEWNOTCH}$ positive, then the change in hours is $ANNHRS_{NEWNOTCH}$, since hours cannot be negative. Recall that the Tobit specification gives the correct estimated coefficients, taking into account the non-negativity constraint. Finally, for individuals with both $ANNHRS_{NEWNOTCH}$ and $ANNHRS_{OLDNOTCH}$ negative, they participate in the labor force neither before nor after the policy change, so their change in hours is zero.

(8) $$\Delta Revenue = w \cdot \tau \cdot \Delta(AnnualHours)$$

taxable income of $1821.02), with tax revenue effects ranging from $66.75 per family in Vermont to $492 per family in Alabama.

6.2 Cost Consequences

Several comments about the *costs* of the expansion, increased Medicaid expenditures, are appropriate. The effects described above hinge on takeup being concentrated only among former welfare recipients. In this case, the cost of Medicaid expenditures would be the same on or off welfare. If new enrollees were taking up coverage, then the expansions create new health care costs. Also if the expansions, which are an income effect, reduce hours among workers, then this affects the revenue calculation in equation (8). Second, there could be self-selection in takeup of the coverage. It is likely that only mothers with healthier children will leave welfare so the average expenditure for a child on Medicaid may overstate the appropriate amount. Recent data suggests that federal mandates are not the primary reason that Medicaid costs have risen so rapidly in recent years (Urban Institute, 1992). While total Medicaid spending rose from $52 billion in 1988 to $88 billion in 1991, and while more than half the new recipients were either pregnant women or young children whose coverage was mandated, this group accounts for only 10.8% of the spending growth. In contrast, the small growth among elderly and disabled beneficiaries accounts for nearly 20% of the spending growth.

To investigate the costs of the expansions, I present evidence on the effect of extending eligibility for Medicaid on the takeup of Medicaid coverage.[32] Based on Figure 5 (Medicaid Source Book: Background Data and Analysis, 1993) there was a dramatic increase in the number of children who were Medicaid beneficiaries without cash assistance in the late 1980s, whereas the number of children enrolled in the Medically Needy program and AFDC program remained relatively stable. In analyzing the costs of the expansions, it is necessary to examine whether the increase in child enrollees were mainly previous welfare recipients, in which case the costs of the expansions were low since the child would have collected Medicaid in any case, or were new enrollees, in which case the costs were high. I extracted all children under age 19 from the 1989, 1990, 1991 and 1992 CPS, resulting in 177,860 observations. 16.9% were on Medicaid. For these children, I again assign eligibility based on age, state of residence and time period. Table 11 shows the results of a linear probability model of Medicaid takeup on eligibility. If the expansions were primarily a "free gift" to people not on welfare, then we should expect the coefficient on eligibility to be close to one. On the other hand, if the expansions were used only as an avenue off welfare then we would observe no change in Medicaid takeup in response to increases in eligibility, so we would expect a coefficient close to zero. Column (1) shows the coefficient estimate on eligibility for the entire sample, where the first row controls for TIME effects only, the second row controls for both STATE and TIME effects and the third row controls for STATE, TIME and STATE*TIME interactions. These coefficients show that

[32] Currie and Gruber (1993) find low takeup rates for the population of women aged 15 to 44 for expansions related to pregnancy.

eligibility for the expansions increased Medicaid takeup by approximately 8%, and is estimated very precisely. Thus, it seems that most of the takeup is not coming from children who were newly eligible for Medicaid. This finding is robust to each of the three specifications.

We might hypothesize that the expansions would have different effects on different groups, however. First, it is possible that children in one parent households have higher takeup rates than children in two parent households, since their mothers are more likely to know about the welfare system. Second, takeup rates should be higher in poor households than in rich households since eligibility is still conditioned on income. While both the marriage and income margins are endogenous, stratifying on them is a useful check on the result.

Columns (2) and (3) separate the sample into husband-wife couples and female heads (children in male headed families are omitted), resulting in 130,370 and 40,897 observations, respectively. The effect of the expansions on takeup is much smaller for traditional families in column (2) than female headed families in column (3). These estimates suggest an increase of approximately 4.6% in Medicaid takeup for children in two parent families and a much larger increase of 20% for children in female headed families. Once again, these coefficients are estimated very precisely.

Columns (4), (5) and (6) stratify on income. Column (4) includes all children whose family income is less than 100% of the FPL and column (5) includes all children whose family income is between 100% and 200% of the FPL. This results in 36,895 and 39,743 observations respectively. As expected, children in poorer families took up

Medicaid coverage much more than children in richer families. The eligibility expansions lead to an increase of 12 to 13% in families with income under 100% of the FPL, while only 7% in families with income between 100% and 200% of the FPL.

Thus, the takeup regressions demonstrate a consistent story. To a large extent, the expansions were not a free gift to newly eligible children. Takeup was far from 100%, though it was higher in groups that were more likely to know about the expansion. The total savings of the expansions might be as high as $700 per family. While this is an upper bound, takeup was not very high for non AFDC families.

7. Concluding Remarks and Extensions

This paper has shown that Medicaid has a substantial effect on labor supply and welfare participation. Using recent Medicaid health insurance expansions for children which severed the link to AFDC and conditioned eligibility on the child's birthday, I show that substantial decreases in the AFDC caseload could occur from completely severing the link between AFDC eligibility and Medicaid eligibility. This would also occur through the introduction of universal health insurance. In addition, expanding eligibility could result in significant savings in AFDC spending and some growth in the taxable base due to increased working hours. A note of caution is appropriate: while large reductions in the welfare rolls is probable from severing the link, such an expansion could cause reductions hours for women currently working since such an expansion is a pure income effect. This could mitigate possible savings since the taxable base would then shrink and, more importantly, the state would be responsible for paying the health care costs of these children. I present evidence, however, that takeup among newly eligible children has been low. One reason higher takeup did not have occurred with the expansions in the 1980's could be due to informational asymmetries between welfare recipients and non-welfare recipients about the existence of the expansions. Any large scale health insurance expansion would surely be known by this second group.

The expansions might shed light on related questions, as well. In future work, I will investigate the effect of the expansions on takeup of Medicaid and transitions out of welfare. Using panel data, I can distinguish between takeup among former welfare recipients and newly eligible non-participants, which bears on the costs of the expansions. In addition, I will examine whether the quality of job matches changed from the expansions and estimate a structural labor supply model. Since job searchers like search on both wage and health insurance, then the expansions may have increased intensity of job search of type of job match. Finally, I plan to jointly model the marriage and labor supply decision and examine the further effects of the expansions in this model.

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FIGURE 1
Budget set *before* Medicaid expansions

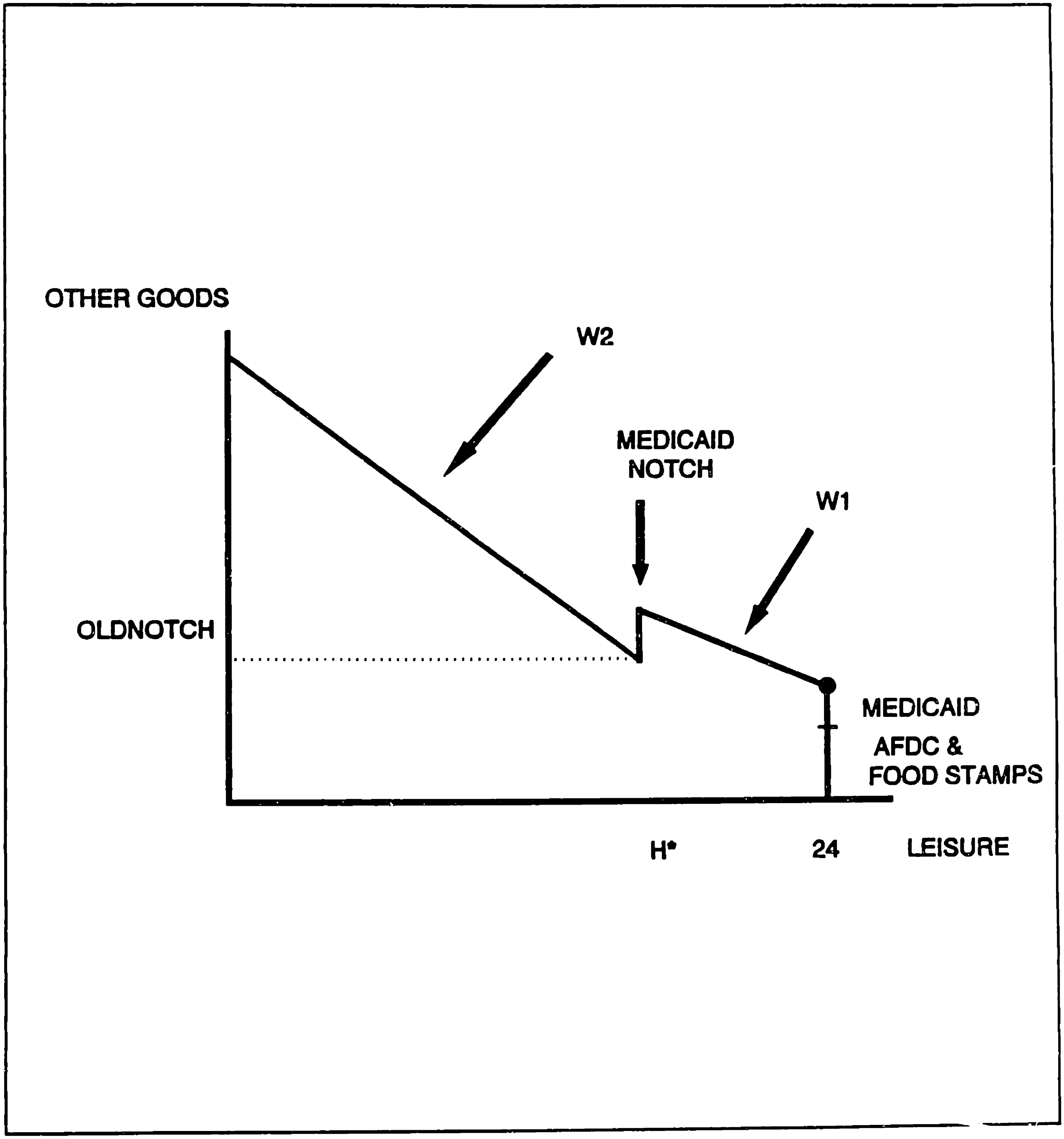


FIGURE 2
Budget set *after* Medicaid expansions

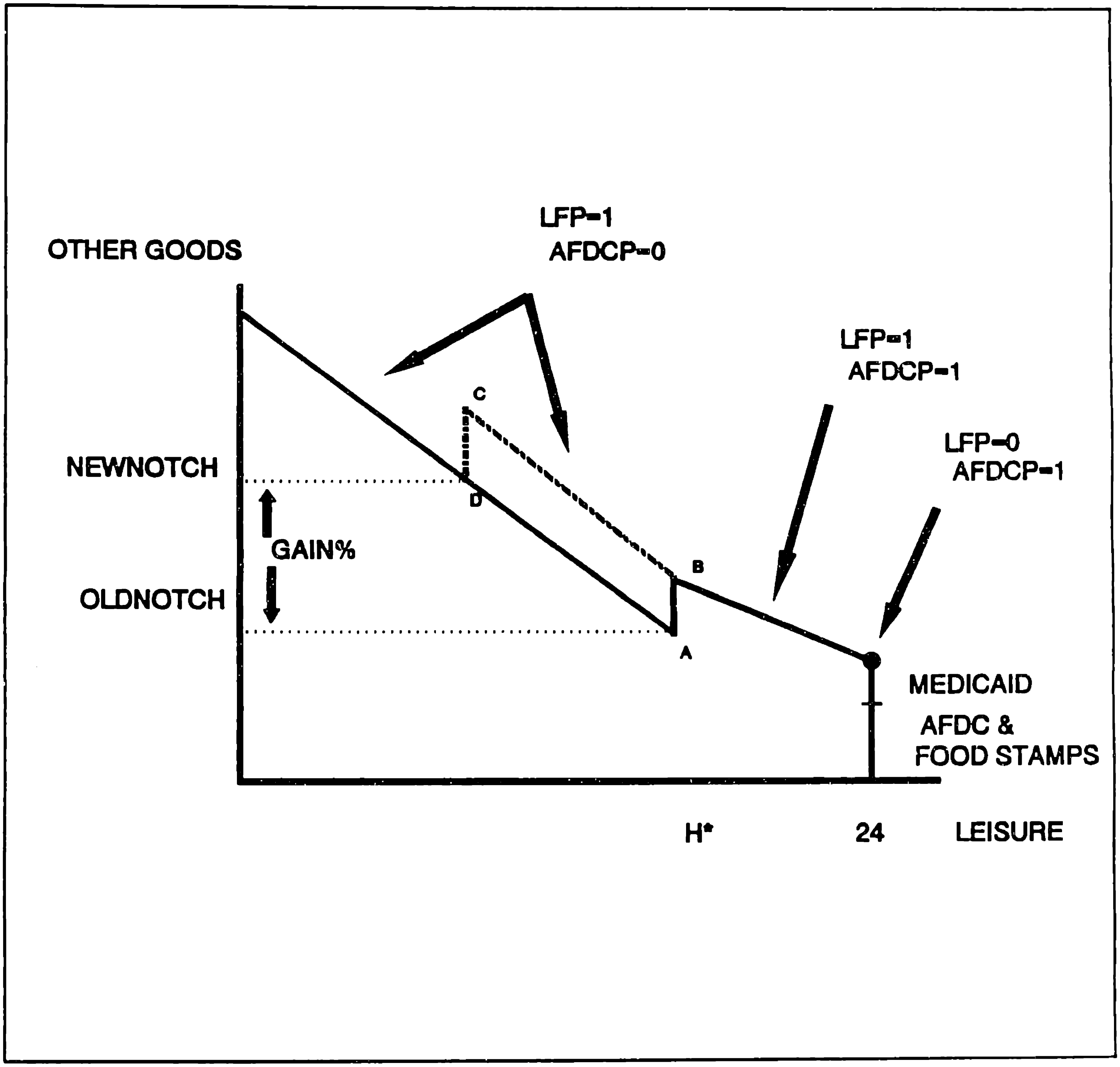


FIGURE 3
The effect of health status on the budget set

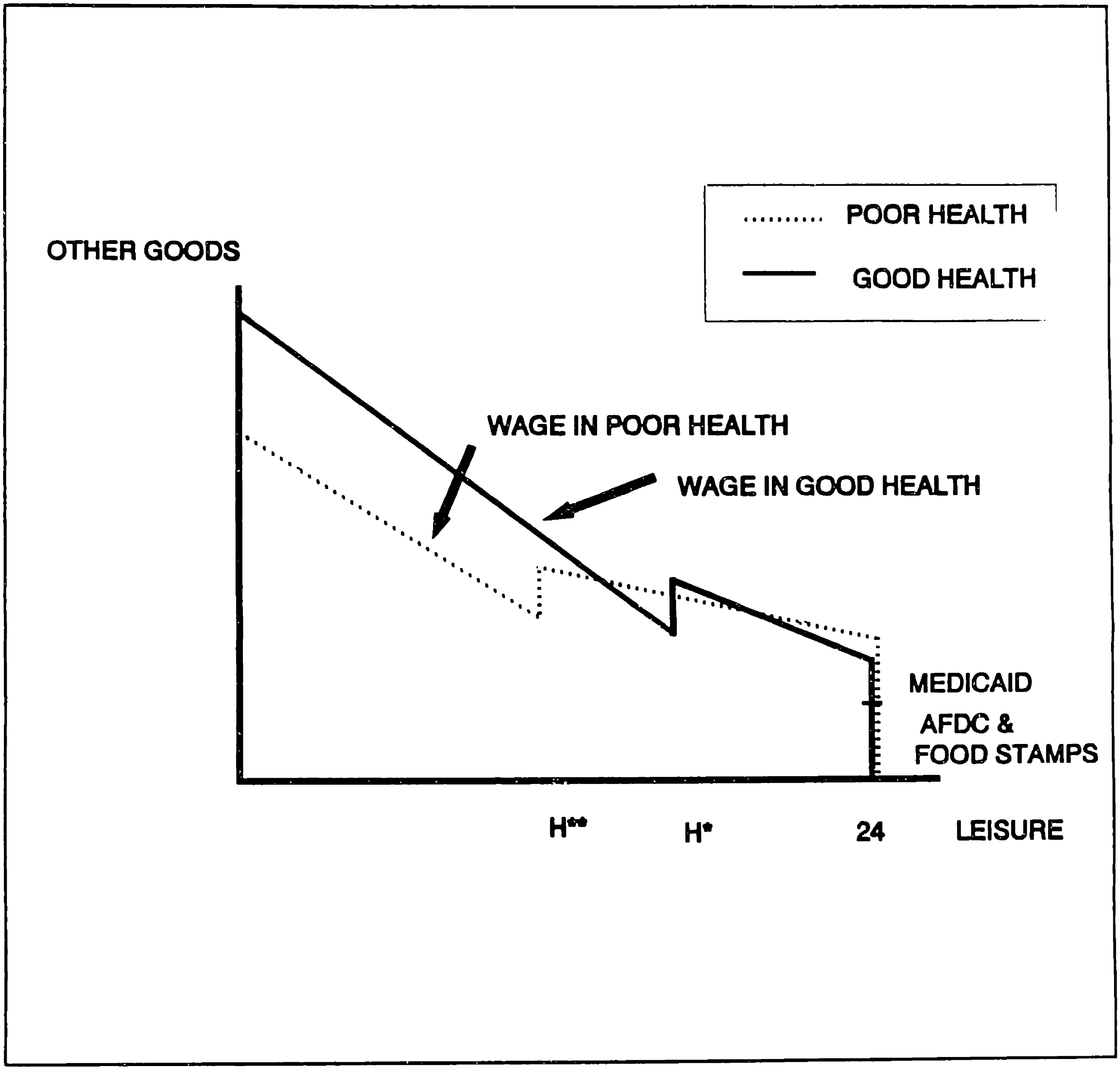
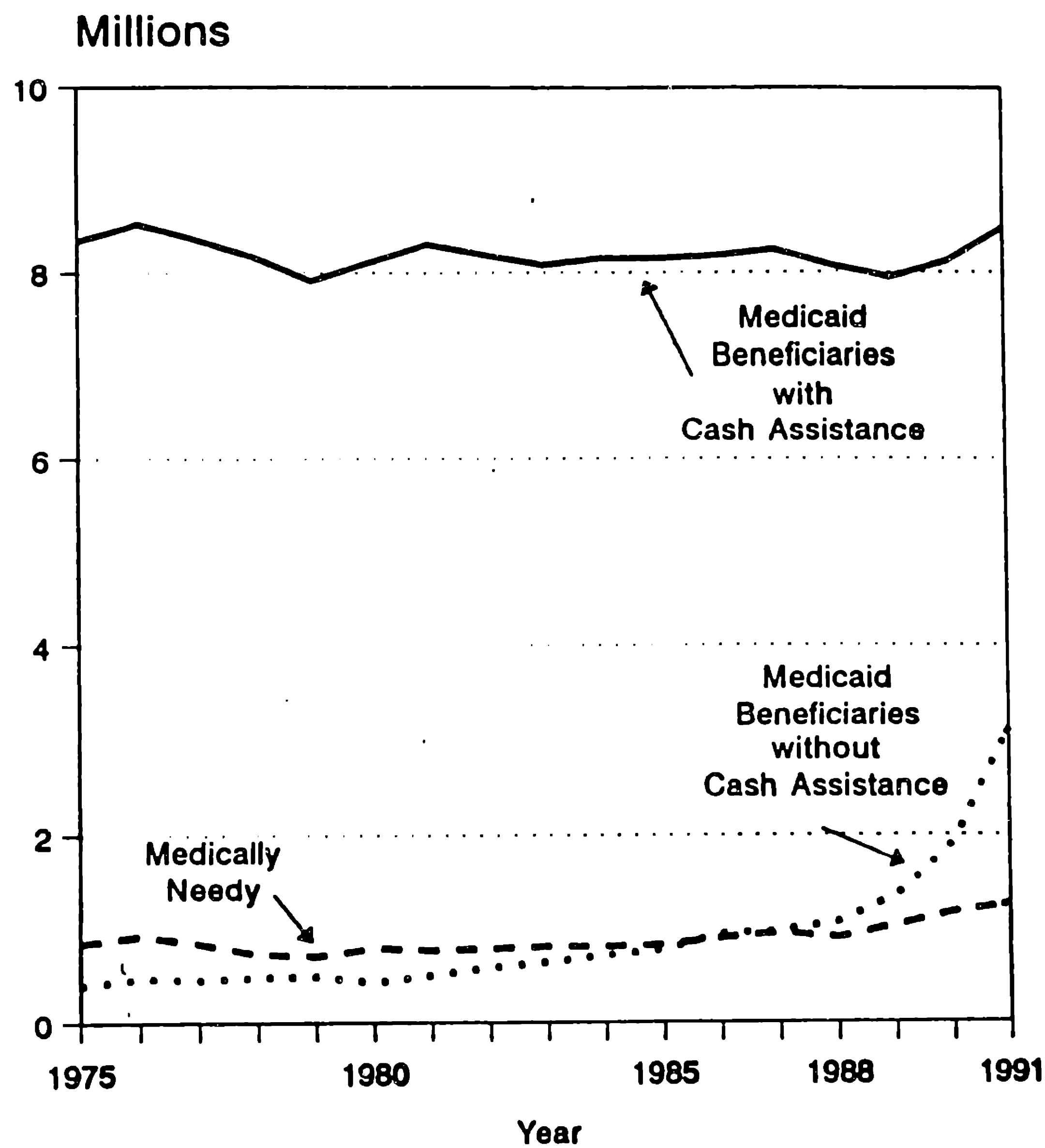


FIGURE 4

Child Beneficiaries by Eligibility Status
FY 1975-FY 1991



Source: Figure prepared by Congressional Research Service based on data from HCFA.

