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EVALUATING THE EFFECT OF MEDICAID AND STATE CHILDREN'S HEALTH INSURANCE PROGRAM EXPANSIONS

By

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ABSTRACT

EVALUATING THE EFFECT OF MEDICAID AND STATE CHILDREN'S HEALTH INSURANCE PROGRAM EXPANSIONS

By

Jason R. Davis

Title XXI of the Balanced Budget Act of 1997 resulted in increased eligibility limits for children in all 50 states. Under Title XXI, states receive enhanced federal matching rates to insure low-income children who would otherwise not qualify for Medicaid. States have the option of expanding eligibility through Medicaid expansions, through the creation of state programs, separate from Medicaid, or from a combination of the two. The key differences between Medicaid and the state programs are that the state programs have more freedom to impose minimal cost-sharing measures and to require that children be uninsured for a specific length of time in order to become eligible.

This dissertation examines the impact these expansions have had on reducing the number of uninsured children in the United States. A probit model is used to estimate the change in insurance status, given the changes in eligibility limits for Medicaid and state programs, based on March 1997-2001 Current Population Survey data. It is estimated that the Title XXI expansions resulted in a 5.9% decrease in the number of uninsured children, based on the simulations performed on the 2000 sample. For states using only Medicaid expansions, the estimated decrease is 8.2%, compared to 5.0% in states using only state program expansions and 7.0% in states using combination expansions.

Comparing the effects of the Title XXI expansions across different poverty level groups, there was not a significant effect for children with family income above 250% of

the federal poverty level. States using only Medicaid expansions had the largest impact on uninsurance rates for children below the federal poverty level. For states using only state program expansions or combination expansions, the largest impact on uninsurance rates is observed for children with family income of 100-150% of the federal poverty level.

Average partial effects are also estimated to provide a more direct comparison of the effect of an average-sized expansion through either Medicaid or through a state program. Overall, it is estimated that an average sized Medicaid expansion would result in a 1.21 percentage-point reduction in the number of uninsured children, compared to a 0.65 percentage-point reduction through an equivalent state program expansion.

Based on specification tests, there is some statistical evidence that Medicaid expansions have a stronger impact on reducing the likelihood of being uninsured, compared to state program expansions. However, the simulated changes in insurance status and average partial effects of the expansions do not show any significant differences resulting from Medicaid expansions, compared to state program expansions.

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CHAPTER 1

INTRODUCTION

Despite strong economic growth, the share of American children without health insurance increased through much of the 1990's. The percentage of children covered by employment-related insurance increased from 58.1% in 1994 to 60.5% in 1998, and to 61.5% in 1999. In spite of this trend, Medicaid coverage was declining and the number of uninsured children was increasing from 14.2% in 1994 to 15.4% in 1998. Between 1998 and 1999, however, the percentage of uninsured children ages 0-17 fell from 15.4% to 13.9% (Fronstin, 2000b). While the continued growth of the U.S. economy helped reduce the number of uninsured children, so did the increase in availability of public health insurance for children.

Congress has taken several measures to assure access to health insurance for low-income children and pregnant women, beginning with the creation of the Medicaid program in 1965. Traditionally, Medicaid was available only to specific populations, such as those who qualified for Aid to Families with Dependent Children (AFDC) payments, low-income aged and disabled people, and those who were "medically needy" (meaning that they had recently incurred large medical expenses, relative to their income). Prior to 1984, low-income children were typically eligible for Medicaid only if they were in families receiving AFDC. Beginning in 1984, the link between AFDC and Medicaid eligibility was relaxed, so that Medicaid eligibility could be expanded to low-income children who did not qualify for AFDC. From 1986 to 1992, there were numerous expansions in Medicaid eligibility for pregnant women and children, which occurred through both federal mandates and optional state expansions. The most recent attempt to ensure that all children in the U.S. have adequate access to medical care was through the passage of Title XXI, States Children's Health Insurance Program (SCHIP), of the Balanced Budget Act of 1997. Title XXI allows states to receive enhanced federal matching funds to reduce the number of uninsured, low- income children through the following options: 1) they can expand their Medicaid income eligibility threshold, 2) they can create new state programs to insure low-income children not eligible for Medicaid, or 3) they can use a combination of the two approaches. States were allowed to receive the enhanced federal matching funds for approved expansions beginning October 1, 1997; for fiscal years 1998 and 1999, 46 states took advantage of these opportunities and received a total of approximately $1 billion in enhanced federal matching funds (Kenney, Ullman, and Weil, 2000). By the end of fiscal year 2000, all states had implemented Title XXI expansions (Mathematica Policy Research, Inc.).

Despite the potential of these initiatives to have far-reaching effects on the health-insurance coverage for children and the size of the budget allocation for this program, not much is yet known about the effects of the Title XXI expansions. While advocates claim that insurance coverage will increase, critics point out that such programs may 'crowd out' private insurance and leave overall insurance rates unchanged.

The purpose of this research is to explore the impact that the Title XXI expansions have had on reducing the number of uninsured children. In addition to the overall effect, I examine the differential impact these expansions have had on reducing the number of uninsured children, based on the type of expansion used. I find that, as of December 2000, these expansions had generated a 5.9% reduction in the number of uninsured children. The expansions generated an 8.2% reduction in the number of uninsured children in states using only Medicaid expansions, compared to a 5.0% reduction in states using only state program expansions, and a 7.0% reduction in states using a combination of the two approaches. After accounting for differences in the average percent change in eligibility limits for states using each type of expansion, I find that the response of uninsurance rates to the changes in eligibility limits was greatest in states with only Medicaid expansions, and least in states using only state program expansions.

The larger effects on uninsurance rates resulting from Medicaid expansions compared to state program expansions may be due, in part, to the fact that the Medicaid programs do not typically use cost-sharing measures or waiting periods for enrollment. Cost sharing measures are allowed in the state programs, which may provide a deterrent for enrollment in these programs. State programs can also require that children be uninsured for a specified length of time in order to be eligible for the programs. While the use of these waiting periods should not affect the eligibility of uninsured children, they may result in a temporary increase in the number of uninsured children if parents are willing to drop private coverage in order to later enroll their children in the state programs.

The reduction in the number of uninsured children, both overall and for each type of expansion, is statistically significant. The differences in the effects of different types of expansions are not statistically significant. However, based on specification tests of the model, there is evidence that an increase in eligibility limits through existing

Medicaid programs has had a greater impact than expansions through separate state programs.