Table 1

Earnings and Benefits for a Mother with Two Children with Daycare expenses after 4 months on job (January 1991) -- (Pennsylvania)

Earnings	EITC	AFDC	Food Stamps	Medicaid $2304	Social Security	Federal Income Tax	State Income Tax	Work Expenses	Income
0	0	5052	2166	**Yes**	0	0	0	0	9522
2000	346	4892	1854	**Yes**	153	0	0	600	11143
4000	692	3292	1974	**Yes**	306	0	0	1200	10756
5000	865	2492	2034	**Yes**	383	0	0	1500	10812
6000	1038	1692	2094	**Yes**	459	0	0	1800	10869
7000	1211	892	2154	**Yes**	536	0	0	2100	10925
8000	1235	0	2241	**Yes**	612	0	0	2400	10768
9000	1235	0	2061	**Yes**	689	0	38	2700	11173
10000	1235	0	1811	**No**	765	0	210	3000	9141
15000	772	0	0	**No**	1148	0	315	4200	10109
20000	154	0	0	**No**	1530	283	420	5200	12721
30000	0	0	0	**No**	2295	1943	630	5400	19732
50000	0	0	0	**No**	3825	6405	1050	5400	33320

Source: 1991 edition of "Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means," page 590.

Table 2: State Eligibility Thresholds for Children

State	Age limit: 1/88	Newnotch %	Age limit: 12/89	Newnotch %
Alabama			1	100
Alaska			2	100
Arizona	2	100	6	100
Arkansas	2	75	6	100
California			5	100
Colorado			1	75
Connecticut	1	100	1	185
Delaware	1	100	3	100
D.C.	2	100	2	100
Florida	2	100	6	100
Georgia			3	100
Hawaii			4	100
Idaho			1	75
Illinois			1	100
Indiana			3	100
Iowa			6	100
Kansas			5	100
Kentucky	2	100	2	100
Louisiana			6	100
Maine			5	100
Maryland	1	100	2	100
Massachusetts	2	100	5	100
Michigan	2	100	3	100
Minnesota			8	100
Mississippi	2	100	5	100
Missouri	2	100	3	100
Montana			1	100
Nebraska			5	100
Nevada			6	75
New Hampshire			1	100
New Jersey	2	100	2	100
New Mexico	2	100	3	100
New York			1	185
North Carolina	2	100	7	100
North Dakota			1	75
Ohio	1	100	1	100
Oklahoma	2	100	2	100
Oregon	2	85	3	85
Pennsylvania	2	100	6	100
Rhode Island	2	100	6	100
South Carolina	1	100	6	100
South Dakota			1	100
Tennessee	2	100	6	100
Texas			4	100
Utah	2	100	1	100
Vermont	2	100	6	225
Virginia			1	100
Washington	2	90	8	100
West Virginia	2	100	6	100
Wisconsin			1	120
Wyoming			1	100

Table 3
Summary Statistics

	All female heads	Never married only	Divorced only	Separated only
OBS	16062	6247	6478	3337
mother's age	31.5	27.4	34.7	32.9
youngest child's age	5.74	3.89	7.54	5.73
oldest childs's age	8.47	6.15	10.24	9.38
# children $<$ age 6	.705	.96	.418	.768
# children $\geq$ age 6	1.13	.75	1.38	1.35
years education $<$ 12	.24	.31	.156	.29
years education $>$ 12	.32	.23	.42	.28
black	.29	.48	.13	.25
white	.66	.47	.82	.71
north	.22	.25	.18	.25
south	.33	.34	.32	.35
west	.21	.17	.24	.21
medicaid particip.	.40	.55	.25	.39
AFDC participation	.32	.45	.20	.33
employer provided health insurance	.36	.24	.50	.30
labor force particip.	.68	.55	.81	.65
eligible for expansion?	.42	.57	.26	.43
real earnings (1987 dollars)	8154	5144	11468	7353
25th percentile	0	0	2062	0
50th percentile	5045	947	9099	3996
75th percentile	13394	8503	17117	11904
90th percentile	21258	15650	25225	19792

Source: Author's tabulations of Current Population Survey.

Table 4

Summary statistics for GAIN%

Sample	Obs	Mean (σ)	Percentiles 10th	25th	50th	75th	90th
Entire sample	16062	.0337 (.0971)	0	0	0	0	.1336
If eligible for expansion	6782	.0800 (.1364)	0	0	0	.1320	.2779
If GAIN% is positive	2613	.2077 (.1477)	.0321	.1021	.1833	.2799	.4194

GAIN%=max{NEWNOTCH%-OLDNOTCH%,0}

GAIN% is the *additional* incentive to leave welfare due to the Medicaid expansions, as measured as a percentage of the federal poverty level. NEWNOTCH% is the percentage of the federal poverty level that the recipient could earn up to after the expansions, typically 100% or 133% if eligible, otherwise only up to OLDNOTCH% if ineligible.

Source: Author's tabulations of Current Population Survey.

Table 5

Probit estimates from Current Population Survey, 1988 to 1991

	Labor Force Participation		
GAIN%	.7205 (.1291)	.3840 (.1509)	.4731 (.1679)
0 → 100:	.1788	.1023	.1223
0 → 50:	.0992	.0536	.0649
0 → 25:	.0518	.0273	.0332
0 → 10:	.0212	.0110	.0135
	AFDC Participation		
GAIN%	-1.1052 (.1325)	-.5188 (.1544)	-.6492 (.1714)
0 → 100:	-.2503	-.1366	-.1638
0 → 50:	-.1494	-.0728	-.0890
0 → 25:	-.0801	-.0374	-.0461
0 → 10:	-.0331	-.0151	-.0187
State	No	Yes	Yes
State*Time	No	No	Yes

Standard errors are in parenthesis. The marginal effects are interpreted as moving the notch by some percentage of the FPL. The sample size is 16,062 observations. The other covariates include:a constant, number of children under age 6, mother's age and its square, dummy variables for divorced and separated, years of education and its square, a dummy variable for black and a dummy variable for residing in a central city. All specifications include FAMSIZE, TIME, KIDAGE and KIDAGE*TIME indicators.

Additional coefficients from Table 5

Probit estimates from Current Population Survey, 1988 to 1991

	Labor Force Participation		
# kids < age 6	-.1230	-.1382	-.1392
	(.0281)	(.0286)	(.0289)
age	.0556	.0597	.0618
	(.0119)	(.0121)	(.0122)
age^2	-.0008	-.0008	-.0008
	(.0001)	(.0001)	(.0001)
divorced	.4207	.3863	.3921
	(.0307)	(.0314)	(.0317)
separated	.1892	.1742	.1751
	(.0317)	(.0322)	(.0325)
black	-.0067	-.0713	-.0705
	(.0267)	(.0301)	(.0304)
central city	-.2773	-.2267	-.2213
	(.0240)	(.0271)	(.0273)
education	-.0379	-.0483	-.0484
	(.0212)	(.0216)	(.0217)
education^2	.0082	.0086	.0087
	(.0009)	(.0010)	(.0010)
log-likelihood	-8391	-8196	-8123
	AFDC Participation		
# kids < age 6	.1686	.1802	.1829
	(.0286)	(.0290)	(.0293)
age	-.0048	-.0007	-.0012
	(.0122)	(.0124)	(.0125)
age^2	-.0001	-.0002	-.0002
	(.0001)	(.0001)	(.0001)
divorced	-.3556	.3528	-.3555
	(.0303)	(.0310)	(.0314)
separated	-.2640	-.2358	-.2376
	(.0318)	(.0323)	(.0326)
black	.0760	.2168	.2205
	(.0265)	(.0301)	(.0303)
central city	.2001	.1889	.1888
	(.0240)	(.0271)	(.0274)
education	.1340	.1341	.1335
	(.0213)	(.0217)	(.0219)
education^2	-.0116	-.0121	-.0121
	(.0010)	(.0010)	(.0010)
log-likelihood	-8560	-8311	-8226
State	No	Yes	Yes
State*Time	No	No	Yes

Standard errors are in parenthesis. The sample size is 16,062 observations. The other covariates include: a constant and family size dummies ranging from 3 to 12 (size of 2 is omitted). TIME, KIDAGE and KIDAGE*TIME indicators included in all specifications.

Table 6

Tobit and OLS estimates from Current Population Survey, 1988 to 1991
Family size dummies included

	Annual hours of work					
	Tobit estimation with censoring at annual hours=0			Ordinary Least Squares conditioned on annual hours>0		
GAIN%	861 (106)	653 (118)	754 (127)	386 (75)	396 (82)	464 (89)
log-likelihood	-98566	-98331	-98261	---	---	---
R^2	---	---	---	.1351	.1479	.1579
censored obs	4897	4897	4897	4897	4897	4897
State	No	Yes	Yes	No	Yes	Yes
State*Time	No	No	Yes	No	No	Yes

Standard errors are in parenthesis. The sample size is 16,062 observations. The other covariates include: a constant, number of children under age 6, youngest child's age, mother's age and its square, dummy variables for divorced and separated, years of education and its square, a dummy variable for black and a dummy variable for residing in a central city. All specifications include TIME and FAMSIZE dummies.

Table 7

Sample Restricted to California, Florida, Illinois, New York and Texas
Current Population Survey, 1988 to 1991

	(1)	(2) DD estimator	(3)	(4) **DDD estimator**
		Probit on Labor Force Participation		
GAIN%	1.1807	.2707	.3614	**.8080**
	(.2110)	(.2561)	(.2902)	**(.3668)**
		Probit on AFDC Participation		
GAIN%	-1.4858	-.6353	-.6592	**-1.0783**
	(.2213)	(.2660)	(.2991)	**(.3749)**
		Tobit on Annual Hours of Work		
GAIN%	1442	662	786	**987**
	(191)	(225)	(251)	**(316)**
KIDAGE, STATE and TIME	No	Yes	Yes	Yes
STATE*TIME	No	No	Yes	Yes
KIDAGE*TIME	No	No	Yes	Yes
KIDAGE*STATE	No	No	No	Yes

Standard errors in parenthesis. Sample includes 5,159 observations in all specifications. Column (2) is the difference-in-differences estimator and column (4) is the difference-in-difference-in-differences estimator. Covariates include: a constant, family size, number of own children under 6, mother's age and its square, mother's education and its square, dummies for black, divorce, separated and residence in a central city. Column (1) includes youngest child's age entered linearly.

Table 8

Linear probability model estimating Medicaid takeup in response to eligibility
Current Population Survey, 1988 to 1991

	(1) Entire sample	(2) Husband-wife families	(3) Female headed families	(4) Income under 100% FPL	(5) Income between 100 and 200%
number of children	177,866	130,370	40,897	36,895	39,743
	Controls for TIME effects only				
Eligible for expansion?	.0829 (.0021)	.0469 (.0018)	.1985 (.0057)	.1274 (.0058)	.0733 (.0042)
	Controls for STATE and TIME effects				
Eligible for expansion?	.0823 (.0021)	.0463 (.0018)	.2007 (.0057)	.1253 (.0057)	.0709 (.0042)
	*Controls for STATE, TIME and STATE*TIME effects*				
Eligible for expansion?	.0846 (.0021)	.0470 (.0018)	.2077 (.0058)	.1317 (.0058)	.0730 (.0043)

Standard errors in parenthesis. Entire sample contains all children under age 19 in the CPS.

APPENDIX I: Legislative changes in the 1980's

Sixth Omnibus Budget Reconciliation Act, 1986 (SOBRA 86): *Pregnant women and children:* Permitted States, without raising their cash assistance standards, to extend categorically need coverage to certain target groups with incomes below the Federal poverty line who were not previously eligible for such coverage. The target group was primarily composed of pregnant women, infants and young children. These are persons who are not eligible for AFDC nor included under the mandatory coverage provisions because their incomes and/or resources exceed AFDC standards. Under SOBRA 86, the provision was effective as of April 1, 1987. Beginning in fiscal year 1988, States could increase the age level by one in each fiscal year until all children under age five were included. States could not elect to cover one age group until all younger age groups were covered.

Omnibus Budget Reconciliation Act, 1987 (OBRA 1987): Expanded the options of SOBRA 86, permitted States to accelerate coverage of children under age 5 whose income is below the poverty line. Effective July 1, 1988, States can cover children under age 2, 3, 4 or 5 (as selected by the State) who are born after September 30, 1983. Effective October 1, 1988, States can expand coverage to children under age 8 born after September 30, 1983. Allows states to extend Medicaid eligibility to pregnant women and infants up to age one with incomes up to 185% of the federal poverty level. States have the option of choosing any percentage of the federal poverty level between 100% and 185%, but if they choose a level exceeding 150%, they can charge the woman a monthly premium, which cannot be more than 10% of the family income that exceeds 150% of the federal poverty level. States are <u>required</u> to cover children through age 5 in FY 89 and age 6 in FY 90 in families who meet that state's AFDC income and resource standards.

Medicare Catastrophic Coverage Act, 1988 (MCCA 88): *Pregnant women and infants:* Previously, states were allowed to cover pregnant women and infants, up to age one, in households with incomes up to the federal poverty level. This legislation <u>requires</u> states to cover people in these two groups on a phased-in schedule: as of July 1, 1989, states must cover infants up to age one and pregnant women at or below 75% of the federal poverty level As of July 1, 1990, this moves up to 100% of the federal poverty level. States are further prohibited from reducing their AFDC payment and eligibility levels below those in effect on May 1, 1988.

Family Support Act, 1988 (FSA 88): *Work transition coverage:* Requires states to continue Medicaid coverage for 12 months for families who received AFDC in three of the previous six months, but became ineligible for assistance because of increased work hours or increased earnings, or due to expiration of the earned-income disregards. The provision will take effect starting April 1, 1990. During the first six months of extended coverage, states can pay the family's health insurance premiums, deductibles and coinsurance for coverage available through the worker's health plan, if one is offered, or through any other state-sponsored health insurance plan, as an alternative to Medicaid. During the second six months of extended Medicaid coverage, states have the option to offer less than a full Medicaid benefits package and can enroll the individual in a private group plan or an HMO. Families whose incomes exceed 185% of the federal poverty level would not qualify for this assistance. Those with family incomes between 100% and 185% of the poverty guidelines could be charged a premium during the second six-month period, but the amount cannot exceed 3% of the family's gross monthly earnings.

Omnibus Budget Reconciliation Act, 1989 (OBRA 89): *Pregnant women and children:* Requires states to extend Medicaid coverage to all pregnant women and children up to age six with family incomes up to 133% of the federal poverty level ($13,380 for a family of three). Effective April 1, 1990.

Omnibus Budget Reconciliation Act, 1990 (OBRA 90): *Children:* Starting July 1, 1991, States are required to cover all children who are under age 19, who were born after September 30, 1983, and whose family income is below 100% of the Federal poverty level. (Coverage of such children through age 7 has been optional since OBRA 87). The 1983 start date means that coverage of 18-year-olds will take effect during fiscal year 2002. *Pregnant women and infants:* States are permitted, but not required, to cover these groups at any rate between 133% and 185% of the Federal poverty level.

Table A.1

Probit estimates from Current Population Survey, 1988 to 1991

	Labor Force Participation		
GAIN%	.7637 (.1282)	.4436 (.1492)	.5526 (.1655)
0 → 100:	.1872	.1166	.1401
0 → 50:	.1048	.0616	.0753
0 → 25:	.0549	.0316	.0388
0 → 10:	.0225	.0128	.0158
	AFDC Participation		
GAIN%	-1.1751 (.1316)	-.6271 (.1527)	-.7995 (.1690)
0 → 100:	-.2600	-.1608	-.1938
0 → 50:	-.1578	-.0873	-.1082
0 → 25:	-.0851	-.0451	-.0566
0 → 10:	-.0353	-.0184	-.0231
State	No	Yes	Yes
State*Time	No	No	Yes

Standard errors are in parenthesis. The marginal effects are interpreted as moving the notch by some percentage of the FPL. The sample size is 16,062 observations. The other covariates include: a constant, family size, number of children under age 6, youngest child's age, mother's age and its square, dummy variables for divorced and separated, years of education and its square, a dummy variable for black and a dummy variable for residing in a central city. Family size is entered linearly and all specifications include TIME, KIDAGE and KIDAGE*TIME indicators.

Table A.2

Probit estimates from Current Population Survey, 1988 to 1991-- Family sizes of 2 to 6

		Labor force participation	
GAIN%	.6252 (.1249)	.3007 (.1440)	.3575 (.1586)
0 → 100:	.1594	.0818	.0951
0 → 50:	.0872	.0424	.0497
0 → 25:	.0452	.0215	.0253
0 → 10:	.0184	.0087	.0102
		AFDC participation	
GAIN%	-1.0057 (.1281)	-.4297 (.1474)	-.5476 (.1618)
0 → 100:	-.2346	-.1156	-.1418
0 → 50:	-.1377	-.0608	-.0759
0 → 25:	-.0733	-.0311	-.0391
0 → 10:	-.0303	-.0126	-.0158
State	No	Yes	Yes
State*Time	No	No	Yes

Standard errors are in parenthesis. The marginal effects are interpreted as moving the notch by some percentage of the FPL. The sample size is 15,802 observations. The other covariates include: a constant, dummy variables for different family sizes, number of children under age 6, youngest child's age, mother's age and its square, dummy variables for divorced and separated, years of education and its square, a dummy variable for black and a dummy variable for residing in a central city. Includes indicator variable for TIME.

Table A.3

Probit estimates from Current Population Survey, 1988 to 1991

	Labor force participation		
GAIN%	.6475 (.1235)	.3378 (.1418)	.4083 (.1559)
0 → 100: 0 → 50: 0 → 25: 0 → 10:	.1649 .0904 .0469 .0191	.0914 .0476 .0242 .0097	.1078 .0566 .0289 .0117
	AFDC participation		
GAIN%	-1.0380 (.1268)	-.5084 (.1453)	-.6416 (.1592)
0 → 100: 0 → 50: 0 → 25: 0 → 10:	-.2411 -.1420 -.0757 -.0313	-.1347 -.0717 -.0368 -.0149	-.1628 -.0884 -.0457 -.0186
State State*Time	No No	Yes No	Yes Yes

Standard errors are in parenthesis. The marginal effects are interpreted as moving the notch by some percentage of the FPL. The sample size is 16,062 observations. The other covariates include: a constant, number of children under age 6, youngest child's age, mother's age and its square, dummy variables for divorced and separated, years of education and its square, a dummy variable for black and a dummy variable for residing in a central city. Includes TIME and family size indicators, but omits youngest child's age.

Chapter Two

Will Extending Medicaid to Two Parent Families Encourage Marriage?

1. Introduction

The past three decades have witnessed drastic changes in the composition of families. More than 25% of all births were to unmarried mothers in 1990, compared to just 5% in 1960. The number of one-parent families increased 165% between 1970 and 1990 to 10.1 million families, with the vast majority headed by women. These changes in family structure coincide with changes in the generosity and scope of the U.S. welfare system. While Aid to Families with Dependent Children (AFDC), which provides cash relief to the poor, was introduced in 1935, Medicaid health insurance and Food Stamps were not introduced until the 1960s.

One distinguishing feature of these programs is their categorical nature. Besides satisfying income requirements, a recipient typically has had to reside in a non-traditional family to be eligible for AFDC and Medicaid. Losing these benefits may be construed as a marriage tax that could affect the decision to marry. Other events were occuring over this period, however, all of which could contribute to changes in family structure. For instance, changes in access to abortion and birth control availability could affect fertility decisions. Changes in the relative wages of men and women and the supply of eligible men could affect the marriage decision.

If the design of benefits does affect family structure, this bears on other issues concerning the welfare system. It is possible that children are adversely affected by living in one-parent families rather than two-parent families. Ellwood (1989) finds that as many as 61% of all children in single-parent households will be poor throughout most of the early years of their childhood. Since one-parent households are usually poorer than two-parent households, children in these one-parent households have a much higher probability of exposure to poverty. Exposure to poverty could consequently affect the child's educational attainment and future earnings leading to further dependency.

Much previous work has looked at the effect of AFDC cash benefits on marital dissolution. For the most part, these studies have found small, but statistically significant positive effects of AFDC benefits on female headship (Danziger, et. al., 1982; Ellwood and Bane, 1985; Moffitt, 1990b; Hoynes, 1993b). These weak effects should not be surprising, since time series trends show a drop in real AFDC benefits for the median state of around 40% since 1970, while female headship has continued to rise (Green Book, 1993). This understates the total benefit of participating in welfare however, since in-kind benefits such as Medicaid and Food Stamps are usually not included. When one includes the "value" of such benefits, the total benefit growth is in line with income growth over this period (Moffitt, 1990a).[1] Medicaid, in particular, has become an increasingly important component in the welfare package because medical care costs have far outpaced inflation in recent decades. For example, in fiscal year 1991 total expenditure on

[1] It should be noted that in-kind benefits are difficult to value, particularly so for Medicaid, since health status, family size and degree of risk aversion all affect its valuation. Many authors have typically used the average expenditure in an individual state for Medicaid's value, but an average may not be particularly informative since health expenditures are highly skewed: many people have no health care utilization whereas others have very high expenditure. In any case, no such valuation will be necessary in this analysis, because I will present differences in marriage rates for two plausibly identical groups including their valuation of health insurance.

Medicaid for welfare recipients, $22 billion, exceeded total spending on AFDC, $21 billion (Green Book, 1993).

While the effects of AFDC cash benefits on marriage decisions are quite well established, I extend the discussion. In this paper, I will examine the effect of extending health insurance to children in two-parent families on the decision to marry. Health insurance may have a different effect than cash benefits, since many potential husbands are unable to replace Medicaid with employer provided health insurance, whereas the loss of cash benefits is more easily offset. Moffitt (1983) presents a model of "welfare stigma" where AFDC income is valued less (in utility terms) than equivalent income from other sources. This implies that an increase in AFDC benefits may not translate into much higher divorce rates since welfare participation is stigmatizing.

To examine Medicaid's effect on marriage decisions, I employ an intriguing "reverse-experiment" that occurred in the 1980s -- health insurance expansions for children. These expansions severed the link to AFDC eligibility, which was traditional avenue for Medicaid eligibility, along two dimensions: the family structure margin and the income margin. As I will show, these expansions usually increase the incentives to marry. This can be construed as a "reverse experiment" from previous work since the expansions increase the incentive for marital unions, while other sources of variation like higher AFDC benefits usually increased the incentive for marital dissolution. There is no compelling reason to expect these two effects to be equal and opposite in size, since the costs and benefits of going between the two states may be asymmetric. For instance, if stigma is usually attached with divorce or participating in AFDC, then increasing the incentives to get divorced may have smaller effects than from increasing the incentives to get married.

These expansions created three dimensions of variation to identify the effect of extending Medicaid on marriage: mothers within a state at a point in time, mothers across states at a point in time, and mothers within a state over time. One appealing feature is that the expansions allow me to exploit within state variation, since they create arbitrary eligibility cut-offs based on the birthday of the child.[2] To identify Medicaid's effect I rely primarily on the within state variation. To analyze Medicaid's effect within a state, children ineligible for the expansion based on their birthday can serve as a "control" for other events occurring within a state, such as attitudes toward female headship, relative wages of men and women, and shifts in the supply curve of marketable men.

I reach three main conclusions. First, extending Medicaid to all children in a family unit has a large positive effect on the probability of marriage relative to those families ineligible for the expansions. By doing so, the probability of marriage increases by 2.61%, which translates into a 3.48% increase in the pool of married couples. Second, I find that whom the benefits are assigned to is important to the findings. Since a woman is usually assigned custody of children upon divorce, her

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[2] Ellwood and Bane (1985) note that work using cross-sectional variation only may confound the effect of AFDC generosity with unmeasured differences that also vary across states, such as attitudes toward female headship and out of wedlock child bearing. To date, their study which uses the "expected AFDC benefit" for a family based on observable demographic charactersistics, is the only one that uses within state variation at a point in time. Hoynes (1993b) uses variation within a state, over time for identification.

children may still be eligible for Medicaid because the expansions also relax the income limit. Consistent with the theoretical predictions in section 4, the estimated effect of extending health insurance increases in size after controlling for outflows from marriage from this "independence effect". The effect nearly doubles -- extending health insurance increases the probability of marriage by 4.36%, translating into an increase of 5.81% increase in the pool of married couples. These results are robust to the inclusion of state fixed effects, which could control for unobservable differences in attitudes toward female headship and divorce that could vary across states, and have been found to be important in the analysis of AFDC cash benefits (Ellwood and Bane, 1985; Hoynes, 1993b). I also provide some evidence that the expansions had the most impact on families where the last child was covered, and little effect on marriage rates for families with only some children covered. Third, the findings are robust to several specification checks. When the sample is restricted to women without employer provided health insurance the estimated effect of the expansions increases. In contrast, when the sample is restricted to women possessing employer provided health insurance, the expansions have no effect on marriage. The findings on Medicaid's impact suggest that previous criticisms can not be taken lightly, and that current debates on health insurance reform could have dramatic effects on marriage decisions if they are not neutral to family structure.

The remainder of the paper is arranged as follows: Section 2 briefly describes the incentives that the welfare system offers for living arrangements and discusses its real life relevance. Section 3 presents an illustration of recent Medicaid expansions for children. Section 4 presents a static model where the decision to marry is endogenous, and offers several predictions from the Medicaid expansions. Section 5 describes construction of the data set, the Current Population Survey. Section 6 presents results the main and several specification checks. Section 7 concludes with several possible extensions and reconciles the large estimates of Medicaid's effect with the much smaller estimates of AFDC's effect.

2. Background on the Welfare System

The U.S. welfare system offers three large benefits to the potential recipient: cash assistance through AFDC, health insurance through Medicaid and food subsidies through Food Stamps. Before recent Medicaid eligibility expansions, a recipient would qualify simultaneously for AFDC and Medicaid.[3] Two distinguishing features of these programs are requirements related to income and family structure. Both AFDC (and hence, Medicaid) and Food Stamps are means-tested. The AFDC income eligibility levels are set by the individual state and range from 27% of the Federal poverty level (FPL) in Alabama to 113% in Arizona for a family of three in 1992.[4] The variation in benefits has been used in previous work on family structure (Ellwood and Bane, 1985; Hutchens, et al., 1989; Hoffman and Duncan, 1988; Duncan and Hoffman, 1990). The other main benefit, Food Stamps is nationally administered and has higher income limits than AFDC. It also treats AFDC benefits as income in its calculation and reduces Food Stamp benefits by 30 cents for each additional dollar of income. The second distinguishing characteristic of AFDC and Medicaid is that eligibility is related to family structure. To qualify, one must

[3] Other ways for two-parent families to qualify for Medicaid include AFDC-UP (unemployed parents) in which the principal wage earner must have a substantial attachment to the labor force and the Medically Needy program, whereby families with income above the AFDC income eligibility limit can qualify if they have substantial medical expenses. See Hoynes (1993a) for more discussion on the AFDC-UP program and Winkler (1994) for evidence on its effect on family structure.

[4] These limits are after 12 months of work while on AFDC. The limits for the first four months of work range from 39% in Alabama to 168% in Arizona. Several other states, including Alaska, California, Connecticut, Hawaii, Maine, Massachusetts, New York, North Carolina, Rhode Island, Vermont and Washington had income eligibility limits of more than 100% of the FPL during the first four months of work while on welfare (Green Book, 1993, pp. 669-671).

typically reside in a non-traditional family. In practice, this translates into a female headed household with children under 18 present.[5] Food Stamps, on the other hand, has no such family structure requirement. Hence, food stamps should not be considered in the marriage tax, because a poor married family can still obtain food stamp benefits.[6] In fact, food stamps mitigates the effect of losing AFDC benefits, because when AFDC benefits are lost (which are counted as income in determining food stamps), food stamp benefits increase.[7]

To illustrate the effect of the marriage penalty, table 1 shows the budget set for a mother with two children in Illinois in 1991. When this mother, whose income sources are documented in column (1) considers marrying the father who earns $15,000 and has no employer provided health insurance, whose income sources are documented in column (2), the couple loses AFDC and Medicaid benefits. In this case, Medicaid is "valued" at the average expenditure in state of Illinois, $2,820. By marrying, their total income drops by $6,220, or 29% of their total income. Thus the disincentive to marry is substantial, and losing Medicaid benefits accounts for a non-trivial part of the total penalty. If both children were covered by the Medicaid expansions, the penalty would be reduced by approximately $1,400, to 22%

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[5] Subfamilies (young mothers with children who live with their parents) also qualify for welfare and are incorporated in my analysis. See Ellwood and Bane (1985) and Hutchens, et al. (1989) for more information on subfamilies.

[6] Several studies on family structure use the sum of the AFDC and Food Stamp guarantees, such as Plotnick, 1989; Plotnick, 1990; and Lundberg and Plotnick, 1990. Moffitt (1990b) and Hoynes (1993b) use the sum of the AFDC and Food Stamp guarantees along with the average Medicaid expenditure in each state.

[7] Moffitt (1989) shows that the food stamp's tie to food consumption is innocuous. That is, the recipients value the coupons as nearly identical to cash, since they would have consumed more food than the food stamp benefit in any case.

of income. While Medicaid reduces the marriage penalty, we see that AFDC should clearly play a large role in family structure outcomes.

There are at least three reasons to believe that extending Medicaid to married couples may result in stronger effects than AFDC benefits. First, the effects of welfare benefits on the marriage decision and divorce decision may asymmetric. The expansions in the Medicaid program mainly offered new incentives to marry whereas increases in the AFDC benefit level offer new incentives to get divorced. Second, AFDC income may be more easily offset than health insurance by the potential husband. Third, the presense of welfare stigma could imply larger effects of incentives to leave welfare than incentives to enter welfare, which many women who respond to the expansions were previously on. Also, since the expansions extend health insurance to married couples, women who were previously single, working, and not on the welfare rolls may also respond to the expansions. Hence, the expansions are not only an avenue off welfare, but more generally an avenue off single motherhood.

3. Description of Recent Medicaid Expansions

To independently identify the effect of Medicaid on the decision to marry, I utilize a series of health insurance expansions for children implemented between 1987 and 1991. These expansions came in response to cutbacks in welfare eligibility in 1981, which were viewed as too severe, along with growing concern over infant mortality and children's health.[8] Prior to these expansions, Medicaid eligibility and AFDC eligibility were nearly collinear. The expansions severed the link to AFDC eligibility along the income margin (by targeting Medicaid eligibility to the Federal Poverty Level instead of a state's AFDC income cut-off) and family structure margin, by eliminating the need to live in a non-traditional household in order to obtain health insurance for their children.

The Federal government first gave states the option, then mandated, Medicaid expansions for children where eligibility was not linked to family structure.[9] The Omnibus Reconciliation Act of 1986 (OBRA) gave states the option to implement the expansions to children under 2 years of age up to 100% of the Federal poverty level (FPL). Within one year of its implementation, half of the States had expanded eligibility to 100% of the FPL. Withing two years, 44 States and the District of Columbia had expanded. OBRA 1987 (effective July 1, 1988) gave states further options, by letting them implement expansions for children up to age 8 to 100% of the FPL, who were born after September 30, 1983. It also increased the income eligibility limit even more for infants. Eighteen states used these options to raise the threshold for infants above the poverty level, most to the upper limit of 185%. Then OBRA 1989 mandated that states cover children under age 6 to 133% of the FPL, by April 1, 1990. The impact of this law was felt more widely by the states. Thirty-two States did not have thresholds at 133 percent of the FPL for infants and

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[8] Currie and Gruber (1993) examine the impact of related pregnancy expansions on prenatal care and infant health outcomes.

[9] This discussion follows Hill (1990).

were required to adjust incrementally, most from 100 percent to 133 percent. A much larger effect surrounded the mandated coverage of children, however. Only 14 States were already covering children to 6 or 7 years of age in April 1989. Twenty-five states were phasing-in coverage of children from 2 to 5 years of age, and 12 States covered only infants to 1 year of age under the newly eligible groups. All States were brought into compliance with the minimum floor of age 6 by the implementation date. Finally OBRA 1990 (effective July 1, 1991) mandated that states cover all children under age 19 to 100% of the FPL who were born after September 30, 1983. Therefore, all poor children under age 19 would be eligible for Medicaid by the year 2002.

These expansions thereby condition eligibility on three potentially exogenous dimensions. Eligibility is a function of the child's birthday (since some children are ineligible based on being born too early and because different states set different age cutoffs). It is also a function of the child's state of residence (since states initially had the option to implement the expansion), and the time period (since the expansions became more generous at the end of the period).[10] These expansions created differences in generosity and scope of coverage, even for families within the

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[10] The expansions also condition on income, meaning the health insurance is still means tested but at a higher level than before. Income is endogenous in the model presented in section 4. See Yelowitz (1993) for the effects of relaxing income eligibility on labor supply. One potential problem associated with this method of identification is that the legislation could be endogenous. States which voluntarily adopted the legislation may have anticipated thatit would have a large impact on marriage. While the criticism may have merit, one must additionally argue that the states deliberately aimed the expansions at only mothers with young children. Otherwise the within state identification should still be valid. In the estimation, the inclusion of state fixed effects and interactions of state and time dummies should control for contemporaneous shocks that affect the decision to marry within a state.

same state at a point in time. Consider the following example, which illustrates incentives offered by the OBRA 1987 legislation:

OBRA 1987 options	
After July 1, 1988	After October 1, 1988
The state *may* extend Medicaid coverage to any age up until 5 if:	The state *may* extend Medicaid coverage to any age up until 8 if:
• Child's age<5 *(state can choose 1, 2, 3, 4 or 5).*	• Child's age< Age limit *(state can choose 6, 7 or 8).*
• Family income less than 100% of FPL *(state can choose any level < 100%).*	• Family income less than 100% of FPL *(state can choose any level < 100%).*
• Child born *after* September 30, 1983.	• Child born *after* September 30, 1983.

In this case mothers with differently aged children receive the following "treatment" on July 1, 1988 (which, in turn, affects her budget constraint as shown in Section 4). A child born on September 30, 1983 would not be covered by the expansions, whereas a child born a day later, on October 1, 1983 would be covered for over three more years. Any child born after July 1, 1988 would be covered for eight additional years. In addition to variation in the child's eligibility, the laws create variation in the income limit where the recipient loses Medicaid coverage after the expansions. That means after the expansions, the new "Medicaid notch" depends on the child's age. For instance, after July 1, 1991, a family faced the following schedule for losing Medicaid coverage, due to the binding Federal mandates, OBRA 1989 and 1990:

Child's age	Age 0	Ages 1 to 5	Ages 6 to 18	Ages 19 and over
Percentage of FPL	185%	133%	100%	0%

After the expansions, a woman could marry a higher earning male and still retain health insurance. A family with a 5 year old can earn up the 133% of the FPL before losing Medicaid, while a family with a 6 year old can only earn up to 100% of the FPL.

These expansions offer three dimensions to identify Medicaid's effect on the marriage decision. These dimensions are:

- Within state: For example, compare marriage rates of mothers with children born before October 1, 1983 to mothers with children born after that date, within a state that implemented a Medicaid expansion.

- Between state: Compare marriage rates of mothers in a state that implemented the Medicaid expansion to mothers in a different state that did not.

- Over time: Compare marriage rates of mothers in a state before and after a Medicaid expansion

Each of these dimensions alone amounts to a "first difference." For example, one potential estimate of Medicaid's effect on marriage would be to compare marriage rates of mothers with a 5 year old in California in 1990 to marriage rates of mothers with a 6 year old in California in 1990. Since the five year old is covered by OBRA 1989 but the six year old is not, then the difference in marriage rates could be attributed to Medicaid. If there are reasons other than the expansions that cause differences in marriage rates between these two groups, however, then we could utilize a second dimension of variation to identify Medicaid's effect. For instance, if a father is more likely to remain attached to the unit when younger children are present, then we can look at the previous groups both before and after the expansion was implemented, in 1989 and 1990. By combining any two of the three dimensions, such as the TIME dimension and the CHILD'S AGE dimension, we obtain a difference-in-differences estimator. If neither of the previous groups were eligible in 1989, then utilizing this second dimension could control for the father's attachment based on the child's age. Finally, by utilizing the variation in all three dimensions, we obtain a difference-in-differences-in-differences estimator.[11]

4. Theoretical Effects of Medicaid on Marriage

Most models of the marriage decision are variants of Becker's (1973, 1974, 1981) influential work, and this section presents a simple static model where the decision to marry is endogenous. Following Moffitt (1990b), the woman simply compares the maximum utility of two different states, married or single. The utility function contains three arguments, a marriage indicator, leisure and other goods. Hence the woman will marry if:

$$U(1,\text{Leisure}_1\ ,\text{Other Goods}_1) > U(0,\text{Leisure}_0\ ,\text{Other Goods}_0)$$

where the first argument is marriage indicator. Clearly the first derivatives $U_2(\bullet)$

[11] See Yelowitz (1993) for a additional discussion of identification.

and $U_3(\bullet)$ are positive, meaning the recipient enjoys more leisure or other goods to less. On the other hand, no restriction is made on $U_1(\bullet)$. That is, it would be hard to justify a functional form restriction on how marriage affects utility, unlike "welfare stigma" where there is more agreement on how program participation affects utility. It is wrong to argue that everyone prefers marriage to being single, so it would be dubious to sign $U_1(\bullet)$ as positive. Fortunately, the expansions allow predictions in spite of this. The expansions relax both the income and family structure margins, which generally create new opportunties on both the single and married woman's budget set, and thus has an ambiguous effect on marriage. In some instances, however, the expansions for single women were not binding. Hence, the only new opportunties on the married woman's budget set and therefore lead to an increase in marriages.

Figure 1(a) illustrates the opportunities facing a single woman before the Medicaid expansions. The welfare system causes the budget set for a single woman to be non-linear. When the woman does not work, her family collects AFDC, Food Stamps and Medicaid benefits. As she begins to work, her AFDC and Food Stamp benefits are taxed away at a high rate, but she retains health insurance until she reaches the point H^*, the hours point where AFDC eligibility ends. By working more than this number of hours, she loses her AFDC and Medicaid eligibility, which creates a dominated part of the budget set. After this point, her after-tax wage is higher (and determined solely through the Federal tax code), but she loses health insurance benefits. Figure 2(a) illustrates the opportunities facing a married woman before the expansions. In this case, her non-labor income is derived mainly from the husband's earnings. It is also assumed that the husband does not have health insurance through his employer.[12]

Figure 1(b) and 2(b) illustrate the effect of the Medicaid expansions on the budget sets of single and married women, respectively. New opportunities exist for single women on areas ABCD, and for married women on areas EFGH. Thus, without restrictions on the utility function, the expansions have an a priori ambiguous effect on the decision to marry. It is possible that an initially married woman located somewhere on the budget constraint in Figure 2(a) would prefer to get divorced and move to a point on the line segment AB in Figure 1(b). This could be construed as an "independence effect" caused by relaxing the income margin (Groeneveld, Tuma and Hannan, 1980). Since the woman often retains custody of the children (who are assigned Medicaid eligibility), then she is not as dependent on her spouse and therefore might get divorced. Even if the independence effect is small in magnitude, it certainly affects a larger portion population, so the aggregate effects of the expansions on marriage rates are unclear.