Looking at the effects of Title XXI expansions on children from different socioeconomic backgrounds, Medicaid expansions had the largest impact on children with family income below the poverty level, while state program expansions had the largest impact on children with family income of 100-150% FPL. There was also a significant impact on children with family incomes of up to 200% FPL for both types of expansions. Medicaid expansions also had a weak impact on children with family income of 200-250% FPL, and no significant effect for children above 250% FPL. State program expansions had no significant effect for children above 200% FPL.

This dissertation is organized as follows. Chapter 2 provides a brief history of Medicaid eligibility for children, as well as a detailed description of the Medicaid and state program expansions and their expected effects on the likelihood of being uninsured. Chapter 3 provides a review of the literature on previous expansions to Medicaid. Chapter 4 describes the data used in this study. Chapter 5 presents the statistical methods used in evaluating the effects of Title XXI expansions. Chapters 6 and 7 summarize the results of the empirical estimation of the effects of Title XXI expansions on children's uninsurance rates. In chapter 6, the model is used to estimate the average effect across all children. In chapter 7, the model is expanded to allow for different effects for children of different socioeconomic status. Chapter 8 estimates the effect of the Title XXI expansions on the likelihood of having an uninsured parent. This is included to test that the model is identifying effects unique to children, since the Title XXI expansions only apply to children's eligibility for public health insurance programs. Finally, chapter 9 summarizes the results and provides a discussion for how these results can be used by policy makers.

CHAPTER 2

OVERVIEW OF MEDICAID AND TITLE XXI EXPANSIONS

2.1 Background on Medicaid Eligibility

Medicaid was initially designed to provide medical coverage to low-income individuals who met specific categorical eligibility requirements. The three main categories of Medicaid eligibility included AFDC recipients, the disabled, and the elderly. As a result, low-income children were usually only eligible for Medicaid if their families received AFDC payments. Children in families not receiving AFDC payments were only eligible if they met one of the other categorical requirements (for example, a disabled child in a low-income family). Because of the link between Medicaid and AFDC eligibility, children in two-parent households were not categorically eligible for Medicaid, since they were not eligible for AFDC payments.

In some states, children who did not qualify for Medicaid under AFDC eligibility rules could still qualify for Medicaid if the state had a Medically Needy or Ribicoff program. Medically Needy programs expanded Medicaid eligibility to some children in families whose incomes exceeded the AFDC eligibility standards, but who otherwise would have qualified for AFDC. These children could become eligible for Medicaid if they had incurred large medical expenditures which brought their remaining income (income net of medical expenditures) within the AFDC income-eligibility requirements. The Ribicoff programs extended Medicaid eligibility to children in two-parent families who met the AFDC income-eligibility requirements (Currie and Gruber, 1996).

Under the Deficit Reduction Act of 1984, states were required to extend Medicaid eligibility to all children who were born after September 30, 1983, and who met the AFDC income-eligibility requirements (Currie and Gruber, 1996). This effectively phased-in Ribicoff programs for all states and was the first federally mandated Medicaid expansion for children beyond those participating in AFDC. Following this legislation, many states also implemented Ribicoff programs for children born prior to September 30, 1983, though they were not required to do so.

From 1986 through1990, several acts increased Medicaid income-eligibility standards for children. These included: the Omnibus Budget Reconciliation Acts of 1986, 1987, 1989 and 1990; the Medicare Catastrophic Coverage Act of 1988; and the Family Support Act of 1988. The end result of these acts was that states were required to extend Medicaid eligibility to all children age 6 or under with family incomes up to 133 percent of the federal poverty level and all children born after September 30, 1983, with family incomes under the federal poverty level. For children born prior to September 30, 1983, states were required to extend Medicaid eligibility to all children whose family income met the AFDC income-eligibility standards. States had the option to expand Medicaid eligibility to infants (less than one-year old) in families with income up to 185 percent of the federal poverty level (Currie and Gruber, 1996).

2.2 Medicaid Services

All Medicaid programs must include a set of services mandated by the federal government. There are also a number of optional services which states can choose to cover under Medicaid. As a result, the scope of Medicaid services varies by state. Services which must be covered under a state's Medicaid Program include:

- Inpatient hospital services.
- Outpatient hospital services.
- Prenatal care.
- Vaccines for children.
- Physician Services.
- Nursing facility services for persons aged 21 and older.
- Family planning services and supplies.
- Rural health clinic services.
- Home health care for persons eligible for skilled-nursing services.
- Laboratory and x-ray services.
- Pediatric and family nurse practitioner services.
- Nurse-midwife services.
- Federally qualified health-center (FQHC) services, and ambulatory services of an FQHC that would be available in other settings.
- Early and periodic screening, diagnostic, and treatment (EPSDT) services for children under age 21 (HCFA, "Medicaid: A Brief Overview").

In addition, there are currently 34 optional services which may be included in a state's Medicaid program. The most common of these include:

- Diagnostic Services.
- Clinic Services.
- Intermediate care facilities for the mentally retarded (ICFs/MR).
- Prescribed drugs and prosthetic devices.
- Optometrist services and eyeglasses.
- Nursing facility services for children under age 21.
- Transportation services.
- Rehabilitation and physical therapy services.
- Home and community-based care to certain persons with chronic impairments (HCFA, "Medicaid: A Brief Overview").

2.3 Payment for Medicaid Services

States are responsible for making payments to health care providers under their Medicaid programs. Traditionally, these payments were made on a fee-for-service basis, although many states have implemented prepayment arrangements, for example, through health maintenance organizations (HMO's). States are generally not allowed to use any cost-sharing measures, such as deductibles, coinsurance rates, or copayments, in their

Medicaid programs for children, although they may use nominal cost-sharing measures for other populations (HCFA, "Medicaid: A Brief Overview").

The federal government reimburses states for a percentage of their Medicaid expenditures, known as the Federal Medical Assistance Percentage (FMAP). The FMAP's are determined annually, based on a formula comparing a state's average per capita income level with the national average per capita income level. Based on this formula, states with higher per capita income receive a lower FMAP than states with lower per capita income. The FMAP's are required by law to be no less than 50% and no more than 83%.

2.4 Title XXI of the Balanced Budget Act of 1997

Starting in October of 1997, Title XXI allows states to receive enhanced federal matching funds for programs to reduce the number of low-income uninsured children. The enhanced FMAP for children covered under Title XXI is more generous than the FMAP for Medicaid, so that the state's share of medical expenditures under Title XXI is 70% of their share under the regular Medicaid FMAP. The total federal expenditure made to a state in a given year is capped at a maximum absolute federal expenditure. For a state using a Medicaid expansion under Title XXI, once it has reached the cap for the enhanced FMAP, it may receive the regular Medicaid FMAP for any additional expenditures stemming from those made eligible for Medicaid through the Title XXI expansion. For states using a new state program, they may not receive any additional federal funds for that program, once they have reached the cap for enhanced FMAP (CMS, 1997).

If a state chooses to expand Medicaid eligibility, it must continue at least the same level of coverage currently existing, with no new cost sharing or restrictions on services. If a state develops a new state program, it has more flexibility to create a new benefit package and to include cost-sharing provisions, such as premiums or copayments, though cost-sharing is not to exceed 5% of a family's annual income. For states implementing a new state program, the benefit package, at a minimum, must be equivalent to (or actuarially equivalent to) one of the following benchmark coverages (CMS, 1997):

1) the Blue Cross/Blue Shield Preferred Provider option offered under the Federal Employees Health Benefits Program
2) a health benefits plan that is offered and generally is available to state employees
3) the HMO plan with the largest commercial enrollment in the state.

State programs may also include eligibility rules to discourage crowding out of private insurance, such as enforcing a waiting period for children with private coverage. For example, states can require that, in order to be eligible for the state program, a child must not have had private coverage in the past year.

States must also ensure that children eligible for Medicaid who apply for either Medicaid or a new state program are enrolled in Medicaid, not the state program. The result is that most states have streamlined their application process. For example, states with separate state programs have often developed a joint application for both Medicaid and the new state programs. This may reduce barriers to enrollment in Medicaid.

2.5 Overview of Title XXI Expansions:

As of March 31, 1997, the federally mandated eligibility thresholds for Medicaid were 133% of the Federal Poverty Line (FPL) for infants and children less than 6 years old, and 100% FPL for children 6 years old and older born after September 30, 1983. At that time, there were 33 states with higher thresholds for infants, 10 states with higher thresholds for children ages 1-5[1], 10 states with higher thresholds for children ages 6-14[1], and 6 states with thresholds greater than 100% for children ages 15-18. The thresholds were 200% FPL or greater in 6 states for infants, in 5 states for children ages 1-5, in 4 states for children ages 6-14, and in 3 states for children ages 15-18 (Health Care Financing Administration, 2000).

Eight states began enrolling children in Title XXI expansion programs in 1997, 32 states in 1998, 8 states in 1999, and 2 states in 2000 (Mathematica Policy Research, Inc., 2001).

As of March 31, 2000, there were 18 approved Medicaid expansion programs, 15 approved state programs, and 17 approved combination programs among U.S. states. At that time, there were 28 states with thresholds of 200% FPL or greater for all children; 7 of these states had implemented expansions solely through Medicaid, 10 had implemented expansions solely through state programs, and 11 had implemented combination expansions. (Mathematica Policy Research, Inc., 2001).

For states using combination expansions, some had expanded Medicaid only to accelerate the phase-in of Medicaid for older children (those born prior to September 30, 1983). For these states, regardless of any expansions resulting from Title XXI, the Medicaid expansions required for older children would have been fully phased in for all children ages 17 and under by 2000 (the last year included in this research). For the purposes of this research, accelerated Medicaid eligibility for older children is ignored in describing the type of expansion used, since these expansions would have taken place by 2000 anyway. Thus, states that use Medicaid expansions only to accelerate eligibility for older children, and use state programs to further increase eligibility under Title XXI, are treated as states using only state program expansions. States described as using combination expansions are those that increased Medicaid eligibility beyond the requirements in 2000 as well as increased eligibility through state programs.

Overall, eligibility limits from public insurance programs increased from an average of 128.96% FPL in 1996 to 205.75 in 2000. These averages are computed as the average eligibility limit for each age, 0-17, within each state. The tables in Appendix A provide a more detailed description of eligibility thresholds in place prior to the Title XXI expansions, and the eligibility thresholds in effect as of December 31, 2000.

2.6 Expected Effects of Title XXI Expansions

About 1 million children were enrolled in Title XXI expansion programs in 1998, increasing to about 2 million in 1999 (Mathematica Policy Institute, 2001). However, the effect of these programs on reducing the number of uninsured children is still not known due to the possibility of crowding out of private insurance. Additionally, it is uncertain whether the best approach is through Medicaid expansions, state program expansions, or a combination of the two.

Expanded eligibility for children through public insurance such as Medicaid or the new state programs would be expected to increase the number of children enrolled in

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[1] Eight of these states had higher thresholds for both the 1-5 and 6-14 age groups.

these plans. However, expanded eligibility for public health insurance may also reduce the number of children enrolled in private health insurance plans, an effect referred to as crowding out.

Crowding out can occur for a variety of reasons. Broadly defined, crowding out refers to those who do not hold private health insurance, but who otherwise would have in the absence of expanded eligibility for public health insurance. While many of these individuals enroll in the pubic health insurance programs, others may become uninsured. The term direct crowding out is used to describe those who enroll in public health insurance programs, but who otherwise would have held private health insurance. The term indirect crowing out refers to those who become uninsured, but who otherwise would have held private health insurance.

Direct crowding out occurs when newly-eligible children are dropped from private coverage and enrolled in a public health insurance program. Additionally, direct crowding out could result if a previously uninsured child is enrolled in a public health insurance program, when they otherwise would have been enrolled under a private policy.

Indirect crowding out may also occur if Medicaid expansions result in non-eligible children being dropped from private coverage and becoming uninsured. For example, many states have traditionally had different eligibility thresholds for children of different ages. Thus, Medicaid expansions may result in some children in a family becoming eligible for Medicaid, while other children in the same family do not. This may give families a financial incentive to change employment-related policies from family to individual coverage. The eligible children may then be enrolled in Medicaid (direct crowding out), leaving the non-eligible children uninsured (indirect crowding out).

Additionally, firms may react to expanded Medicaid eligibility by increasing the employee share of premiums or, in a more extreme case, by discontinuing individual and/or family health insurance from their benefit package completely. This, again, may result in direct crowding out for those eligible for Medicaid and indirect crowding out for those that become uninsured.

Previous studies have focussed on the effect of earlier Medicaid expansions and the resulting magnitude of crowding out of private insurance. However, expansions through Medicaid may not have the same effects as expansions through the new state programs, for a number of reasons.

First, promotion of a new state program might lead to higher enrollment for both the state program and Medicaid, due to increased awareness of the availability of public insurance. While promotion of Medicaid expansions could have a similar effect, we might expect this effect to be stronger in states using state programs if there is less of a welfare stigma associated with the state program, compared to Medicaid. These differences may be small since most states had already renamed their Medicaid programs prior to 1997 in an effort to reduce the welfare stigma associated with Medicaid.