With new opportunities on both budget sets, the effect of the expansions on marriage is theoretically ambiguous. Consider a health insurance expansion that was not binding for the single woman, however. That is, consider an expansion in a state with high income eligibility limits for single women. If this is the case, then the

[12] The expansion's effect on marriage should be smaller by including all mothers (including those with employer provided health insurance), since the law changes should have no impact for those with insurance. The results in section 6 bear this out.

opportunities for the single woman do not change after the expansion (so area ABCD on the single woman's budget set does not exist). There are still new opportunities for the married woman, since the expansion severs the link to family structure. In this case, the incentive to marry unambiguously increases, solely through revealed preference arguments. Since the married woman could have picked any point on the single woman's budget set before the expansions, then she will not get divorced afterwards (which she already could have done). Therefore when we control for the "independence effect" we obtain the unambiguous prediction that marriages should increase in response to eligibility expansions. A state may implement such an expansion if its goal is simply to increase health insurance coverage among the poorest children.

Several aspects about the model deserve mention. First, the model yields symmetric responses for marriage and divorce. With different fixed costs of moving between these states (such as divorce stigma or lawyer fees), it would be easy to obtain asymmetric responses. Second, the presentation assumes that the labor market decisions of the (potential) husband remain unchanged in response to the expansions. If the husband did change his labor supply, the effect would most likely be smaller. Third, the model is static but a dynamic marital search problem certainly would capture more richness than is presented here. This is beyond the scope of the paper and has not been explored in the literature.

5. The Data Set

I use the 1989, 1990, 1991 and 1992 March Current Population Surveys (CPS) in the analysis. The CPS is a large, timely, nationally representative data set. I include all women between the ages of 18 and 55 with at least one child under 15 present. I therefore include all women in any marital state (married, never married, divorced, separated or widowed) and include women living in subfamilies.[13] To each mother's record I linked all her children's ages. This results in 69,948 observations, where the unit of observation is the mother. From the time period, state of residence and child's age, I imputed eligibility for the Medicaid expansions based on information from publications of the Intergovernmental Health Policy Project.[14]

Table 2 presents summary statistics of the variables used in the analysis. The dependent variable is marital status asked as of March 1 of the survey year.[15] Approximately three quarters of the sample is married, but the marriage rates vary along several dimensions. First, white mothers are more than twice as likely to be married than black mothers, with a rate of 80% compared to 37%. Second, marriage

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[13] I further restrict the sample to households with less than eleven persons, since some of the data on a state's AFDC characteristics only includes information for families up to ten persons. This is a trivial exclusion, and I retain 99.94% of the sample. One note of caution is the potential endogeneity of children. Moffitt (1990b) notes that since the welfare system conditions eligibility on having children present, it may be more appropriate to include all women, even those without children. Ellwood and Bane (1985) find little effect of the welfare system on childbearing, however.

[14] I do not use information on a family's income to impute eligibility. Since the CPS contains only the age as of March 1 of the survey year, I randomly assign a month of birth in the year which the child could have been born. For further discussion of this procedure, see Yelowitz (1993).

[15] Danziger, et al. (1982) presents a discussion of stocks versus flows in analyzing the marriage decision.

rates declined during the sample period, from 76.5% in 1989 to 73.9% in 1992. On the surface, this time series trend bodes poorly for the hypothesis that the Medicaid expansions encouraged marriage (which became more generous over time), but one should recognize that other time specific factors could enter the analysis. For instance, economic conditions, which were changing over time, could enter into the decision to marry. In all specifications in the econometric analysis, I will control for such time-specific shocks which could affect the decision to marry independent of the Medicaid expansions. Third, there are differences in marital status by educational attainment and age group. Marriage rates are increasing in age until age 45, and then decrease. The rest of the table contains independent variables that will be used in different specifications. To parameterize the expansions, I try three measures of the law change:

- ALLELIG, which is an indicator variable equal to one if all the children under 15 in the family were covered by the expansion and zero otherwise.

- ANYELIG, which is an indicator equal to one if any of the children under 15 in the family were eligible.

- PCTELIG, which is the percentage of the children in the family that were covered by the expansions.

Therefore, if two of three children were covered by the expansion based on their ages, ALLELIG would equal zero, ANYELIG would equal one, and PCTELIG would equal 2/3. ALLELIG corresponds to covering the oldest child in the family, whereas ANYELIG corresponds to covering the youngest child. In the entire sample, the mean of ALLELIG is 0.250, the mean of ANYELIG is 0.425 and the mean of PCTELIG is 0.422. Other covariates included in the analysis are the race, age, and educational attainment of the mother, an indicator for residence in a city, and the number of children under age 6 and number of children between ages 6 and 17. Approximately 11.6% of the sample is black, 4.4% is other non-white, and the remainder of the sample is white. The average age 33.6 and the average educational attainment is 12.74 years. Nearly one quarter of the sample lives in a city. The average number of children under 6 is 0.741 and between 6 and 18 is 1.238.

6. Results From the CPS

To analyze Medicaid's effect, I estimate a probit model to predict the effect of eligibility on marriage.[16] The equation used in estimation is:

$$(1) \quad MARRIED^{*}_{ijt} = \beta_0 + \beta_1 \cdot ELIG + \beta_2 \cdot X_i + \beta_3 \cdot Z_{jtn} + \epsilon_{ijt}$$

where i indexes mothers, j indexes state of residence, t indexes time, and n indexes youngest child's age. The variable MARRIED* represents the underlying index function (where MARRIED is the observed, discrete outcome). MARRIED is a level (in contrast to a change in marital state) which equals one if the woman is married and zero otherwise. The key independent variable is ELIG, which will correspond to one of the three measures outlined in the previous section. X_i is a vector of exogenous individual characteristics of the mother, including dummy variables for black and other non-white, dummy variables for the mother's age and education, a

[16] The results from a linear probability model are extremely similar and available from the author upon request.

central city indicator, the number of children between zero and five, and the number of children between six and eighteen.[17] The vector Z_{jtn} contains various interactions of state dummies, time dummies and youngest child's age dummies. In all specifications, this vector includes time dummies and youngest child's age dummies.

The subsequent analysis focuses on reduced form results. A structural approach would need to address several issues that are avoided in a reduced form. First, one would need to specify a utility function, which includes imposing some restriction on how the marriage indicator affects utility. Second, the difficulties of unobserved wages in a female labor supply problem are compounded here. In this case, not only do we need to impute the wage-health insurance coverage for non-working women, but we need to impute the wage-health insurance coverage for the potential husbands of single women.

Table 3 presents results using the first measure, ALLELIG, whether all the children in the family were eligible. All specifications presented include indicator variables for time, youngest child's age and mother's age. Recall that the predicted effect of the eligibility expansions is ambiguous, since the independence effect could be important. Even without controlling for the independence effect, the coefficient $\beta_{ALLELIG}$ is positive, which means the expansions encourage marriage. The third column, which additionally controls for STATE and STATE*TIME interactions, is the preferred specification. By including these interactions, I control for the potential impact of AFDC and AFDC-UP on marriage separately from Medicaid's

[17] The dummy variables for different levels of educational attainment are: less than high school, some high school, completed high school, and college.

effect. In this case, extending Medicaid coverage to the last child in the family increases the probability of marriage by 2.61%.[18] In addition, the coefficient estimate is significant, so we reject that the expansions have no effect. In addition, we see that being black has a huge, negative impact on the probability of marriage. In contrast, the other non-white indicator has only a small negative effect. Lower levels of mother's education decrease the probability of marriage. Residing in a central city has a substantial negative impact on marriage, and the number of children (of any age group) has a substantial positive impact on the probability of marriage. As columns (1), (2) and (3) show, these coefficient estimates are very similar, even with the inclusion of state effects, and state-year interactions.

Table 4 shows the effects of using ANYELIG instead of ALLELIG, where all specification once again include time, youngest child's age and mother's age dummies. We might expect that the result should be weaker by not (necessarily) covering every child in the family, but this measure yields results indistinguishable from zero. The estimated effect of covering any children would increase the probability of marriage by 0.07%, a very small effect. The other covariate's coefficients are similar in magnitude and significance to table 3.

Table 5 presents the results using PCTELIG, the percentage of children in a household eligible for the expansions. Time, mother's age and child's age dummies are included in each specification. In this case, the effects are positive and

[18] The marginal effects are calculated by evaluating each individual's predicted probability for ALLELIG (and similarly for ANYELIG and PCTELIG) evaluated at one and zero and taking the average difference across all observations.

significant. By increasing this variable from zero to one can be interpreted as a policy change from covering none of the children in the household to covering all the children in the household, in which case is associated with an increase in the probability of marriage of 1.94%. The estimated coefficients on the covariates are quite robust across different specifications.

Table 6 controls for the "independence effect." This is motivated from previous studies on the Negative Income Tax, which found that whom the benefits were awarded to (either the entire family unit, including the husband, or to the wife only) affected the likelihood of divorce.[19] Recall that the expansions severed the link to AFDC eligibility along both the income margin and family structure margin. Since severing the link along the income margin could lead to new opportunities on the single woman's budget set, the previous sets of estimates could *understate* Medicaid's true impact on the decision to marry (since not all the incentives offered by the expansions work in the direction of marriage). To control for this independence effect, I restrict the sample to those for whom the opportunities on the single woman's budget set did not change; that is, those for whom the loosening of the income requirements had no effect on the single woman's opportunities. To do this, I define GAIN% as the change of the income eligibility limit for Medicaid, normalized by the Federal poverty level. When the expansions have no "bite" for

[19] Groeneveld, Tuma and Hannan (1980) find that at the highest guarantee level, the income and independence effects were approximately equal, but that the independence effect dominates at the lower guarantee levels.

single women, GAIN% equals zero.[20] However, since the expansions also severed the link to family structure, they will still have impact on the decision to marry, and in this case the estimated coefficients are unambiguously positive. Restricting the sample to mothers in states that implemented the law change and have GAIN% equal to zero leads to 44,760 observations, rather than the 69,948 previously. Observations are assigned zero if either a child is ineligible (for instance, if a child born before September 30, 1983 resides in the household) or if the family lives in a generous welfare state, so that the law change is not binding for single women. Since the expansions relax both the income and family structure margins, a state might implement a non-binding expanison (for single women) if its objective was to cover children in married families. Table 6 shows that the independence effect is quite important in the coefficient estimates. The estimated effect of Medicaid nearly doubles from before. For instance, the coefficient on ALLELIG increases from 2.61% to 4.36% in column (3) after controlling for this effect. The effect on ANYELIG increases from 0.07% to 0.87%, and the effect on PCTELIG increases from 1.94% to 4.09%. Thus, the coefficients (as might be expected), are larger and still significant for ALLELIG and PCTELIG. The independence effect is quite

[20] GAIN% is meant to capture the change in opportunities facing the single woman after the expansion. GAIN% is defined as the maximum of new income eligibility limit minus the old income eligibility limit and zero (that is, if the expansion is not binding then we do not penalize the woman by assigning a negative value, since there are no changes in her opportunities, rather than a reduction in them). In calculating the old income eligibility limit, I account for both the gross income test (based on a state's need standard) and the countable income test (based on the state's payment standard). The source of information for need and payment standards come from the National Governor's Association, and are based on July of each year. Since the limits vary by family size, I use the (number of children + 1) as the family size. See Yelowitz (1993) for a detailed discussion of this measure or the Green Book (1993, pp. 621).

important, in aggregate, even if its impact is small, since it affects a much larger portion of the population.

Table 7 stratifies the sample by race, since the marriage markets for black and white women may be quite different in nature (other non-white is not presented). This yields 58,674 observations for white mothers and 8,161 observations for black mothers. The results of this table shed some light on why the parameterization of ANYELIG yields puzzling results. For the upper part of the table, in examining white women, the results are roughly consistent with the entire sample: both ALLELIG and PCTELIG are positive and significant, while ANYELIG has a puzzling negative (although indistinguishable from zero) effect on the probability of marriage. In contrast, the lower part of the table, in examining black women, yields plausible signs and magnitudes, though only of marginal significance, possibly due to the smaller sample size. Interestingly, the effect of Medicaid is largest using ANYELIG for black women -- extending health insurance to the first child increases the probability of marriage by 4.03% in column (3). Apparently black mothers respond to smaller incentives (covering the first child in the family) than white women, who need a larger incentive to get married (covering the last child in the family).

To examine the stark difference between the coefficient estimates of ALLELIG and ANYELIG, Table 8 stratifies the sample. All specifications control for both state and time effects. The first column contains observations with either all children eligible or no children eligible, which differs from table 3 which compares all children eligible to some or no children eligible. Inclusion into this sample is highly dependent on family size: a family with one child, by defininition, is included while families of larger sizes are likely excluded. This restriction leads to a reduction of 12,288 observations. In this case, the marginal effect is 1.24% and significant, but smaller than the effect in table 3. When the sample is restricted to 32,604 observations with GAIN% equal to zero, the magnitude shrinks further, to 0.93% and insignificant. Columns (2) through (5) might provide a more illuminating examination. In these specifications, separate probit models were run for different numbers of children. There were a sufficient number of children to perform this exercise for families with up to four children. For a given number of children, separate dummy variables were included for each number of children eligible, where the reference group is no children eligible. This specification allows us to identify where the expansions have the most impact. For example, we can estimate the change in probability of marriage from expanding Medicaid coverage from the first to the second child in a three child family, whereas ALLELIG and ANYELIG would remain unchanged. While the results are not conclusive, they are at least suggestive of the hypothesis that covering the last child in a family matters to marriage. While column (2) has insignificant coefficients, columns (3) and (4), the two and three child families illustrate the point nicely: covering all children has an important impact on marriage, while covering only some children has no effect. While column (5) shows a positive impact for any amount of coverage, the coefficients are estimated imprecisely and it is difficult to make inferences.

Finally, a specification check was performed. The check involves separating the sample into those with or without employer provided health insurance, since the expansions should have different effects on these groups. While those who have employer provided health insurance may be systematically different than those who do not, we should still see a larger effect of the expansions by excluding them. This restriction yields 46,347 mothers with employer provided health insurance and 23,601 mothers without. In this case, the coefficient on ALLELIG increases from 2.61% to 4.68% for those without employer provided health insurance. On the other hand, covering all children in a family has an insignificant effect on families with employer provided health insurance, with the marginal effect being 0.66%. Similarly, PCTELIG increases from the initial specification, from 1.94% to 2.76% for those without employer provided health insurance, but is only 0.76% for those with. Once again, ANYELIG remains a puzzle. For both specifications, those with and without employer provided health insurance, the effect is insignificant, but is actually negative for those without.

7. Concluding Remarks

This paper has shown a strong impact of extending Medicaid on the marriage decision, which stands in contrast to previous work on AFDC cash benefits. Extending Medicaid health insurance to all children in a family is associated with an increase in the probability of marriage of 4.45%. This finding is robust to the inclusion of state fixed effects which have been important in previous work. One interesting finding is that Medicaid's effect on marriage is concentrated on covering the *last* child in a household. Apparently large changes in benefits are needed to increase marriages.

Previous work finds a small, but statistically significant effect of these benefits on the marriage decision. There are several reasons why these findings can be reconciled. First, the effect of welfare benefits on the decision to marry and the decision to divorce may be asymmetric. If negative connotations are associated with the later, through some kind of "divorce stigma," then welfare benefits may not have much impact. Second, AFDC cash income may be more easily offset than health insurance. In fact, Moffitt (1983) models AFDC cash income as being valued less than other income (because of welfare stigma). If this is the case, then increasing the cash benefits may not have much impact on divorce.

There are several extensions which I hope to examine. First, it would be fruitful to extend the static model into a dynamic marital search model to examine whether the expansions had similar effects on transitions into and out of marriage. This may also shed some more light on the importance of the independence effect, the transition from marriage to divorce. Second, I will examine whether the characteristics of the mates chosen changed in response to the expansions. The relative attractiveness of potential spouse with health insurance is lowered compared to a potential spouse without health insurance. Third, as Moffitt (1990b) points out, changes in welfare benefits could also change search behavior of men. Finally, the impact of the Medicaid expansions on fertility behavior is unknown.

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Budget set for single woman

Fig. 1(a): Before Expansion

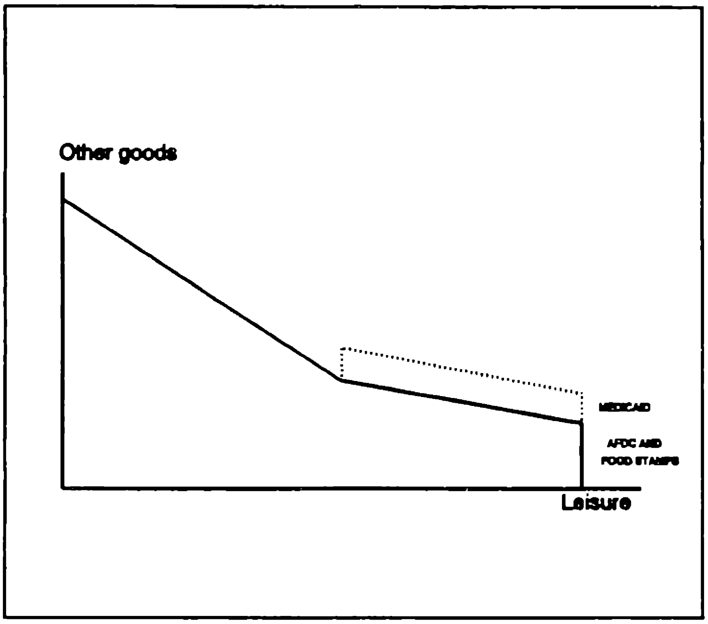
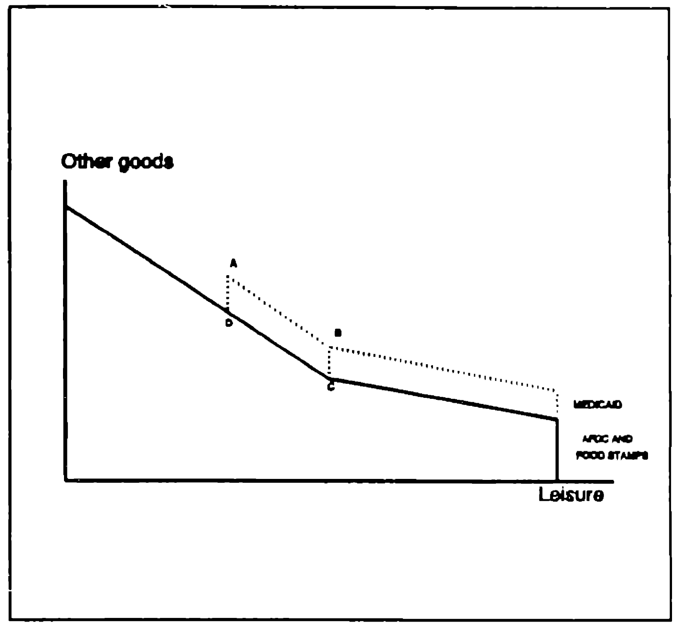