Second, the state programs generally include cost-sharing measures, such as premiums, copayments, or deductibles. These cost-sharing measures are not typically allowed under Medicaid without a special waiver from the Health Care Financing Administration. As a result, a state program expansion would likely lead to smaller increases in enrollment, compared to an equivalent Medicaid expansion.

A third difference between state program expansions and Medicaid expansions is that the state programs often have provisions to discourage crowding out. Many states require a waiting period for children who previously had private insurance, before they can become eligible for the state program. The goal of these provisions is to limit participation in the state program to those children who would not otherwise have access to affordable insurance. If successful, these provisions would result in smaller increases in enrollment for a state expansion, compared to an equivalent Medicaid expansion, but only for those children who would likely have had private insurance coverage in the absence of eligibility expansions. However, these crowd-out provisions might have some temporary adverse affects as well, if parents are willing to drop private coverage for their children in order to obtain public coverage some time in the future. Overall, these provisions would be expected to reduce the degree of crowding out, if not perfectly, as long as some parents would not risk dropping their child's health insurance for a period of time.

Because of these differences between Medicaid and new state programs, the net effect on enrollment is ambiguous. In addition, there are likely differences in the degree of crowding out which results from either Medicaid or state program expansions. The combined effect on the reduction of uninsured children is, thus, also ambiguous.

Prior to Title XXI, previous expansions of federal/state funded public insurance always took place through Medicaid. It is expected that increased generosity would result in an increase in Medicaid enrollment. However, these increases may have attracted both those who were previously uninsured and those who would have private insurance, in the absence of Medicaid expansions. Crowding out is usually defined as the percent of the increase in Medicaid enrollment which can be attributed to decreases in private insurance coverage, resulting from an increase in Medicaid eligibility. If crowding out occurs at a rate of 25%, then the number of children who otherwise would have had private insurance, in the absence of Medicaid expansions, account for one-fourth of those enrolled in Medicaid as a result of the expansions. In other words, the net reduction in the number of uninsured children is only three-fourths of the increased enrollment in Medicaid, resulting from expanded Medicaid eligibility. The result of crowding out is that the government insures more children than simply the number of children who otherwise would have been uninsured. Thus, the greater the extent of crowding out, the larger the publicly-financed cost of the expansions, in terms of the average public cost per child who otherwise would have been uninsured.

Under Title XXI, states may choose to expand coverage through Medicaid expansions, through state program expansions, or through a combination of the two. As a result, there may be differences across states in the success of the expansion on the intended goal of reducing the number of uninsured children, as well as differences in the extent that crowding out occurs. The intent of this research is to provide a first step in understanding the effects of the Title XXI expansions, by estimating the impact these expansions have had on reducing the number of uninsured children.

CHAPTER 3

PREVIOUS STUDIES

3.1 Relationship Between Health Insurance and Health Outcomes:

Much of the previous literature has focussed on whether Medicaid expansions have increased insurance coverage. While increased insurance coverage is expected to improve access to health care, it does not necessarily imply better health. Marquis and Long (1996) have studied health-care service utilization, based on source of insurance. They found few differences in utilization of health-care services, comparing Medicaid recipients to the privately insured. However, they found that the uninsured had much lower levels of utilization compared to those with Medicaid or private insurance.

Currie and Gruber (1996) have examined the effects of Medicaid expansions on health outcomes for newborns. Using data from 1979 to 1992, they found that Medicaid expansions not only increased Medicaid participation for pregnant women, but that the Medicaid expansions had a significant impact on reducing the incidence of infant mortality and low birth weight. Thus, there is evidence that increasing health-insurance coverage improves access to care, and may improve health outcomes as well.

3.2 Previous Studies on Medicaid Expansions and Crowding Out:

Previous studies of the effects of Medicaid expansions on insurance coverage have focused heavily on the issue of crowding out, the extent to which increases in public insurance are offset by reductions in private coverage. While I do not estimate crowding out in this dissertation, I review the literature in some detail, because it is relevant to how I identify policy-induced changes in uninsurance rates.

If we only look at changes in insurance source following an increase in Medicaid eligibility thresholds, we ignore the fact that other factors may have contributed to these changes regardless of the changes in Medicaid eligibility. For example, the 1991 recession would have resulted in increased Medicaid eligibility and decreases in employment-related private coverage, regardless of any changes in Medicaid policy which took place during that time. In order to control for these other factors, a control group is used that is not affected by the Medicaid expansions, but is otherwise similar to those who are affected. The success in this approach depends on the suitability of the control group. The main differences in previous studies of Medicaid expansions result from both differences in the control group used and from differences in the type of data used. Many of the previous studies use cross-sectional data, such as the March CPS. The main advantages of using cross-sectional data, and the March CPS in particular, are that they have large sample sizes and are available fairly quickly. Other studies have used panel data, which provide more detailed information on how individuals' behavior changes over time, though such data are not publicly available as quickly as cross-sectional data sources.

Because of the time lag in the availability of panel data, only cross-sectional data are currently available for the study of recent changes in eligibility for public insurance. Cutler and Gruber (1996), Dubay and Kenney (1997), and Rask and Rask (2000) offer three different approaches to measuring the impact of previous Medicaid expansions using cross-sectional data. Their models and results are discussed in detail, since these are most relevant to the model presented in Chapter 5. There is also a brief summary of results for Shore-Sheppard, Buchmueller, and Jensen (2000) who examine whether firms' decisions to provide health insurance benefits are affected by increased generosity of public insurance programs. Finally, a brief summary is provided for studies by Thorpe and Florence (1998), Yazici and Kaestner (2000), and Blumberg, Dubay and Norton (2000) which use panel data to assess the effects of previous Medicaid expansions.

Cutler and Gruber (1996) provided the first study of the Medicaid expansions which took place in the late 1980's and early 1990's. Their study examined how changes in individual eligibility for Medicaid affect the likelihood of holding private insurance, public insurance, or of being uninsured. They use data on women of childbearing age and children from the March CPS, collected in 1988-1993. Cutler and Gruber use state-level eligibility variation to identify the effect of gaining eligibility on the choice of insurance (Medicaid, private insurance, or uninsured). The variation in state-level eligibility arises due to variation in eligibility thresholds over time as well as from variation between states, both in the timing and size of the expansions, and from variation within states for children of different ages. In essence, the control group consists of those children not eligible for Medicaid. Because of the multiple sources of variation in eligibility limits, the group of children not eligible for Medicaid includes children with otherwise similar characteristics to the group of children eligible for Medicaid.

Cutler and Gruber use the linear probability model to estimate the effect of changes in Medicaid eligibility on each source of insurance (Medicaid, private coverage, or uninsured) for women of child-bearing age and for children. The regression for each source of insurance is performed separately. Their regressions include demographic controls for race, sex (for children), marital status (for women), type of family(male/female head, male only, female only), number of persons and number of workers in the household, as well as state, age, and year dummy variables. They acknowledge that using eligibility itself as the "treatment" variable may create biased estimates because of: 1) omitted individual characteristics which might be correlated with Medicaid eligibility; 2) endogeneity bias resulting from the fact that jobs with employer-sponsored private insurance coverage may offer lower wages, which possibly affects Medicaid eligibility; and 3) measurement error in determining eligibility. In order to remove these sources of bias, they create an instrument for Medicaid eligibility using a national sample of women and children. This sample is used to compute the average percent of children of each age who are eligible for Medicaid in each state for each year. This instrument is correlated with the state policies in each year, but is not otherwise correlated with the individual demand for insurance. Indicator variables for each age, state and year are also included in the regressions. The treatment effect is thus identified only by differential *changes* in eligibility across ages, states, and years.

While this technique does provide a correction for the problems listed above, it does not address the question of whether the state policy changes, themselves, are endogenous. For example, differences in the size of eligibility expansions across states may be made in response to state-level differences in the insurance rates or differences in the trends of insurance rates for children. Cutler and Gruber recognize this possible source of bias, but point out that such bias is unlikely to exist since 90% of those made eligible through the expansions gain eligibility under federal mandates, rather than state choices to extend eligibility beyond the national mandates.

Cutler and Gruber estimate the rate of crowding out by taking the ratio of the coefficient on eligibility from the regression on private insurance coverage divided by the coefficient on eligibility from the regression on Medicaid coverage. In other words, this is the marginal change in private coverage divided by the marginal change in Medicaid coverage, given a gain in eligibility for Medicaid. They find a crowding out rate of 31% for children and more than 100% for women.

The above estimates are all based on the changes in insurance coverage, given a change in individual eligibility for Medicaid. Cutler and Gruber expand their basic model to account for the fact that expanded eligibility for some family members may cause other, non-eligible, family members to lose private coverage. As a greater percentage of the household becomes eligible for Medicaid, it may be more likely that private insurance is dropped not only for those eligible for Medicaid, but also for other family members who may become uninsured. For example, for a family with three children where the younger two are eligible for Medicaid, the primary wage earner may switch from family to single coverage, leaving the oldest child uninsured. These spillover effects are an important source of potential crowding out and should not be ignored. Because Cutler and Gruber model a change in eligibility status for some members of households, they need to model the effects of changes in eligibility for any member of the household on the choice of insurance for all members of the household.

One approach for accounting for these spillover effects onto other family members would be to include one variable indicating if an individual was eligible for Medicaid, as before, and a second variable measuring the percent of the health insurance unit which is eligible for Medicaid. The health insurance unit is defined as the head of the family, spouse, and children under age 19 (or children under age 23 who are full-time students). The expected results are: 1) a gain in individual eligibility would lead to a higher likelihood of enrolling in Medicaid, and a lower likelihood of holding private insurance; and 2) an increase in the percent of the health insurance unit eligible for Medicaid would lead to a lower likelihood of holding private insurance, even for those not eligible for Medicaid, themselves. The problem with this approach is that families may not value health insurance equally for all members of the health insurance unit. For example, parents may have a stronger preference toward having health insurance for their children, than for themselves. Additionally, parents may have a stronger preference for insuring younger children, compared to older children who may have less need for routine medical care, such as immunizations. Unfortunately, it is impossible to infer the relative value placed on holding insurance for each family member with the available data. As a proxy for the actual value, Cutler and Gruber use the expected annual health spending for each age, based on the 1987 Nation Medical Expenditure Survey.

Rather than simply using the Medicaid eligibility variables, one for individual eligibility and one for the percent of the health insurance unit eligible, Cutler and Gruber convert these to the percent of expected annual health expenditures for the health insurance unit which could be covered through: 1) individual Medicaid eligibility, and 2) Medicaid eligibility for all other members of the health insurance unit.

Using this specification, Cutler and Gruber estimate the rate of crowding out to be 40% for children and more than 100% for women of child-bearing age. This analysis provides greater rates of crowding out since it accounts for parents and children who become uninsured, rather than gaining public insurance, but who otherwise would have been covered under private policies.

Other researchers have directly used comparison groups to measure the magnitude of crowding out, based on cross-sectional data. Dubay and Kenney (Health Affairs, 1997), for example, use a comparison group of men ages 18-44 to measure the magnitude of crowding out for women ages 18-44. Their data are based on the 1989 and 1993 March CPS. In order to estimate the rate of crowding out, they first calculate the percentage point changes in employer-sponsored coverage and Medicaid coverage for both women and men, ages18-44. They then take the differences in the percentage point changes for women compared to men, using a difference-in-differences approach. Finally, they compute the rate of crowding out as the difference in percentage point change in employer-sponsored coverage divided by the difference in percentage point change in Medicaid coverage. Since adult men were generally not affected by changes in Medicaid policy, this suggests that they would make a suitable control for how women's participation in public or private health insurance would have changed in the absence of Medicaid expansions. One potential difficulty with this approach is that women typically become eligible for Medicaid only when they become pregnant. Pregnancy, itself, is likely to affect women's demand for health insurance coverage in ways that are not comparable to men.

Dubay and Kenney estimate an overall rate of crowding out for women of childbearing age to be 45%. They also separate their population into further income groupings and find no evidence of crowding out for women with incomes <100% of poverty, crowding out of 29% for women with incomes 100-133% of poverty, and 59% for women with incomes 134-185% of poverty. Thus, they conclude that crowding out becomes more likely as the eligibility rates expand further. Finally, they construct a weighted average of these crowding out rates, based on the percent of women in the sample that become eligible within each income group, to conclude that the total rate of crowding out is only 14%.

Rask and Rask (2000) study the crowding out effect of not only Medicaid expansions, but also the effect on private insurance coverage resulting from changes in subsidies to health care providers, either through public hospitals or through uncompensated care reimbursement funds. They find that all three forms of public insurance acted as substitutes for private insurance, and that more substitution resulted from provider subsidies than from Medicaid expansions. This is likely due to the fact that Medicaid expansions only extend health insurance to specific income groups, while provider subsidies potentially affect all demographic groups.