Fig. 1(b): After Expansion



Budget set for married woman

Fig. 2(a): Before Expansion

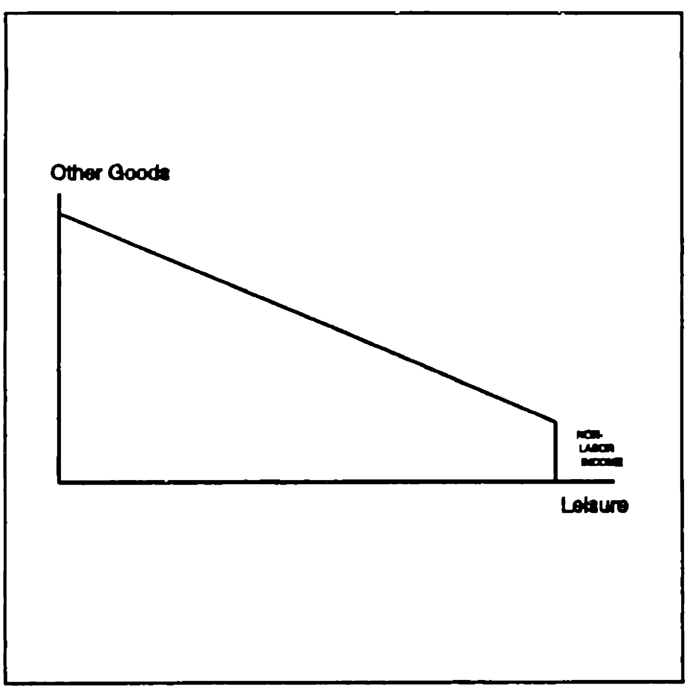


Fig. 2(b): After Medicaid

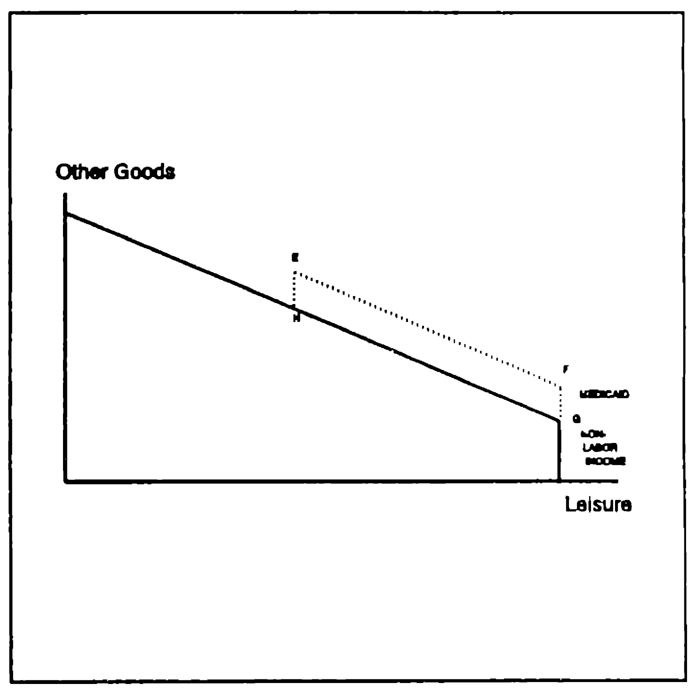


TABLE 1

Marriage penalties for a mother with two children an no earnings living in the State of Illinois, 1991

	Mother of two, $0 earnings	Single male	Marriage, family of four
Earnings	0	$15,000	$15,000
EITC	0	0	770
AFDC	$4,404	0	0
Food Stamps	2,820	0	1,368
Medicaid	**2,342**	**0**	**0**
Federal income tax	0	(1,418)	(210)
Disposable income	9,566	12,134	15,480
Marriage penalty, loss of income			6,220
Percent change			-29

Source: Green Book (1993), pages 1257-1265. Assumes child care expenses of zero since mother does not work, work expenses of $300 per year for the male ($25 per month for public transportation) and Social Security taxes of $1,148 for earning $15,000. Note that Food Stamps is available to married couples, and partially offsets the loss in AFDC cash benefits for two reasons: Food Stamps taxes AFDC income at 30% in its calculation (so a reduction of $1.00 in AFDC income implies an *increase* of $0.30 in Food Stamp income) , and the Food Stamp benefits are increasing in family size. Medicaid benefit is "cashed-out" at the average expenditure in the state.

TABLE 2

Unweighted Summary Statistics		
mother married (%)	.751	{0,1}, 1=yes
black	.376	8,161 observations
white	.802	58,674
1989	.765	16,522
1990	.754	17,909
1991	.748	17,969
1992	.739	17,548
education $\leq$ 8	.702	3,695
9 $\leq$ education < 12	.559	7,218
education = 12	.743	29,047
education > 12	.811	29,988
18 $\leq$ age < 25	.552	7,287
25 $\leq$ age < 30	.718	13,349
30 $\leq$ age < 35	.773	18,255
35 $\leq$ age < 40	.798	16,474
40 $\leq$ age < 45	.811	9,897
45 $\leq$ age < 50	.786	3,579
50 $\leq$ age $\leq$ 55	.751	1,107
all children eligible	.250	{0,1}, 1=yes
at least one child eligible	.425	{0,1}, 1=yes
percentage of children in family eligible	.334	[0,1]
black	.116	{0,1}, 1=yes
other non-white	.044	{0,1}, 1=yes
mother's age	33.6	[18,55]
years of education	12.74	[0,18]
lives in city	.226	{0,1}, 1=yes
number of own children ages 0 to 5	.741	[0,6]
number of own children ages 6 to 17	1.238	[0,8]

Source: Author's tabulations from March Current Population Survey, 1989, 1990, 1991 and 1992. Unit of observation is mother. Number of observations is 69,948.

TABLE 3

Probit model predicting the increase in probability of marriage			
	(1)	(2)	(3)
Independent variable	Dependent variable = MARRIED		
All children eligible: $\beta_{ALLELIG}$	**.0945** **(.0181)** ***.0249***	**.0991** **(.0184)** ***.0259***	**.1001** **(.0188)** ***.0261***
Black	-1.0474 (.0162)	-1.0814 (.0174)	-1.0834 (.0174)
Other non-white	-.0553 (.0268)	-.0496 (.0283)	-.0511 (.0284)
Education$<$9	-.3352 (.0249)	-.3409 (.0255)	-.3434 (.0255)
9$\leq$Education$<$12	-.4836 (.0187)	-.4938 (.0189)	-.4966 (.0190)
Education$=$12	-.1021 (.0125)	-.1039 (.0126)	-.1063 (.0126)
Central city	-.3205 (.0130)	-.3047 (.0141)	-.3055 (.0141)
Number of children between 0 and 5	.1075 (.0114)	.1102 (.0145)	.1106 (.0145)
Number of children between 6 and 17	.1132 (.0074)	.1141 (.0074)	.1148 (.0075)
State effects	No	Yes	Yes
State*Time effects	No	No	Yes
log likelihood	-33,569	-33,374	-33,294

Notes: Coefficients each from separate regression. Estimates from Current Population Survey, 1989 through 1992. Standard errors in parenthesis. Marginal effects in italics. Sample size is 69,948. All specifications include time effects, youngest child's age dummies and mother's age dummies.

TABLE 4

Probit model predicting the increase in probability of marriage			
	(1)	(2)	(3)
Independent variable	Dependent variable = MARRIED		
Any children eligible:	**.0129**	**.0093**	**.0029**
$\beta_{ANYELIG}$	**(.0183)**	**(.0190)**	**(.0203)**
	.0034	***.0024***	***.0007***
Black	-1.0483	-1.0824	-1.0844
	(.0162)	(.0174)	(.0174)
Other non-white	-.0554	-.0487	-.0501
	(.0268)	(.0283)	(.0284)
Education<9	-.3353	-.3413	-.3438
	(.0249)	(.0254)	(.0255)
9≤Education<12	-.4860	-.4965	-.4992
	(.0187)	(.0189)	(.0190)
Education=12	-.1037	-.1056	-.1080
	(.0125)	(.0126)	(.0126)
Central city	-.3205	-.3044	-.3051
	(.0130)	(.0141)	(.0141)
Number of children between 0 and 5	.1019	.1043	.1046
	(.0144)	(.0144)	(.0145)
Number of children between 6 and 17	.0990	.0993	.0999
	(.0069)	(.0070)	(.0070)
State effects	No	Yes	Yes
State*Time effects	No	No	Yes
log likelihood	-33,582	-33,388	-33,308

Notes: Coefficients each from separate regression. Estimates from Current Population Survey, 1989 through 1992. Standard errors in parenthesis. Marginal effects in italics. Sample size is 69,948. All specifications include time effects, youngest child's age dummies and mother's age dummies.

TABLE 5

Probit model predicting the increase in probability of marriage			
	(1)	(2)	(3)
Independent variable	Dependent variable = MARRIED		
Percent children eligible: $\beta_{PCTELIG}$	**.0701** **(.0212)** ***.0186***	**.0730** **(.0219)** ***.0193***	**.0734** **(.0230)** ***.0194***
Black	-1.0483 (.0162)	-1.0822 (.0174)	-1.0842 (.0174)
Other non-white	-.0551 (.0268)	-.0493 (.0283)	-.0508 (.0284)
Education < 9	-.3355 (.0249)	-.3411 (.0255)	-.3435 (.0255)
9 ≤ Education < 12	-.4853 (.0187)	-.4956 (.0189)	-.4983 (.0190)
Education = 12	-.1031 (.0125)	-.1050 (.0126)	-.1074 (.0126)
Central city	-.3201 (.0130)	-.3044 (.0141)	-.3052 (.0141)
Number of children between 0 and 5	.1028 (.0144)	.1052 (.0144)	.1055 (.0145)
Number of children between 6 and 17	.1051 (.0072)	.1055 (.0072)	.1062 (.0073)
State effects	No	Yes	Yes
State*Time effects	No	No	Yes
log likelihood	-33,577	-33,383	-33,303

Notes: Coefficients each from separate regression. Estimates from Current Population Survey, 1989 through 1992. Standard errors in parenthesis. Marginal effects in italics. Sample size is 69,948. All specifications include time effects, youngest child's age dummies and mother's age dummies.

TABLE 6

Comparisons when controlling for the independence effect or not.	(1)	(2)	(3)
Controlling for the independence effect by restricting sample to GAIN%=0 (N=44,760)			
All children eligible: $\beta_{ALLELIG}$	.1336 (.0259) *.0352*	.1705 (.0269) *.0441*	.1706 (.0274) *.0436*
At least one child eligible: $\beta_{ANYELIG}$	.0213 (.0258) *.0057*	.0315 (.0263) *.0084*	.0331 (.0269) *.0087*
Percent of children eligible: $\beta_{PCTELIG}$	.1171 (.0323) *.0313*	.1537 (.0336) *.0405*	.1517 (.0345) *.0409*
Not controlling for the independence effect (N=69,948)			
All children eligible: $\beta_{ALLELIG}$	.0945 (.0181) *.0249*	.0991 (.0184) *.0259*	.1001 (.0188) *.0261*
At least one child eligible: $\beta_{ANYELIG}$	.0129 (.0183) *.0034*	.0093 (.0190) *.0024*	.0029 (.0203) *.0007*
Percent of children eligible: $\beta_{PCTELIG}$	.0701 (.0212) *.0186*	.0730 (.0219) *.0193*	.0734 (.0230) *.0194*
State effects	No	Yes	Yes
State*Time effects	No	No	Yes

Notes: Estimates from Current Population Survey, 1989 through 1992. Standard errors in parenthesis. Marginal effects in italics. Covariates included in all specifications: Time, youngest child's age and mother's age dummies, education dummies, dummies for black and other non-white, central city, number of children 0 to 5, number of children 6 to 17, and a constant.

TABLE 7

Probit model predicting the increase in probability of marriage, stratified by race.	(1)	(2)	(3)
White only (N=58,674)			
All children eligible: $\beta_{ALLELIG}$	.0915 (.0203) *.0235*	.0952 (.0206) *.0242*	.0981 (.0210) *.0248*
At least one child eligible: $\beta_{ANYELIG}$	-.0060 (.0204) *-.0015*	-.0111 (.0212) *-.0028*	-.0175 (.0225) *-.0045*
Percent of children eligible: $\beta_{PCTELIG}$	.0588 (.0235) *.0152*	.0607 (.0243) *.0156*	.0627 (.0255) *.0160*
Black only (N=8,161)			
All children eligible: $\beta_{ALLELIG}$	.0591 (.0477) *.0213*	.0942 (.0488) *.0330*	.0886 (.0503) *.0041*
At least one child eligible: $\beta_{ANYELIG}$	.0848 (.0490) *.0250*	.1397 (.0517) *.0489*	.1333 (.0559) *.0403*
Percent of children eligible: $\beta_{PCTELIG}$	.0812 (.0575) *.0293*	.1400 (.0601) *.0490*	.1314 (.0639) *.0060*
State effects	No	Yes	Yes
State*Time effects	No	No	Yes

Notes: Estimates from Current Population Survey, 1989 through 1992. Standard errors in parenthesis. Marginal effects in italics. Covariates included in all specifications: Time, youngest child's age and mother's age dummies, education dummies, central city, number of children 0 to 5, number of children 6 to 17, and a constant.

TABLE 8

Probit model predicting marriage
Not controlling for the independence effect

	(1) No restrictions	(2) 1 child only	(3) 2 children only	(4) 3 children only	(5) 4 children only
All children eligible	.0463 (.0227) *.0124*	---	---	---	---
4 children eligible	---	---	---	---	.1641 (.1751) *.0355*
3 children eligible	---	---	---	.1139 (.0683) *.0244*	.0984 (.1405) *.0217*
2 children eligible	---	---	.1453 (.0409) *.0333*	-.0194 (.0670) *-.0043*	.2708 (.1447) *.0568*
1 child eligible	---	.0192 (.0292) *-.0057*	-.0655 (.0323) *-.0162*	.0040 (.0545) *.0009*	.1878 (.1191) *.0404*
N	57,660	31,018	25,845	9,726	2,435
log likelihood	-27,910	-16,474	-11,167	-3,904	-899

Notes: Controls for STATE and TIME interactions. Where there is more than one child, GAIN% corresponds to the gain for the oldest child. All children eligible is compared to no children eligible (in contrast to some children eligible).

TABLE 8, continued

Probit model predicting marriage *Controlling for the independence effect*					
	(1) No restrictions	(2) 1 child only	(3) 2 children only	(4) 3 children only	(5) 4 children only
All children eligible	.0365 (.0383) *.0101*	---	---	---	---
4 children eligible	---	---	---	---	.1789 (.2271) *.0379*
3 children eligible	---	---	---	.2141 (.0940) *.0442*	.0851 (.1515) *.0185*
2 children eligible	---	---	.2041 (.0584) *.0474*	-.0475 (.0839) *-.0108*	.1770 (.1845) *.0375*
1 child eligible	---	.0254 (.0508) *.0077*	-.0295 (.0406) *-.0075*	-.0207 (.0678) *-.0046*	.0832 (.1511) *.0181*
N	32,604	17,492	17,573	7,139	1,843
log likelihood	-16,190	-9,537	-7,841	-2,901	-673

Notes: Controls for STATE and TIME interactions. Where there is more than one child, GAIN% corresponds to the gain for the oldest child. All children eligible is compared to no children eligible (in contrast to some children eligible).

TABLE 9

Specification check: Observations with or without employer provided health insurance	(1)	(2)	(3)
Without employer provided health insurance (N=23,601)			
All children eligible: $\beta_{ALLELIO}$	.1335 (.0275) *.0458*	.1327 (.0282) *.0444*	.1405 (.0289) *.0468*
At least one child eligible: $\beta_{ANYELIO}$	.0104 (.0283) *.0036*	-.0226 (.0299) *-.0076*	-.0304 (.0326) *-.0102*
Percent of children eligible: $\beta_{PCTELIO}$	.0929 (.0329) *.0320*	.0733 (.0344) *.0247*	.0825 (.0367) *.0276*
With employer provided health insurance (N=46,347)			
All children eligible: $\beta_{ALLELIO}$	.0300 (.0270) *.0055*	.0355 (.0274) *.0065*	.0360 (.0279) *.0066*
At least one child eligible: $\beta_{ANYELIO}$	.0276 (.0268) *.0051*	.0341 (.0277) *.0063*	.0330 (.0289) *.0061*
Percent of children eligible: $\beta_{PCTELIO}$	.0336 (.0307) *.0062*	.0414 (.0315) *.0076*	.0413 (.0327) *.0076*
State effects	No	Yes	Yes
State*Time effects	No	No	Yes

Notes: Estimates from Current Population Survey, 1989 through 1992. Standard errors in parenthesis. Marginal effects in italics. Covariates included in all specifications include: Time and youngest child's age dummies, mother's age dummies, education dummies, dummies for black and other non-white, central city, number of children 0 to 5, number of children 6 to 17, and a constant.

Chapter Three

Is Health Care Coverage a Pro-Natal Policy?

1. Introduction

The past three decades have witnessed dramatic changes in family structure and fertility rates in the United States. For instance, the fraction of children born out of wedlock has risen dramatically. Fully two-thirds of black infants were born to single mothers in 1990 (Green Book 1993). While some critics (Murray 1984; Mead 1992) attribute these demographic trends to changes in the welfare system, this view is not universally shared (Ellwood 1986).

Since child bearing is an irreversible decision, it is important to know which policy instruments affect it. A good deal of political pressure against raising AFDC benefits, which are a function of family size, is motivated precisely because of these concerns. For instance, New Jersey recently received a waiver from the federal government to sever the link between AFDC cash benefits and family size for additional children born after August 1, 1993 (New York Times, 12/25/93). In addition to the number of children, some attention has been paid to the mother's age at first birth because the public costs of teenage child bearing are high. In 1990, the expected discounted costs to the public from a teenager having a baby was $23,000 for women under age seventeen, and $14,581 for women ages eighteen to nineteen (Green Book, 1993).[1]

While the impact on fertility of certain policy instruments such as cash benefits from Aid to Families with Dependent Children (AFDC) and to a lesser exent the tax

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[1] This includes all births, regardless of marital status. Since older teenagers are more likely to have a baby when married, this may explain some of the dramatic change in public costs.

benefits from the personal exemption have been explored, the role of Medicaid has not. This paper attempts to address the role of health insurance coverage in the childbearing decision.

To do so, I examine the income effects resulting from a recent broadening of the Medicaid program in the eligibility for children. Simple models by Becker (1992) suggest that increased fertility could be a response to such income effects. By utilizing variation in the implementation of the Medicaid expansions across states and over time, I show that the expansions offered variation in the income effects to potential mothers along two important margins. First, they offered the "contingent commodity" of health insurance coverage to a newborn. This coverage was not conditioned on the mother's marital status and covered children of mothers with a much higher income level than under AFDC. Second, the expansions offered health insurance coverage to some older children based on their birthday. This provides another income effect, by lowering the "goods cost" of child rearing.

To analyze the effect on fertility, I link the *Current Population Survey (CPS)* -- a large, nationally representative microdata set -- with health care expenditure information from the *National Medical Expenditure Survey (NMES)* to construct an exogenous valuation of the eligibility expansion for each family. I find that the expansions have a statistically significant effect on fertility, confirming some simple predictions of the theory. On the other hand, the economic importance is small. In the preferred specification, raising the value of Medicaid for a family by \$1,000 increases the probability of having a child by 0.33%. By separating this income effect into the contingent commodity for the newborn and the income effect for older children, I find, somewhat surprisingly, that the effect on fertility is concentrated entirely in covering older children. That is, the mothers in the sample apparently respond to income shocks by increasing child quantity, rather than increasing child quality for instance. This finding is in contrast to the conventional wisdom that the elasticity of child quantity with respect to income is low (whereas the elasticity of child quality with respect to income is thought to be high). Finally, these findings hold up against a variety of specification checks, such as stratifying the sample by marital status. In addition, the finding is robust to the definition of health insurance: the same broad conclusions emerge from using mean health care expenditure, median expenditure, or expenditure in the seventy-fifth percentile to construct the valuation.

The remainder of the paper is organized as follows. Section 2 gives details on these recent expansions, explaining their legislative history, theoretical effects on fertility and previous work exploring the expansions. Section 3 reviews previous work on policy instruments and fertility, focusing primarily on the effect of AFDC cash benefits and tax policy. Section 4 explains the construction of the data set and the results. Section 5 offers some conclusions and several avenues for additional research.