Rask and Rask's analysis of changes in Medicaid eligibility is based on cross-sectional data from the 1989 and 1992 National Health Interview Surveys. They identify whether individuals are eligible for Medicaid under AFDC eligibility, Medically Needy eligibility, or AFDC-UP eligibility (which extends Medicaid to two-parent families who otherwise meet the AFDC eligibility standards). Their dataset is stratified by poverty-level groupings (<100% FPL, 100-200% FPL, 200-400% FPL, and >400% FPL). The effects of differences in Medicaid eligibility are estimated for the first three groups using a mulitnomial logit regression, with possible outcomes defined as: private insurance, Medicaid, or no insurance. The effects for the highest income group are estimated using a binomial probit model with possible outcomes of private insurance or no insurance (since Medicaid is unlikely to be available to this group). These estimates are made separately for 1989 and for 1992. Comparing the results from the two years, they conclude that increases in Medicaid generosity result in increased Medicaid coverage, along with decreased private insurance coverage (crowding out) and decreases in the uninsured. For the below-poverty group, increased likelihood of having Medicaid was offset mostly by a decreased likelihood of being uninsured. For the 100-200% FPL group, increased Medicaid coverage was offset by both decreased private coverage and decreases in the uninsured.

Rask and Rask's model does control for many demographic characteristics, including race, marital status, income, education, family size, and broadly defined age category indicators. However, the fact that the two years are estimated separately indicates that the results are mainly observational. There is no attempt to account for other changes over time which may affect insurance choice.

Shore-Sheppard, Buchmueller, and Jensen (2000) use firm-level data from various surveys taken between 1989 and 1995 to assess employers' responses to Medicaid expansions. They find that firms employing large fractions of low-wage workers were significantly less likely to offer insurance, but that firms' decisions to offer insurance was unaffected by the percentage of workers eligible for Medicaid. They did find that firms with a higher percentage of workers eligible for Medicaid were significantly less likely to offer family coverage. They conclude that crowding out occurs mainly through reduced take-up of employer-sponsored coverage, rather than through reductions in the availability of employer-sponsored coverage, particularly for workers who would be required to contribute directly toward premiums.

Many researchers have also used longitudinal data from the SIPP and NLSY to measure both the magnitude of crowding out and the choice of insurance, both before and after a change in eligibility. Thorpe and Florence (1998) use NLSY data from 1989 to 1994 to show that, although approximately one-third of children enrolled in Medicaid had held private coverage the previous year, only 16% of children newly enrolled had access to private insurance through a parent's employment at the time of enrollment. This indicates that Medicaid expansions are less likely to crowd out employment related coverage than non-group coverage. This result would be expected due to the fact that: 1) employment related coverage premiums are typically less than non-group coverage, and 2) employment related coverage premiums are often subsidized by the employer. Thus, households may have a greater incentive to drop non-group coverage, compared to employer-related coverage, in order to enroll in Medicaid. Yazici and Kaestner (2000) use NLSY data from 1988 to 1992 to estimate that crowding out accounted for 18.9% of the increased participation in Medicaid. They compare the insurance coverage for children under age 8 in 1988 who were made eligible through expansions to those who were never eligible. Blumberg, Dubay, and Norton (2000) use data from the 1990 SIPP Panel to examine the change in insurance coverage between 1989 and 1992. They estimate that 23% of the movement from private insurance to Medicaid was attributable to crowding out as a result of the expansions. However, they find no evidence of crowding out among those who were initially uninsured. While using longitudinal data certainly has many advantages in assessing changes in the choice of insurance, there is a much longer lag in the availability of such data, making it currently inaccessible for studying the effects of state programs.

Overall, crowding out is estimated to account for 15-40% of the increased children's enrollment in Medicaid, following the expansions in eligibility which occurred in the late 1980's and early 1990's. In comparing the different approaches used, Cutler and Gruber are the only researchers that have used cross-state variation directly to identify the response to changes in Medicaid eligibility. This distinction does appear to have an effect on the estimates of crowding out, since Cutler and Gruber's estimates are noticeably larger than that of other researchers. Part of this difference is due to the fact that Cutler and Gruber explicitly allow for indirect crowding out which results from a gain in Medicaid eligibility of other family members. However, the estimates from their first model, which does not account for such spillover affects, are still greater than those of other researchers.

The accuracy of any of the difference-in-differences models depends on whether the comparison group (those not affected by the policy change) is otherwise similar to those who are affected by the policy change. In Cutler and Gruber's model, children not eligible for Medicaid (based on their age, state, and the year of the observation), provide a natural control for otherwise similar children who are eligible for Medicaid, due to the multiple sources of variation in Medicaid eligibility. In the other difference-in-differences models, the downfall is that there may be important differences between the comparison groups, aside from the change in Medicaid eligibility. For example, women of child-bearing age would likely have a stronger preference toward holding health insurance (especially those who are pregnant), compared to single men of the same age group. As a result, women of child-bearing age may not have been as likely to drop private health insurance coverage, in response to the 1991 recession, compared to single men. Thus, single men may not provide an equivalent comparison group for women of child-bearing age. As another example, families may have a stronger preference toward insuring young children (who may require more frequent medical attention), compared to older children. Again, families may have been less likely to drop private coverage for young children, in response to the 1991 recession, compared to older children. Thus, older children, who were less likely to be affected by previous Medicaid eligibility expansions, may not provide an equivalent comparison group for younger children. In my opinion, it is the lack of an equivalent control group which has generated lower estimates of crowding-out, compared to Cutler and Gruber.

All of the previous studies looked specifically at the extent of crowding out of private insurance, rather than the net change in the number of uninsured children. However, the calculations for crowding out can be related directly to the effect on uninsurance rates. For example, Cutler and Gruber provide the highest estimate of crowding out as 40%, meaning that 40% of those enrolled in Medicaid, in response to eligibility expansions, can be attributed to reductions in private insurance coverage. In terms of the effect on the number of uninsured, this implies that 60% of those enrolled in Medicaid, in response to eligibility expansions, can be attributed to reductions in the number of uninsured children.

CHAPTER 4

DATA

Data on the Medicaid and state program eligibility limits, and the effective dates of these changes, is gathered from the following sources: a report issued by Mathematica Policy Research, Inc. (2001); state reports made to the Health Care Financing Administration (HCFA); and from the State Children's Health Insurance Program (SCHIP) Database, available from the U.S. Department of Health and Human Services, Office of the Assistant Secretary for Planning and Evaluation (1999).