2. Legislative History and Effects of the Expansions

2.1 *Legislative History*

Starting in the mid 1980s (and continuing until the present), the Federal government implemented a series of Medicaid health insurance expansions for children which severed the link to AFDC eligibility.[2] The states were first given the option, and then mandated, to offer health insurance coverage for children. The initial legislation allowed states to implement Medicaid coverage for young children living in families with income up to 100 percent of the Federal Poverty Line (FPL), and later legislation mandated even more generous coverage. By April 1990, all states were mandated to cover children until the age of six in families with incomes under 133 percent of the FPL. By July 1991, all states were mandated to cover children born after September 30, 1983 until age 19 in families with income under 100 percent of the FPL. This means that all poor children would be eligible for Medicaid by October 2001.

These expansions have significantly expanded health insurance eligibility. Prior to the enactment of the Omnibus Reconciliation Act (OBRA) 1986, the average state Medicaid eligibility limit for a family of three was 56 percent of the federal poverty guideline. Within two years of the effective date of OBRA 1986, the average coverage level for infants had nearly doubled, to 96.7 percent of the FPL. By January 1989, the average coverage level was 127 percent of the FPL for infants, and reached 161 percent by January 1993. In addition, a number of states took advantage of provisions of the

[2] This section closely follows the information from publications obtained by the National Governor's Association. See Appendix 1 of Yelowitz (1993a) for a detailed account of the provisions of each piece of legislation.

Medicaid statue that permit them to broadly expand eligibility above the levels specified by the OBRA 1986, 1987, 1989 and 1990 legislation. In January 1994, Missouri began covering children ages six through eighteen in families under 100 percent of the FPL and New Hampshire began covering children under eleven years living below 170 percent of the federal poverty level. In addition to expanding the income limits and length of coverage, many states have streamlined their eligibility process. As of January 1994, forty-five states dropped the assets test in determining Medicaid eligibility. Thirty states added presumptive eligibility, which is immediate, short-term Medicaid eligibility at the provider site while formal determination is being made. Forty-two states shortened their application form, twenty-five states expedited eligibility determinations, twenty states allowed mail-in eligibility and twenty-eight states provided continuous eligibility for newborns.

Table 1 shows the recent impact of the expansions. Clearly many of the states have exceeded even the OBRA 1989 and 1990 mandates. By using either state funding or eligibility expansions authorized under Section 1902(r)(2) of the Social Security Act, eight states currently cover all teenagers to at least 100% of the FPL as of January 1994.

The key dimension of variation used in this paper is the length of health insurance coverage. The expansions offered health insurance for long periods of time for only some children, based on their birthday. To construct a family-specific income effect, therefore, I utilize variation in the age distribution of the children. The following table shows the variation created by a state taking up the optional OBRA 1987 legislation:

Child's Birthday	Eligible for Expansion?	Length of Coverage
12/25/88	Yes	8 more years
10/1/83	Yes	3.25 more years
9/30/83	No	0 years

It shows that a mother with a child born on December 25, 1988 would receive health insurance coverage for a full eight years and a child born on October 1, 1983 would receive coverage for more than three years. In contrast, a child born a day earlier, on September 30, 1983 would not receive coverage. Since the age distribution of older children is presumably exogenous, this creates useful within state variation in the generosity of Medicaid coverage.

2.2 Theoretical Effects of Health Insurance on Fertility

The early work and further refinements by Becker (1960, 1992) and Becker and Lewis (1973) provide the framework for analyzing the effect of income on fertility. He postulates that the family's utility function is:

(1) $U=U(G,N)$

(2) $N=qn$

where G represents other goods consumed by the household and N is the quality adjusted number of children. The second equation defines the quality adjusted number of children as the quality invested per child, q, multiplied by the total number of children, n. Quality, which may be thought of as attention from the parent as well as monetary investment, is the key difference between children and other economic goods. In this case, the utility function will give a demand curve for children, but the effect of income is not clear. Instead of increasing the quantity of children, n, in response to an income shock, a household might instead invest more in each child, q. He goes on to postulate that the elasticity of child quality with respect to income is large, while the elasticity of child quantity with respect to income is small. His model also makes predictions about fertility in response to men's and women's wages, which are beyond the scope of this paper.

In this case, the Medicaid expansions would enter through a dynamic budget constraint. The expansions offer two types of income effects: the contingent commodity of health insurance coverage for newborns and health insurance coverage for older children. This second income effect implies that the expansions should be more valuable to families with more covered children.[3] As Leibowitz (1991) notes, the reduction in medical care accounts for 5 to 9 percent of the total discounted costs of child rearing, and 10 to 16 percent of the total discounted expenditures on goods.

2.3 Previous Work on the Medicaid Expansions

There has been a fair bit of recent work analyzing the effects of the Medicaid expansions for pregnant women and young children. Yelowitz (1993a) examines the effect of the child health insurance expansions on a single mother's decision to work. Since the expansions relax the income limits where her children lose health insurance, it is expected that labor force participation should increase and welfare participation should decrease in response to the expansions. He finds significant effects on both these

[3] This is similar in spirit to one of the tests of job lock found in Madrian (1994).

outcomes and suggests that universal coverage may allow many recipients to leave the welfare rolls. Yelowitz (1993b) examines the effect of the expansions on the mother's decision to marry. By severing the link to family structure, the expansions offer new incentives to get married. By relaxing the income margin, the expansions allow a married mother to get divorced and still retain health insurance for her children. He finds that, on net, the expansions significantly increased the probability of marriage by severing the link to family structure, and after controlling for the potential outflows from marriage the results get even stronger.

Currie and Gruber (1994) examine the effects of earlier Medicaid expansions for pregnant women on the use of prenatal care and low birth weight. They find that the expansions lowered the incidence of infant mortality and low birthweight. A twenty percentage point increase in Medicaid eligibility among 15 to 44 year old women was associated with a decrease in infant mortality of seven percent. Shore-Shepard (1993) looks at take-up of the expansion benefits. She finds that the rules did increase enrollment independent of the recession, but the fraction of the eligible population enrolled actually decreased during the period. This should not be surprising since the woman made newly eligible by the expansion has a lower proclivity to participate than the average woman. The expansions offered health insurance coverage to children of higher income women, who are more likely to have health insurance through the job and are less likely to be familiar with the welfare system. Her findings are consistent with the hypothesis that the expansions acted as an avenue off of welfare for former recipients rather than acting as a free gift for newly eligible people.

3. Previous Literature on Policy Instruments and Fertility

3.1 AFDC Cash Benefits

While much of the emphasis in previous work has been on the influence of welfare benefits on the marriage decision rather than childbearing, some work has been done on the influence of cash benefits. Moffitt (1992) presents a thorough overview of the subject.[4]

Ellwood and Bane (1985), who focus only on first births, find that cash benefits have a weak positive association with fertility, though statistically insignificant. By using the data set with approximately 150 births, it is not clear whether the weak effects are because benefits do not influence on fertility or due to sampling error. They find some increase in the fraction of never-married women aged 24 to 34 with children in high benefit states. A key insight of their paper is controlling for state fixed effects, which controls for unmodelled factors within a state (such as religious composition or attitudes toward child bearing) that affect fertility independent of AFDC benefits.

Schultz (1994) uses the 1980 Census to find that Medicaid benefits are associated with *lower* levels of fertility among both black and white women, while lower AFDC cash benefits are associated with lower fertility among white women aged 15 to 24. He controls for the wage opportunities facing men in women in analyzing AFDC's

[4] Especially his table 9, which shows that most studies of welfare benefits and family structure examine headship rather than child bearing decisions. Murray (1993) provides a full account of previous work on illegitimacy, including critiques of Ellwood and Bane (1985), Winegarden (1988), Ozawa (1989), Bernstam (1988), Plotnick (1990), Lundberg and Plotnick (1990), Duncan and Hoffman (1990) and An, Haveman and Wolfe (1990).

importance. In addition, he pools both married and unmarried women together suggesting this would be more likely to lead to unbiased results. Since he uses a cross section, however, he is unable to control for state fixed effects, which Ellwood and Bane found to be important.

Finally, Leibowitz (1991) examines the effect of health insurance coverage using data from the Rand Health Insurance Experiment (HIE). She finds that women who were randomly assigned to receive free medical care for three to five years had 29 percent higher birth rates than women who were assigned to plans that required cost sharing. She finds some evidence of shifting in the timing of child bearing, rather than the level of children. After an initial delay, birth rates were higher in the second and third years of the HIE before declining in the fourth and fifth years.[5]

There are several key differences between the Medicaid expansions presented here and the HIE examined by Leibowitz. First, the Medicaid expansions amounted to a permanent increase in health insurance coverage while the HIE had an anticipated deadline. Second, I examine differences in health insurance eligibility rather than differences in health insurance characteristics (since all participants in the HIE were eligible). Finally, I am able to obtain a much large and more representative sample. The HIE was performed in only several locations, so it is not clear how applicable the results are to the nation. In addition, the HIE was conducted in the early 1970s, and the

[5] Burtless and Orr (1986) review some of the tradeoffs associated with controlled experiments. These include non-response bias, representativeness of the experimental sample, limited duration bias, replicability and Hawthorne effects, queueing bias, and partial versus general equilibrium effects. As Leibowitz acknowledges, the HIE is most likely to suffer from limited duration bias, because the participants knew in advance when their health insurance would expire.

importance of health insurance has grown enormously since then.

3.2 Tax Policy

In addition to work done on AFDC cash benefits, an interesting area that has been recently explored by Whittington, Alm and Peters (1990) is the effect of tax policy on fertility. This effect could occur because the value of the personal exemption for dependents is a function of family size. The personal exemption represents a subsidy for each child, which value of which depends on the marginal tax rate for the family. Using aggregate data from the United States from 1913 to 1984, they find that the value of the personal exemption leads to larger fertility responses than AFDC benefits. The personal exemption has a positive and significant effect on the national birthrate, though the elasticity of the birthrate with respect to the exemption is not large, ranging from 0.127 to 0.248. WAP note that the personal exemption, unlike AFDC cash benefits, it assured throughout the entire range of childhood. In contrast AFDC cash benefits are extended only to those in economic need, only to single parent families and may not necessarily be received over the entire dependency of the child. The Medicaid expansions, on the other hand, closer in spirt to the personal exemption. While the expansions are not conditioned on the mother's marital status, they do have income limits but these are typically two or three times the AFDC income limit. Zhang, Quan and Van Meergergen (1994) replicate these findings using aggregate Canadian data. Engelhart (1991) finds that the WAP results are sensitive to the modelling of the serial correlation, however. In some of his specifications, the impact of the personal exemption is unrealistically large.

4. Data Construction and Results

4.1 Data Construction

To construct a microdata sample to analyze the Medicaid expansions, I extracted all women between the ages of 15 and 44 (which is generally thought to be child bearing age) from the Current Population Survey (CPS), which is the unit of observation used in the analysis.[6] Using the 1989, 1990, 1991 and 1992 March CPS extracts, I obtained 145,300 observations. To these women, I linked information regarding the ages of any children they had. I then simulated potential eligibility for the Medicaid expansions. Using information regarding the timing, implementation and generosity of the Medicaid expansions obtained from the Intergovernmental Health Policy Project, I imputed Medicaid eligibility and length of coverage for each child based on his or her age.[7] It is important to note that in the later years of the sample, the treatment of infants is quite similar across states since OBRA 1989 and 1990 mandated health care coverage for six and eighteen years respectively. These mandates essentially remove any cross state variation in the contingent commodity of health insurance coverage for infants. Therefore, the only useful variation for the 1991 and 1992 for infants is the time dimension.

Figure 1 plots health care expenditures by the age of the child using the 1987

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[6] In addition, family size was restricted to ten persons or less.

[7] It was necessary to impute the birth month of the child, since the March CPS does not provide that information. This was done using the empirical birth distribution for 1987, obtained from the *Vital Statistics*. See Yelowitz (1993a) for more detail on this imputation procedure and potential measurement error issues.

National Medical Expenditure Survey (NMES).[8] The NMES is one of the few nationally representative sources of medical expenditure that disaggregates by child's age. As this figure shows, expenditures are highly non-linear. The average expenditure for an infant is nearly \$3,000 and then falls rapidly to slightly more than \$1,000 for a one-year old. From ages two to age twelve, average expenditure is fairly stable at approximately \$300 to \$500 per year. Health care expenditure then increases during the teenage years, increasing from approximately \$600 in the early teens to around \$800 in the late teens, in part due to increased accidents. The other three lines show the expenditure at the 25th, 50th and 75th percentiles. As expected, each of these are below the arithmetic mean because health expenditures are highly skewed. The 75th percentile has the same general shape as the arithmetic mean, decreasing rapidly during the early years of life, then flattening out, and finally increasing during the teenage years. The 50th and 25th percentiles also decline rapidly and then permanently flatten out. These plots suggest that the value of health insurance, and thus the income effect from the Medicaid expansions, is likely to vary substantially based on the age distribution of the children. Since these plots illustrate that most of the health care expenditure is front-loaded, then additional years of coverage may not be incredibly valuable. That is, moving from six to eighteen years of coverage will not result in a very large income effect because of declining expenditure and the fact that later expenditures are discounted.[9]

[8] I am grateful to David Cutler for providing me with this data.

[9] I use a discount rate of six percent.

While much discussion has been focused on how to find the cash value of in-kind benefits such as health insurance, the most common procedure has been to use the (arithmetic) average health care expenditure, which I will follow in this analysis. The average Medicaid expenditure has been utilized in Blank (1989), Winkler (1991), and Schultz (1994).[10] In constructing a value for the Medicaid expansions, I implicitly assume that moral hazard is absent. That is, I assume that the quantity of health care services demanded does not change with insurance status.

Table 2 gives (unweighted) summary statistics of the CPS sample. Roughly one-half the sample of women between 15 and 44 are married, slightly more than one-third have never been married and the remainder are either divorced, separated or widowed. While Yelowitz (1993b) explains that marital status itself could be a function of the Medicaid expansions, it will be fruitful to stratify the sample to see if the expansions affect fertility only through the decision to marry or whether the value of health insurance has independent explanatory power after controlling for marital status. The average educational attainment was 12.74 years of schooling. Thirty six percent of the sample finished high school, forty-four percent have at least some college, roughly fifteen percent have some high school and four percent did not attend high school. Education attainment could proxy for permanent income or labor market opportunities, which in turn affect the timing and number of children to bear. Eleven percent of the sample is black and eighty-four percent is white. Other work, primarily Schultz (1994) has found differences in fertility and marriage patterns across race. It is possible that different

[10] See Murray (1993) and Yelowitz (1993a) for discussions of such procedures, however.

cultural norms operate in different communities.

The birth rate in the sample is 5.65 percent. A birth, by necessity, is defined as having a child under age one present in the household.[11] Figure 2 compares birth rates from the CPS sample to data from the *Vital Statistics* for the years 1988 and 1989. The first fact that emerges is that birth rates are highly nonlinear, rising until ages 25 to 29 and declining thereafter. In all specifications, I will control for the mother's age with a full set of dummy variables. The second fact that emerges is that the CPS understates the birth rate for younger women, regardless of whether the sample is weighted or not. For the age group 15 to 17, the birth rate constructed from the CPS, 15.6 births per thousand women, is less than half of what the Vital Statistics data report, 36.5 births per thousand women. This problem becomes less important as the mother's age increases, and the CPS data mimic the Vital Statistics data quite well for groups aged 25 and beyond. The number of births per thousand women aged 30 to 34 are 71.7 in the CPS (unweighted) and 76.2 in the Vital Statistics. Several possible explanations can reconcile the undercount in the CPS data at younger women's ages. First, it is much more likely that a child born to a young woman will not be living with her. The baby may instead live with other relatives or be given up for adoption. Second, it is likely that younger women will misreport to the CPS. Third, since I define a birth as the presence of a child under age 1, it is possible that younger women slide into the next age bracket, that is, women who had their child when they were seventeen slide into the 18 to 19 group, or women who had their baby when they were nineteen slide into the 20 to 24 group.

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[11] Other work, such as Ellwood and Bane (1985), Leibowitz (1991) and Schultz (1994) use similar definitions.

In addition, the average number of older children which is defined as own children between the ages of one and eighteen, is 1.19 with a standard deviation of 1.22. The average age of women in my sample is 29.82. Finally, table 2 offers information on the income effects from the Medicaid expansions. The total income effect is defined as the sum of the potential income effect for having another child and the income effect for older children. On average, the total income effect (which is discounted) was quite large. The average is $5,897 while the median is $5,499. By separating this effect into its two separate parts, the contingent commodity for an infant and the income effect for older children, however, a different story appears. On average, the income effect for older children was $743. This will obviously be highly skewed as a function of family size, since a woman with no older children will receive an income effect of zero, as will a mother with older children who are ineligible for the expansion. The table shows that even at the 75th percentile, the income effect from covering older children is zero. The contingent commodity of health insurance coverage for the baby therefore contributes a great deal to the level of the income effect, but separating this effect by years shows that it contributes little to the *variation* in the health insurance subsidy. For the years 1991 and 1992, there is essentially no cross-state variation in this subsidy for children. On average, the subsidy for a newborn was $5,522 in 1991 with a standard deviation of $102 (where OBRA 1989 mandated coverage at least until age six, though a few states exceeded this requirement). In 1992, the average subsidy was $8,411 for all infants in all states, since OBRA 1990 guaranteed health coverage until the age of eighteen.[12] Since I control for both time-specific and state-specific effects in my regressions, there are essentially only two years of useful data to identify the effect of the contingent commodity.

Finally, it is important to note some limitations of the CPS relative to other data sets like the PSID. Unfortunately the CPS does not contain information on religious affiliation, which likely affects fertility. By including state fixed effects, I hope to control for state-wide attitudes, including religion that might influence fertility. The CPS does have several advantages. The primary advantages are its timeliness and sample size. I am able to observe thousands of births in the CPS during the time of the Medicaid expansions. Other work using the PSID observed less than 200 births. The CPS allows me to identify extremely precisely economic effects which may be small in magnitude.

4.2 Current Population Survey Results

The outcome of interest is whether or not the woman had a baby in response to the expansions. This discrete outcome is modeled as an index function:

$$(1) \qquad BIRTH_{ijt}^{*} = \beta_0 + \beta_1 VHI_{ijt} + \beta_2 X_{ijt} + \beta_3 STATE_j + \beta_4 TIME_t + \beta_5 MOMAGE_{ijt} + \beta_6 KIDNUM_{ijt} + \epsilon_{ijt}$$

where i indexes mothers, j indexes states and t indexes time period. VHI_{ijt} represents the present, discounted value of health insurance from the Medicaid expansion for this

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[12] To put this into relation to other benefits, Whittington, Alm and Peters (1990) estimate the the value of the tax subsidy from the personal exemption represents between 4 to 9 percent of the annual costs for the first child and 6 to 14 percent of the costs for subsequent children. At a discount rate of 5 percent, they show the present discounted value of the personal exemption is $4,000. Thus, the health insurance subsidy is in the same range as the personal exemption.

woman (which includes both the income effect for older children and the contingent commodity of health insurance for the infant). In several specifications, this variable will be replaced by its two components, $BABYSUB_{ijt}$ and $OLDERSUB_{ijt}$, the subsidies for the infant and older children respectively. X_{ijt} represents a set of individual covariates including educational attainment (less than high school, some high school and some college), race, and residence in a central city. In several specifications it also includes indicators for marital status (divorced, separated and never married) and the number of children. The omitted categories will be finished high school, non-black, and married. $STATE_j$ and $TIME_t$ represent dummy variables for state of residence and time period. These will help control for any state-specific or time-specific factor that affects fertility that is omitted from the regression. For instance, $STATE_j$ controls time invariant shocks within a state such as the religious composition state or state-specific attitudes toward childbearing that cannot be parameterized. $TIME_t$ controls nationally uniform, time varying shocks that affect fertility such as national economic conditions or changes in birth control technology.[13] Finally, $MOMAGE_{ijt}$ is a full set of indicator variables for the mother's age, which ranges from 15 to 44 and $KIDNUM_{ijt}$ is a set of indicator variables for the number of children between the ages of 1 and 18 present.

In this case we observe the discrete outcome of a birth if:

$$(2) \qquad \begin{array}{ll} BIRTH_{ijt} = 1 & \text{if } BIRTH_{ijt}^* > 0 \\ BIRTH_{ijt} = 0 & \text{otherwise} \end{array}$$

By assuming that ϵ_{ijt} is distributed normally and denoting Φ as the cumulative normal

[13] For instance, Butz and Ward (1979) examine countercyclical fertility and Leibowitz (1990) points out that health insurance coverage could exacerbate this trend. The TIME indicators help control for such cyclical conditions.

distribution, we obtain the probit equation,

$$(3)\qquad \text{Prob}(BIRTH_{ijt} = 1) = \Phi(\beta_0 + \beta_1 VHI_{ijt} + \beta_2 X_{ijt} + \beta_3 STATE_j + \beta_4 TIME_t + \beta_5 MOMAGE_{ijt} + \beta_6 KIDNUM_{ijt})$$

which is the primary parameterization.[14] It is hypothesized that the policy variable of main interest, the income effect from Medicaid, should have a positive effect on fertility, implying that $\beta_1 > 0$.

Table 3 presents the primary results, using the total value of the Medicaid expansions (VHI). All specifications include controls for education, race, central city, number of children between ages 1 and 18, time period, mother's age and a constant. In addition, columns (1) and (3) include indicators for divorced, separated and never married, while columns (2) and (4) omit marital status entirely. Columns (3) and (4) contain indicators for state fixed effects, while columns (1) and (2) do not. These final two columns correspond to the "difference-in-differences" specification by utilizing variation in the income effect within a state, over time. This table clearly shows that the income effect had a statistically significant, positive effect on fertility. In the preferred specification, column (4), which controls for state fixed-effects but omits marital status, the coefficient β_1 is estimated as 0.0445 with a standard error of 0.0022.[15] While the probit coefficient is not directly interpretable as a probability, we find that the marginal effect of increasing the value of health insurance by $1,000 is to increase fertility by 0.32

[14] The result from a linear probability model were similar for all specifications and available upon request.

[15] As Schultz (1994) explains, pooling the sample instead of stratifying by marital status is more likely to give unbiased estimates of the effect of benefits on fertility.

percent.[16] The other specification in columns (1), (2) and (3) all lead to highly significant estimates as well, with marginal effects which are slightly lower, ranging from 0.20 percent to 0.27 percent.

It is interesting to realize that while the statistical significance of health insurance is striking, other explanatory variables have a far greater economic contribution to fertility and seem to operate in plausible directions. Less education is associated with increased fertility. Education might proxy for the labor market opportunities or permanent income. Relative to a woman with a high school diploma, not finishing high school raises the probability of having a baby by approximately 3.2 percent and only having some high school increases the probability by roughly 2 percent. The effect of additional education beyond high school only has a small negative effect on fertility, however, lowering the probability by less than 1 percent. After controlling for marital status in columns (1) and (3), being black raises the probability of having a child by more than 3 percent. On the other hand, this race effect is much smaller (but still positive) when marital status is not controlled for in columns (2) and (4). While being single has a large negative impact on fertility (as should be expected), many black women are single, so part of the effect of being single is entering through the race indicator in columns (2) and (4). Relative to married women, the probability of birth by a never married woman is more than 8 percent lower, more than 7 percent lower for divorced women, and more than 4 percent lower for separated women. By comparing

[16] The marginal effects are calculated by evaluating each individuals probability of birth at $(VHI_{ijt} + 1000)/1000$ and at $(VHI_{ijt}/1000)$. The difference between these two predicted probabilities is then averaged across individuals. Similar methods are used for evaluating the marginal effects of the other coefficients as well.

column (1) to (2) or by comparing column (3) to (4), we can see that the effect of health insurance is nearly one-third larger when marital status is excluded. The marginal effect increases from 0.20 percent to 0.26 percent in columns (1) and (2), and from 0.27 percent to 0.32 percent in columns (3) and (4), with no appreciable change in the standard errors. This suggests effect of fertility is partly entering through an increased probability of marriage, which is plausible. Having children out-of-wedlock might be associated with some sort of stigma, which marriage eliminates. There is no clear pattern for residence in a central city, which is somewhat surprising. Living in a central city might be associated with a greater familiarity with the welfare system, so these women would be expected to be even more responsive to the expansions. The effect is indeed positive and significant after controlling for marital status, but reverses sign when marital status is not controlled for. The economic effect, nevertheless, is small in any case. Living in a central city changes the probability of having a child by less than 0.38 percent. Finally, the number of older children (entered linearly) has a strong and significant negative effect on fertility. This variable proxies for preferences in that families with more children are likely to have completed their childbearing. The presence of another older child lowers the probability of birth by around 1 percent.

Table 4 separates VHI_{ijt} into $BABYSUB_{ijt}$ and $OLDERSUB_{ijt}$, the subsidies for the infant and older children. By breaking out this income effect, table 4 shows that the effect of health insurance on fertility is concentrated in covering older children. While the contingent commodity of health insurance coverage for the infant apparently has a negative effect, it is insignificant when state fixed effects are including. In the preferred specification, column (4), shows the coefficient on *BABYSUB* is -0.0014 with a standard error of 0.0042. On the other hand, the effect of covering older children is positive and significant in all specifications. In column (4), the coefficient estimate on *OLDERSUB* is 0.0652 with a standard error of 0.0027. The marginal effect of an additional $1,000 of health insurance coverage for older children is 0.66%, while the marginal effect of covering an infant is only -0.01%. Thus, the economic importance of covering older children far outweighs the importance of covering the infant. The potential explanation for this seemingly anomalous result is suggested from the means in table 2. Since the federal mandates effectively removed any cross-state variation in the subsidy for babies for the final two years of the sample, then there is unfortunately too little variation to convincingly identify any effect of expansions for infants.[17] While the subsidy for babies has a significant negative effect on fertility when state fixed effects are excluded, there is a strong argument that this correlation is driven by unobservable characteristics of the state. For instance, the states that initially implemented the expansions tended to be located in the South, and it is possible the baby subsidy is simply capturing some attitude towards childbearing for that region. In any case, the economic effect is still five times smaller than for the income effect of covering older children. Column (2), which omits state fixed effects and omits marital status, shows that an additional $1,000 to the baby subsidy lowers fertility by 0.12%, while $1,000 directed towards older children raises fertility by 0.65%. The other coefficients are very similar to table 3. Once again less education and being black significantly raise fertility, while being single

[17] In future work, Currie, Gruber and Yelowitz plan to analyze the effect of some pregnancy expansions which occurred several years before the expansions analyzed in this paper.

or having older children significantly lower fertility.

Table 5 further investigates the hypothesis that the effect of the expansions on fertility is entering through increased marriage rather than through some income effect that independently affects fertility. Yelowitz (1993b) shows that these Medicaid expansions did increase the probability of marriage. To address this issue, I restrict the sample to 75,856 married women in column (1). The specification includes controls for the mother's education, race, central city, number of children between 1 and 18, time effects, mother's age effects, state effects and a constant. This column illustrates that the income effect from the expansions increased fertility, independent of the effect on marriage, even though married women tend to be richer, have health insurance through other sources and are not as familiar with the welfare system as single women. The coefficient β_1 is estimated as 0.0256 with a standard error of 0.0027. The marginal effect of another $1,000 of health insurance coverage is to increase the probability of birth by 0.36%, very close to the estimate obtained in table 1. While this lends some support to the hypothesis that the income effect itself increases fertility, it is important to note that it is not clear how marriage and childbearing should jointly react to the expansions. A couple could get married in response to having a baby, or having a baby could conceivably drive the father away. The other covariates in table 5 enter slightly differently than before. While less education (relative to high school) raises fertility, so does some college. The effects of being black or living in a central city are negative but insignificant. Finally, the number of older children has a strong negative effect on fertility. An addition child lowers the probability of birth by more than 2 percent.

Columns (2) and (3) of table 3 further stratify the sample into married women whose husbands do have private health insurance and married women whose husbands do not (and include the same covariates as the first column). This results in 59,079 and 16,777 observations respectively. As a first thought, we might expect that the expansions should have a stronger income effect on the second group, those without health insurance. It might therefore seem initially surprising that the effect of the expansions is stronger on those with private health insurance. The coefficient estimate for those with health insurance is 0.0268 with a standard error of 0.0032, while the estimate is 0.0206 with a standard error of 0.0052 for those without health insurance. The marginal effects are also stronger for the first group. The explanation is adverse selection. As Leibowitz (1991) notes, pregnancy is an anticipated, high-cost event. In this case, those families who plan on having another child might seek out health insurance coverage, so that rather than health insurance coverage causing additional births, it is births causing health insurance coverage.

Table 6 reestimates the original model restricting the sample to women in the ten largest states with less than five older children present. These states include California, New York, Texas, Florida, Pennsylvania, Michigan, Illiois, New Jersey, Ohio and North Carolina. These restrictions lead to a more homogenous sample and leave 72,662 observations. In this case, I include a set of third order interactions between the ten states, four time periods, and five family sizes (that is, either 0, 1, 2, 3 or 4 older children present). By including these interactions, I am attempting to control for other factors that vary over states or over time that affect might differentially affect fertility of larger families from smaller families. For instance, the Earned Income Tax Credit (EITC) has recently offered different schedules based on family sizes. This credit now varies from 7 to 40 percent based on the number of children present.[18] The level of AFDC cash benefits varies across states, over time and by family size.[19] The marginal benefit of having another child on AFDC varies tremendously because the first child entitles the mother to Medicaid benefits, whereas the second child only adds health insurance for one person. I had to restrict the sample to only the ten largest states and only families with less than five children because of computational constraints.

Each specification includes variables for education, race, residence in a central city and a constant (in addition to the third order interaction). Column (1) also includes indicators for marital status. The results of health insurance are stronger when the sample is restricted to a more homogenous group. The coefficient β_1 is estimated to be 0.0837 with a standard error of 0.0056 when marital status is included, and 0.1057 with a standard error of 0.0057 when it is omitted. These translate into marginal effects of 0.43% and 0.49%, respectively, from an additional $1,000 of health insurance coverage. The other coefficients enter in the same fashion as table 3: less education and being black have significant, positive effects on fertility, whereas being single has a significant negative effect.

Table 7 attempts to replicate the approach of Ellwood and Bane (1985) be restricting the sample to *first* births. This leaves 54,449 women from the original

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[18] See Eissa and Leibman (1993) and Scholz (1993) for more discussion of the EITC.

[19] Schultz (1994) notes that the correlation of AFDC benefits across family sizes in different states is quite high, on the order of .9.

145,300. In this case VHI_{ijt} must equal $BABYSUB_{ijt}$ because the number of older children equals zero. The results of this procedure are in line with their findings for AFDC benefits: the value of health insurance has no effect on fertility. Columns (1) and (2) which include state fixed effects have negative and insignificant signs. Columns (3) and (4) recognize that much of the variation in the income effect is remove by controlling for time, and thus omit the time dummies. In this case, column (3) shows an insignificant effect on fertility (though positive), while the coefficient in column (4) continues to be negative and insignificant. Thus, the effects of income on fertility appear to enter through covering older children.

Since Schultz (1994) tends to find different responses to AFDC benefits across race (with lower AFDC benefits associated with lower fertility levels among white women aged 15 to 24 but not among Black women), table 8 stratifies the sample into Black and white. There are 16,061 Blacks and 129,199 whites in the sample. All specifications control for education, residence in a central city, number of older children present, state fixed effects, time fixed effects, mother's age, and a constant. In addition columns (1) and (3) included marital status indicators. This table shows that the effect are significant and positive for both groups, but stronger for Blacks. An additional \$1,000 of health insurance coverage implies an increase in the probability of birth of 0.39 percent for Blacks and 0.30 percent for whites. The larger response for Blacks might occur because of greater familiarity with the welfare system, which means they may be more likely to know about and respond to the expansions. More education and being single have much stronger negative effects on fertility for whites than Blacks. On the other hand, the effect of older children is approximately the same for the groups. In either case, the presence of older children significantly reduces fertility by approximately 1 percent.

Finally, table 9 tests the proposition that the legislation was endogenous. In this case, the argument would likely suggest that the coefficients are biased downward. That is, *individual states delayed implementation of the expansions because they realized that a large fertility response would occur.* In this case, it is advantageous to delay implementation in order to save money on Medicaid expenditure. To explore this, I separate the states according to their generosity before the 1989 and 1990 OBRA mandates. I classify fifteen states (42,661 observations) as "stingy" if their expansions were only for infants under 100 percent of the FPL by December 1989. I classify another fifteen states (37,416 observations) as "generous" if they had expanded Medicaid to children ages six or above and to at least 100 percent of the FPL.[20] If the legislation was endogenous, then we should expect a larger coefficient for the stingy states, since they expect a large fertility response. In fact, we observe little evidence of this occuring. We cannot reject the null hypothesis of equality of coefficients. By comparing columns (1) and (3), where marital status is including, the coefficient estimate β_1 is 0.0460 for stingy states (with a standard error of 0.0050) and is actually larger at 0.0467 for generous states (with a standard error of 0.0052). The same basic inferences hold up by comparing columns (2) and (4), which exclude marital status. On the other hand, the

[20] The stingy states include: Alabama, Colorado, Connecticut, Idaho, Illinois, Montana, New Hampshire, New York, North Dakota, Ohio, South Dakota, Utah, Virginia, Wisconsin and Wyoming. The "generous" states include: Arizona, Arkansas, Florida, Iowa, Loisiana, Minnesota, Nevada, North Carolina, Pennsylvania, Rhode Island, South Carolina, Tennessee, Vermont, Washington and West Virginia.

marginal effect are indeed larger (by around 50 percent) for the stingy states. Therefore there is weak evidence of endogenous legislation, but in any respect, it suggests that the fertility response is *underestimated*.

5. Conclusions and Directions for Additional Research

This paper has explored the role of health insurance in fertility. The key finding is that health insurance coverage significantly increases fertility. An additional $1,000 of health insurance coverage would increase the probability of birth by 0.33 percent. As Madrian (1994) notes, the perceived value of health insurance increases with family size. In some sense then, health insurance coverage acts as a pro-natalist policy. Since covering children would encourage births, the expenditure on newborns could easily dominate the savings from reduced AFDC expenditure of single mother increasing their labor supply.

One issue that I was unable to resolve in this paper was: how important is the contingent commodity of health insurance for the infant on fertility? The expansions I examine were quite generous to infants, and the federal mandates imposed in the later half of the sample remove much of the cross state variation. Hence, I am unable to separately identify this effect on fertility. A more promising identification strategy would be to examine the earlier pregnancy expansions used in Currie and Gruber (1993). In mid 1980s, states had the option of covering pregnant women and infants, which might create a more useful source of cross state variation. A second issue that could be explored in panel data is whether the expansions affected the timing of births or the level of births.

Finally, it could be fruitful to jointly model the outcomes of child quantity and child quality. Other work, such as Currie and Gruber (1993) focuses exclusively on child quality (by examining birthweight and infant health), but little work has actually focused on both outcomes simultaneously. This could allow direct tests of Becker's hypothesis that the elasticity of child quality with respect to income is larger than the elasticity of child quantity.

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Medical Expenditure by Child's Age
(source: 1987 NMES)