Data from the March CPS Supplements taken in years 1997-2001 (representing data from 1996-2000) are used to determine whether children have insurance, as well as for household demographic data. The March CPS is the most widely used data set for estimates of the number of uninsured. It is also the only data set currently available with data recent enough to study the Title XXI expansions. In addition to household demographic questions, the survey asks respondents about the type of insurance coverage held by all members of the household during the previous calendar year. Thus, including data beginning with the March 1997 CPS will allow for one full year of data prior to the implementation of Title XXI expansions.

March CPS survey asks respondents who, if anyone, in the household received Medicaid coverage (using state-specific names as well as Medicaid) during the previous year. Similar questions are asked about employer- or union-sponsored plans, non-group plans, Medicare, Military and VA health plans. The survey later asks whether anyone in the household was covered by any other type of health insurance plan (including state-specific state program names) not already talked about. These questions make it possible to determine whether each member of the household has health insurance, or if they are uninsured. Coverage under state programs is reported as "other government health care" which may include other state plans with limited coverage; the March 2001 CPS includes new questions relating directly to state programs.

One potential problem with the March CPS data is that they typically under-report Medicaid participation, compared to administrative data from the Health Care Financing Administration (HCFA). In 1995, HCFA reported that 36.7 million non-elderly individuals were enrolled in Medicaid at some point during the year, compared to only 29 million, based on March CPS data (Fronstin, 2000a). Part of this under-reporting may be due to the fact that many states have renamed their Medicaid programs. The March CPS uses the state-specific names for Medicaid in their interviews to minimize this sort of confusion. However, as Fronstin (2000a) points out, the list used during the March 1999 CPS did not contain Maryland's Medicaid program, which had been renamed HealthChoice, resulting in a substantial under-reporting of the number of Maryland Medicaid participants. Measuring Medicaid and state program participation accurately has likely become even more difficult now that more states have renamed Medicaid and many have added state programs (some of which have the same name as the Medicaid program, such as Indiana's "Hoosier Healthwise"). Accurately reporting an individual's coverage as either public or private may have become more difficult as well, since many states are using capitated payments to private insurance providers, making the public programs appear more like private plans to the recipients. Granted, this difficulty in assessing the correct source of health insurance coverage is likely not unique to the March CPS, since other surveys are facing the same confusions.

In order to minimize the effects of inaccurate reporting of type of insurance coverage, my model is based on whether an individual is uninsured. This will avoid bias due to individuals reporting private insurance when they really have Medicaid, for example.

Estimates of the number of Americans who are continuously uninsured over the course of a year, using the March CPS, are also typically higher than estimates from other surveys. Swartz (1986) has shown that March CPS estimates are much more similar to other surveys' point-in-time estimates of the number of uninsured. Thus, Swartz suggests that respondents to the March CPS may be reporting their current source of health insurance, rather than their source(s) of health insurance for the previous year.

In 1995, the March CPS survey was expanded to include questions about the current source of insurance, as well as questions pertaining to the previous year. Bennefield (1996) has shown that both the point-in-time estimates and previous-year estimates of the uninsured, using the March CPS, were greater than estimates from the Survey of Income and Program Participation. In addition, the March CPS point-in-time estimate was considerably greater than the previous year estimate. Thus, while the March CPS tends to over-report the number of uninsured, compared to other surveys, it does not appear that respondents are merely reporting their current source of health insurance (Bennefield, 1996).

One other difference between the March CPS and other surveys is that individuals are identified as being uninsured if they do not respond affirmatively to any of the health insurance questions. That is, the March CPS never asked if an individual is uninsured, prior to 2000. Beginning with the March 2000 CPS, a verification question was added for individuals who did not respond affirmatively for any source of health insurance. For the March 2000 CPS, there are duplicate sets of responses for each source of insurance: one set is based on the original responses to the questions, where the second set includes updated information obtained through the verification question. The inclusion of the verification question resulted in a reduction in the measured percent of uninsured children from 13.9% to 12.6% (Nelson and Mills, 2001).

While the inclusion of the verification question improves the accuracy of the estimates of the number of uninsured, it is impossible to infer how this would have affected responses for years prior to 2000. In order to avoid bias in the effects of the Title XXI expansions, resulting from changes in the survey questions, I ignore the updated responses due to the verification question in determining health insurance status[1]. Failure to report insurance status consistently would create an artificial decrease in the number of uninsured children for years including the verification question, since part of the decrease is due only to the change in the survey question.

For the March 2000 CPS, this is easily accomplished because of the duplicate sets of variables for sources of health insurance. For the March 2001 CPS, these variables are defined only after accounting for responses to the verification question.

The verification question is only asked of those individuals who did not previously report any source of insurance. As a result, those who respond affirmatively to the verification question are identified as insured, based on the verification question, but would have been identified as uninsured, in the absence of the verification question, as in previous survey years. Those who respond negatively to the verification question are identified as uninsured regardless of whether the verification question is asked; those who are not asked the verification question are identified as insured regardless of whether the verification question is asked.

In order to provide a consistent measurement of insurance status for data in the March 2001 CPS, compared to earlier survey years, I designate anyone who responded affirmatively to the verification question as being uninsured. This approach was tested using the duplicate responses in the March 2000 CPS. The set of individuals who respond affirmatively to the verification question is the same as the set who are identified as insured, based on the post-verification variables, and as uninsured, based on the pre-verification variables.

The sample used from the March CPS, reflecting years 1996-2000[2], includes all never-married children ages 0-17 from civilian households. I have excluded children who are identified as the head of a household, family, or subfamily; and children identified as primary individuals. Heads of households are treated as adults in the CPS, making it impossible to determine some variables, such as the number of parents present. Children identified as primary individuals (meaning that they are unrelated to other members of the household) are automatically recorded as having no income, making it impossible to describe their income and poverty-level group accurately.

Table 1 shows the percent of uninsured children, ages 0-17, in years 1996-2000 and various subgroups. All of the percentages in Table 1 are based on insurance status, assuming that the verification question had not been asked, following the procedure outlined above. The first column shows the percent of uninsured children for each year. The second column shows the percent of uninsured children from civilian households, for

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[1] This adjustment did not noticeably change any of the results reported.
[2] These data are from the March CPS surveys collected in 1997-2001.

each year. The third and fourth columns show the percent of uninsured civilian children based on the observations included and excluded, respectively, in the dataset. Finally the fifth column shows the percent of uninsured civilian adults, ages 21-64.

Table 1: Uninsurance Rates, 1996-2000.