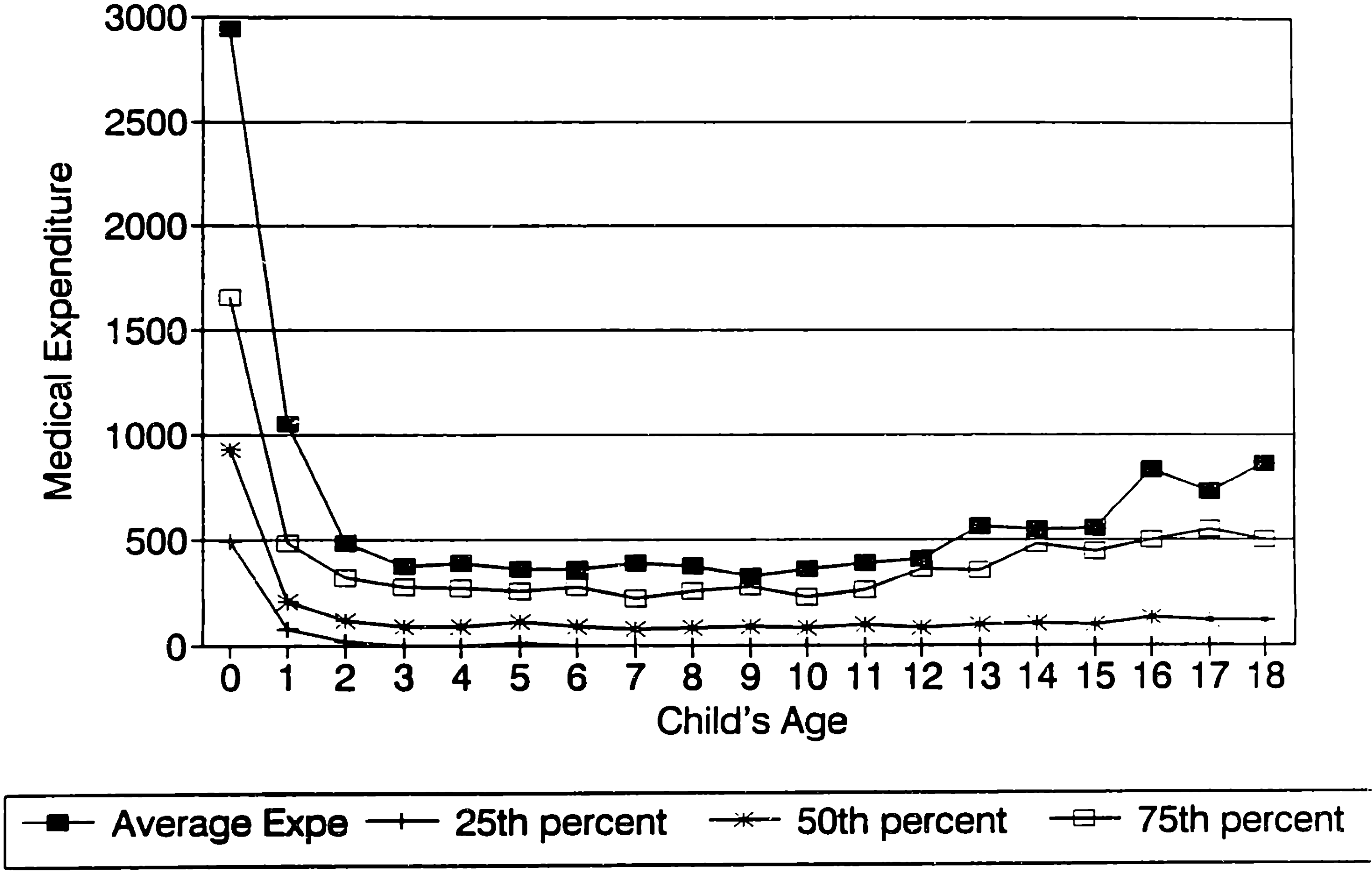

FIGURE 2
Birth rates across data sources

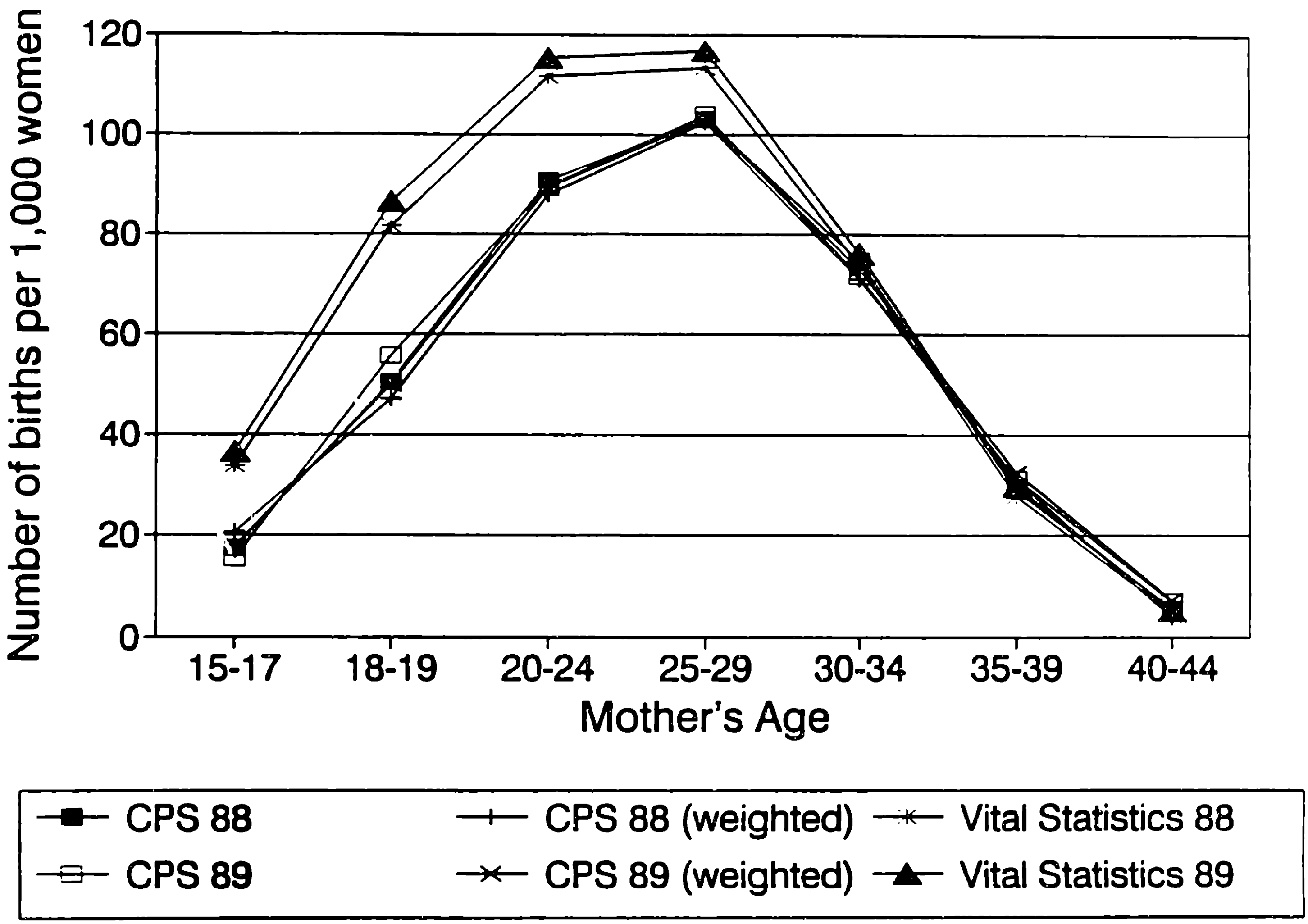

Table 1
State Health Insurance Expansions beyond OBRA 1990
January, 1994

State	Children covered until age:	Percentage of FPL	Financing Mechanism
Arizona	14	100	Medicaid
California	1	200	State Funds
Connecticut	6	185	Medicaid
Delaware	19	100	Medicaid
Georgia	19	100	Medicaid
Maine	19	125	Medicaid
Maryland	10	185	Medicaid
Massachusetts	6	200	State Funds
Minnesota	18	275	State Funds
Missouri	19	100	Medicaid
New Hampshire	11	170	Medicaid
New Jersey	1	300	State Funds
New York	13	160	State Funds
Pennsylvania	6	185	State Funds
Vermont	18	225	Medicaid
Virginia	19	100	Medicaid
Washington	19	100	Medicaid
West Virginia	19	150	Medicaid
Wisconsin	5	155	Medicaid

Source: National Governor's Association, 1994.

Table 2
Summary Statistics for Variables used in Analysis

<u>Marital Status</u>	
% Married	52.2
% Divorced	8.28
% Separated	3.24
% Never Married	35.4
<u>Education</u>	
Average Educational Attainment	12.74 (2.61)
% Less than High School	4.21
% Some High School	14.74
% High School Diploma	36.2
% Some College	44.8
<u>Race</u>	
Black	11.08
White	84.35
Other	4.56
Mother's Age	29.82 (8.31)
Number of children ages 1 to 18	1.19 (1.22)
Birth rate %	5.65
<u>Total Income Effect</u>	
25th percentile	$4,672
50th percentile	5,499
75th percentile	8,411
mean (standard deviation)	5,897 (3,970)
<u>Income Effect for Older Children</u>	
75th percentile	$0
mean (standard deviation)	743 (2,140)
<u>Potential Income Effect for Infants</u>	
25th percentile	$4,672
50th percentile	5,499
75th percentile	8,411
mean (standard deviation)	5,153 (2,750)
<u>Income Effect for Infants by Year</u>	
1989	$2,822 (2,559)
1990	3,751 (2,448)
1991	5,522 (102)
1992	8,411 (0)

Source: Author's tabulation of Current Population Survey

TABLE 3
Probit model on all women aged 15 to 44
Dependent variable is: Baby Born? (1=yes)

	(1)	(2)	(3)	(4)
Value of health insurance/1000	**.0256 (.0020)**	**.0322 (.0020)**	**.0364 (.0023)**	**.0445 (.0022)**
Less than high school	.2735 (.0282)	.2524 (.0273)	.2741 (.0288)	.2534 (.0278)
Some high school	.2003 (.0198)	.1561 (.0192)	.2118 (.0199)	.1623 (.0193)
Some college	-.0368 (.0131)	-.0809 (.0125)	-.0433 (.0131)	-.0849 (.0126)
Black	.2950 (.0184)	.0653 (.0176)	.3351 (.0195)	.0953 (.0186)
Central city	.0379 (.0136)	-.0233 (.0131)	.0222 (.0148)	-.0335 (.0142)
Divorced	-.6198 (.0285)	---	-.6171 (.0285)	---
Separated	-.3182 (.0322)	---	-.3165 (.0323)	---
Never married	-.9470 (.0176)	---	-.9552 (.0178)	---
Number of own children aged 1 to 18	-.1182 (.0058)	-.0674 (.0055)	-.1329 (.0060)	-.0843 (.0057)
onstant	-1.8682 (.0901)	-2.8071 (.0864)	-2.0133 (.1108)	-2.9878 (.1065)
log-likelihood	-27435	-29238	-27314	-29113
STATE dummies	No	No	Yes	Yes

Notes: Standard errors in parenthesis. Included TIME and MOTHER'S AGE dummies in all specifications. 145,300 observations of women aged 15 to 44 from the 1989, 1990, 1991 and 1992 March Current Population Survey.

TABLE 3
Marginal Effects
Dependent variable is: Baby Born? (1=yes)

	(1)	(2)	(3)	(4)
Value of health insurance/1000	**.0020**	**.0026**	**.0027**	**.0032**
Less than high school	.0321	.0324	.0321	.0325
Some high schooi	.0224	.0188	.0238	.0195
Some college	-.0035	-.0082	-.0040	-.0085
Black	.0345	.0072	.0399	.0106
Central city	.0038	-.0024	.0022	-.0035
Divorced	-.0709	---	-.0706	---
Separated	-.0438	---	-.0435	---
Never married	-.0884	---	-.0887	---
Number of own children aged 1 to 18	-.0109	-.0068	-.0120	-.0083
STATE dummies	No	No	Yes	Yes

Notes: Included TIME and MOTHER'S AGE dummies in all specifications. 145,300 observations of women aged 15 to 44 from the 1989, 1990, 1991 and 1992 March Current Population Survey.

TABLE 4
Probit model on all women aged 15 to 44
Dependent variable is: Baby Born? (1=yes)

	(1)	(2)	(3)	(4)
Subsidy to baby/1000	**-.0105** **(.0033)**	**-.0099** **(.0031)**	**-.0021** **(.0043)**	**-.0014** **(.0042)**
Value to older children/1000	**.0532** **(.0028)**	**.0645** **(.0027)**	**.0538** **(.0028)**	**.0652** **(.0027)**
Less than high school	.2791 (.0283)	.2578 (.0274)	.2766 (.0289)	.2553 (.0279)
Some high school	.2035 (.0199)	.1592 (.0193)	.2129 (.0200)	.1635 (.0194)
Some college	-.0407 (.0131)	-.0834 (.0126)	-.0449 (.0132)	-.0856 (.0126)
Black	.2983 (.0185)	.0696 (.0177)	.3350 (.0195)	.0953 (.0187)
Central city	.0318 (.0137)	-.0298 (.0132)	.0213 (.0148)	-.0341 (.0142)
Divorced	-.6150 (.0285)	---	-.6138 (.0286)	---
Separated	-.3187 (.0323)	---	-.3180 (.0324)	---
Never married	-.9372 (.0176)	---	-.9490 (.0178)	---
Number of own children aged 1 to 18	-.1455 (.0062)	-.0992 (.0059)	-.1495 (.0063)	-.1040 (.0060)
Constant	-1.6832 (.0916)	-2.5744 (.0880)	-1.7998 (.1129)	-2.7219 (.1087)
log-likelihood	-27337	-29094	-27260	-29030
STATE dummies	No	No	Yes	Yes

Notes: Standard errors in parenthesis. Included TIME and MOTHER'S AGE dummies in all specifications. 145,300 observations of women aged 15 to 44 from the 1989, 1990, 1991 and 1992 March Current Population Survey.

TABLE 4
Marginal Effects
Dependent variable is: Baby Born? (1=yes)

	(1)	(2)	(3)	(4)
Subsidy to baby/1000	**-.0011**	**-.0012**	**-.0002**	**-.0001**
Value to older children/1000	**.0051**	**.0065**	**.0051**	**.0066**
Less than high school	.0328	.0331	.0324	.0327
Some high school	.0228	.0191	.0239	.0196
Some college	-.0038	-.0084	-.0042	-.0086
Black	.0348	.0076	.0398	.0106
Central city	.0032	-.0031	.0021	-.0035
Divorced	-.0698	---	-.0699	---
Separated	-.0434	---	-.0434	---
Never married	-.0870	---	-.0879	---
Number of own children aged 1 to 18	-.0131	-.0097	-.0134	-.0101
STATE dummies	No	No	Yes	Yes

Notes: Included TIME and MOTHER'S AGE dummies in all specifications. 145,300 observations of women aged 15 to 44 from the 1989, 1990, 1991 and 1992 March Current Population Survey.

TABLE 5
Probit model on married women aged 15 to 44
Dependent variable is: Baby Born? (1=yes)

	All married women	Husband has HI	Husband without HI
Value of health insurance/1000	**.0256**	**.0268**	**.0206**
	(.0027)	**(.0032)**	**(.0052)**
Less than high school	.1980	.1699	.1562
	(.0359)	(.0579)	(.0490)
Some high school	.0237	-.0288	.0364
	(.0282)	(.0395)	(.0422)
Some college	.0796	.0944	.0013
	(.0155)	(.0177)	(.0345)
Black	-.0099	-.0285	.0279
	(.0316)	(.0400)	(.0525)
Central city	-.0208	-.0261	-.0179
	(.0187)	(.0224)	(.0349)
Number of own children aged 1 to 18	-.1342	-.1574	-.0762
	(.0073)	(.0088)	(.0135)
Constant	-6.6278	-6.4804	-6.6618
	(.1710)	(.2430)	(.2926)
N	75,856	59,079	16,777
log-likelihood	-19205	-14276	-4859
STATE dummies	Yes	Yes	Yes

Notes: Standard errors in parenthesis. Included TIME and MOTHER'S AGE dummies in all specifications. Data from the 1989, 1990, 1991 and 1992 March Current Population Survey.

TABLE 5
Marginal Effects
Dependent variable is: Baby Born? (1=yes)

	All married women	Husband has HI	Husband without HI
Value of health insurance/1000	**.0036**	**.0037**	**.0029**
Less than high school	.0348	.0292	.0272
Some high school	.0037	-.0044	.0059
Some college	.0131	.0156	.0002
Black	-.0016	-.0046	.0045
Central city	-.0034	-.0042	-.0028
Number of own children aged 1 to 18	-.0207	-.0238	-.0118
N	75,856	59,079	16,777
STATE dummies	Yes	Yes	Yes

Notes: Included TIME and MOTHER'S AGE dummies in all specifications. Data from the 1989, 1990, 1991 and 1992 March Current Population Survey.

TABLE 6
Ten largest states and less than 5 older children
Probit model on women aged 15 to 44
Dependent variable is: Baby Born? (1=yes)

	(1)	(2)
Value of health insurance/1000	**.0837** **(.0056)**	**.1057** **(.0057)**
Less than high school	.2563 (.0362)	.2837 (.0351)
Some high school	.2178 (.0279)	.1853 (.0271)
Some college	-.0832 (.0191)	-.1147 (.0183)
Black	.3044 (.0262)	.0716 (.0250)
Central city	.0360 (.0190)	-.0204 (.0183)
Divorced	-.6659 (.0433)	---
Separated	-.3091 (.0436)	---
Never married	-.9513 (.0251)	---
Constant	-3.7352 (.6765)	-4.9493 (.6508)
log-likelihood	-13479	-14373
STATE*TIME*(#KID>0)	Yes	Yes

Notes: Standard errors in parenthesis. Included MOTHER'S AGE dummies in all specifications. 72,662 observations of women aged 15 to 44 from the 1989, 1990, 1991 and 1992 March Current Population Survey.

TABLE 6
Marginal Effects
Dependent variable is: Baby Born? (1=yes)

	(1)	(2)
Value of health insurance/1000	**.0043**	**.0049**
Less than high school	.0299	.0371
Some high school	.0248	.0226
Some college	-.0077	-.0113
Black	.0352	.0078
Central city	.0036	-.0021
Divorced	-.0745	---
Separated	-.0429	---
Never married	-.0894	---
STATE*TIME*(#KID>0)	Yes	Yes

Notes: Included MOTHER'S AGE dummies in all specifications. 72,662 observations of women aged 15 to 44 from the 1989, 1990, 1991 and 1992 March Current Population Survey.

TABLE 7
Replication of Ellwood and Bane: restrict to first births
Probit on women aged 15 to 44 without older children
Dependent variable is: Baby Born? (1=yes)

	(1)	(2)	(3)	(4)
Value of health insurance/1000	**-.0054** **(.0072)**	**-.0031** **(.0067)**	**.0006** **(.0038)**	**-.0023** **(.0035)**
Less than high school	.1840 (.0550)	.1761 (.0509)	.1844 (.0550)	.1765 (.0509)
Some high school	.2310 (.0365)	.1907 (.0346)	.2313 (.0365)	.1911 (.0346)
Some college	-.1545 (.0215)	-.2078 (.0199)	-.1543 (.0215)	-.2077 (.0199)
Black	.3800 (.0325)	.1263 (.0306)	.3801 (.0325)	.1264 (.0306)
Central city	.0115 (.0238)	-.0470 (.0220)	.0117 (.0238)	-.0469 (.0220)
Divorced	-.8096 (.0524)	---	-.8091 (.0524)	---
Separated	-.4965 (.0689)	---	-.4959 (.0689)	---
Never married	-1.1245 (.0243)	---	-1.1240 (.0243)	---
Constant	-.6875 (.1788)	-1.6544 (.1718)	-.7236 (.1763)	-1.6654 (.1696)
log-likelihood	-10156	-11482	-10157	-11483
TIME dummies	Yes	Yes	No	No
STATE dummies	Yes	Yes	Yes	Yes

Notes: Standard errors in parenthesis. Included MOTHER'S AGE dummies in all specifications. 54,449 observations of women aged 15 to 44 from the 1989, 1990, 1991 and 1992 March Current Population Survey.

TABLE 7
Marginal Effects
Dependent variable is: Baby Born? (1=yes)

	(1)	(2)	(3)	(4)
Value of health insurance/1000	**-.0005**	**-.0003**	**.0001**	**-.0002**
Less than high school	.0021	.0254	.0222	.0254
Some high school	.0286	.0278	.0286	.0278
Some college	-.0150	-.0230	-.0150	-.0230
Black	.0457	.0151	.0457	.0152
Central city	.0011	-.0051	.0011	-.0051
Divorced	-.1081	---	-.1080	---
Separated	-.0786	---	.0785	---
Never married	-.1262	---	.1261	---
TIME dummies	Yes	Yes	No	No
STATE dummies	Yes	Yes	Yes	Yes

Notes: Included MOTHER'S AGE dummies in all specifications. 54,449 observations of women aged 15 to 44 from the 1989, 1990, 1991 and 1992 March Current Population Survey.

TABLE 8
Probit model on all women aged 15 to 44, separated by race
Dependent variable is: Baby Born? (1 =yes)

	(1) White	(2) White	(3) Black	(4) Black
Value of health insurance/1000	**.0313** **(.0025)**	**.0419** **(.0024)**	**.0562** **(.0063)**	**.0565** **(.0062)**
Less than high school	.2961 (.0303)	.2753 (.0291)	.0629 (.1046)	.0315 (.1043)
Some high school	.2266 (.0221)	.1820 (.0213)	.0818 (.0480)	.0574 (.0478)
Some college	-.0122 (.0142)	-.0727 (.0134)	-.1911 (.0385)	-.1795 (.0382)
Central city	.0167 (.0163)	-.0401 (.0155)	.0383 (.0392)	.0165 (.0389)
Divorced	-.6408 (.0309)	---	-.3069 (.0776)	---
Separated	-.3010 (.0368)	---	-.1230 (.0710)	---
Never married	-1.1152 (.0208)	---	-.3260 (.0426)	---
Number of own children aged 1 to 18	-.1402 (.0066)	-.0821 (.0062)	-.1086 (.0154)	-.0967 (.0151)
Constant	-1.9433 (.1330)	-3.1519 (.1282)	-7.0126 (.6450)	-7.0281 (.5512)
log-likelihood	-23612	-25528	-3455	-3487
STATE dummies	Yes	Yes	Yes	Yes

Notes: Standard errors in parenthesis. Included TIME and MOTHER'S AGE dummies in all specifications.

TABLE 8
Marginal Effects
Dependent variable is: Baby Born? (1=yes)

	(1) White	(2) White	(3) Black	(4) Black
Value of health insurance/1000	**.0022**	**.0030**	**.0043**	**.0039**
Less than high school	.0324	.0348	.0085	.0039
Some high school	.0236	.0216	.0112	.0073
Some college	-.0010	-.0071	-.0220	-.0195
Central city	.0015	-.0041	.0046	.0018
Divorced	-.0718	---	-.0367	---
Separated	-.0415	---	-.0165	---
Never married	-.0933	---	-.0385	---
Number of own children aged 1 to 18	-.0119	-.0080	-.0120	-.0100
STATE dummies	Yes	Yes	Yes	Yes

Notes: Included TIME and MOTHER'S AGE dummies in all specifications.

TABLE 9
Probit model testing Endogenous Legislation
Dependent variable is: Baby Born? (1=yes)

	(1) Stingy	(2) Stingy	(3) Generous	(4) Generous
Value of health insurance/1000	**.0460** **(.0050)**	**.0533** **(.0049)**	**.0467** **(.0052)**	**.0578** **(.0050)**
Less than high school	.2177 (.0624)	.1862 (.0599)	.2607 (.0691)	.1712 (.0671)
Some high school	.2025 (.0383)	.1368 (.0372)	.1848 (.0396)	.1391 (.0384)
Some college	-.0247 (.0239)	-.0665 (.0230)	-.0259 (.0262)	-.0760 (.0250)
Black	.3300 (.0375)	.1041 (.0358)	.4182 (.0369)	.1271 (.0345)
Central city	.0212 (.0282)	-.0436 (.0270)	.0457 (.0308)	-.0244 (.0295)
Divorced	-.5686 (.0516)	---	-.6539 (.0574)	---
Separated	-.3741 (.0638)	---	-.3332 (.0631)	---
Never married	-.9729 (.0336)	---	-.9959 (.0367)	---
Number of own children aged 1 to 18	-.1334 (.0110)	-.0907 (.0104)	-.1612 (.0131)	-.1066 (.0124)
Constant	-1.8356 (.1927)	-2.7497 (.1847)	-2.0559 (.2214)	-3.1214 (.2113)
log-likelihood	-8011	-8534	-6774	-7242
STATE dummies	Yes	Yes	Yes	Yes

Notes: Standard errors in parenthesis. Included TIME and MOTHER'S AGE dummies in all specifications. Stingy states include: Alabama, Colorado, Connecticut, Idaho, Illinois, Montana, New Hampshire, New York, North Dakota, Ohio, South Dakota, Utah, Virginia, Wisconsin and Wyoming. Generous states include: Arizona, Arkansas, Florida, Iowa, Loisiana, Minnesota, Nevada, North Carolina, Pennsylvania, Rhode Island, South Carolina, Tennessee, Vermont, Washington and West Virginia.