Year	All Children Ages 0-17	Civilian Children	Included Children	Excluded Children	Civilian Adults Ages 21-65
1996	14.82%	15.04%	14.60%	41.56%	18.71%
1997	15.00%	15.20%	14.71%	43.63%	19.60%
1998	15.37%	15.57%	14.96%	48.17%	19.80%
1999	13.86%	14.03%	13.52%	42.37%	19.61%
2000	12.93%	13.10%	12.64%	35.53%	19.50%

Source: Author's calculations, based on March CPS surveys from 1997-2001.

The children excluded from the dataset are much more likely to be uninsured than those included. However, their exclusion does not dramatically alter the percent of uninsured children and does not change the year-to-year trends. Additionally, the changes in uninsurance rates over time are very similar for both the included and excluded children.

Pooling all years together, the omissions account for less than 1.8% of the total number of observations, representing less than 1.8% of the total population of children. In any given survey year, the omissions are always less than 1.9% of either the total number of observations or the represented population. Thus, it is unlikely that the omitted data have a significant impact on the findings of this research.

Finally, the last column of Table 1 shows the percent of uninsured, civilian adults for years 1996-2000. This is presented as a comparison to the trends observed in the children's data. For both children and adults, the percent of uninsured increases from 1996-1998, and then decreases from 1998-2000. Thus, this downward change in the trend for children is not necessarily related to the increased availability of public insurance, since the Title XXI expansions would not be expected to have the same effect on adults. Part of the downward trend for both children and adults is likely attributable to the economic expansion over this period. However, the larger decline for children's uninsurance rates may indicate an additional effect from the Title XXI expansions.

CHAPTER 5

STATISTICAL METHODS

5.1 Model Design

Given that the primary objective of the Title XXI expansions is to decrease the number of uninsured children, I develop a model that will assess which type of Title XXI expansion has the largest impact in reducing the percentage of uninsured children.

The approach used is most similar to that of Cutler and Gruber (1996). However, Cutler and Gruber analyze the effect of changes in eligibility (instrumented by state-level variation in eligibility) on changes in insurance source. Eligibility for state programs may depend on a child's current or recent enrollment in private insurance policies, if waiting periods for eligibility are used. Since this cannot be observed with March CPS data, inference about actual eligibility is not possible. Instead of trying to infer eligibility from the data, the model described below looks at changes in the likelihood of being uninsured, given changes in the state eligibility limits. Thus, the model estimates the average response across all children, both eligible and non-eligible, with the expectation that larger changes in eligibility will have a stronger effect on uninsurance rates. Since Cutler and Gruber use variation in state-level eligibility to instrument for estimated eligibility, the identification strategy is very similar. The use of state-level policy changes, rather than actual changes in eligibility, has also been used by Yelowitz (1995) to estimate changes in women's labor supply and welfare participation in response to expanded Medicaid eligibility, relative to AFDC eligibility.

Additionally, where Cutler and Gruber analyze changes in insurance source, this model looks only at the likelihood of being uninsured versus uninsured. This is because of difficulties in determining insurance source, as discussed in chapter 4.

One additional difference between this model and that of Cutler and Gruber is that I do not control for changes in insurance status resulting from expanded eligibility for siblings. This model only estimates the change in insurance status in response to changes in the individual child's eligibility for public insurance. Under previous Medicaid expansions, the result was that young children were more likely to gain eligibility, compared to older siblings. The Title XXI expansions had the effect of equalizing eligibility limits across all ages, though in some states, it is possible that a younger child is eligible for Medicaid, while the older siblings are eligible for the state program, if one exists.

The fact that there is a larger change in eligibility limits for older children may not have any effect on younger siblings, though. For example, suppose a family has previously dropped private coverage, insured their younger child in Medicaid, leaving an older sibling (who is not eligible for Medicaid), uninsured. If the older child gains eligibility through Title XXI expansions, that child may be then be enrolled in Medicaid, or the state program, while the younger child remains enrolled in Medicaid. In this case, there is no change in the insurance status of the younger child, and thus, no spillover effect from the older sibling gaining eligibility.

Another scenario is that the same family had chosen to continue private coverage, even though the youngest child is eligible for Medicaid. If the older sibling now gains eligibility to a public program, both children may be enrolled in Medicaid and/or the state program. However, since this model does not distinguish between public or private insurance, the children would be designated as insured both before and after the change. While there is a spillover effect onto the younger child's source of insurance, the fact that the child is still insured is reflected in this model.

Data on children from the March CPS Supplement representing years 1996-2000 is used to estimate the following probit[1] regression:

$$prob(U=1)=\Phi(\alpha+\beta_1 X+\beta_2 MLim+\beta_3 SDif+\beta_4 State+\beta_5 Year) \quad (1)$$

In the above equation, Φ represents the standard normal cumulative distribution function. 'X' is a vector of socioeconomic controls including dummy variables for the age, race, and gender of the child, as well as dummy variables representing the type of family (both parents present, mother only, father only, or neither parent present). 'X' also includes family earnings (divided by 10,000), family non-earned income (divided by 10,000), the number of persons in the family, as well as the squares of each of these variables. Finally, 'X' includes dummy variables for poverty level groupings, based on this ratio(<50%FPL, 50-100%FPL, 100-150%FPL, 150-200%FPL, 200-250%FPL, 250-300%FPL, 300-350%FPL, and >350%FPL). Together, this will account for any changes in the likelihood of being uninsured which are attributable to changes in the characteristics of families in the United States. Thus, to the extent that the number of children being covered by employer-sponsored coverage increases over this period, this may in part be explained by changes in the proportion of families in each income grouping. The inclusion of state and year dummy variables will account for differences in the likelihood of being uninsured due to economy-wide effects specific to particular

[1] For a thorough description of the probit model, see Wooldridge, 2000, pages 530-540.

years and due to state-level differences which are unrelated to the Title XXI expansions. However, state variables do not control for any state-level differences which *are* related to the Title XXI expansions. For example, differences in states' policy changes may have been made in response to differences in previous uninsurance rates or trends. This possibility of policy endogeneity is discussed further in Section 5.2.

The goal is to establish the effect of the Title XXI expansions, after controlling for changes in the socioeconomic characteristics of households, as well as fixed-effect time trends and state-level differences. A simpler specification of this model would be to include a set of dummy variables measuring if there was: 1) an expansion in Medicaid eligibility, based on the child's age, state, and year; 2) a new state program in place for that state; or 3) a combination of both expanded Medicaid eligibility and a new state program. However, this approach is only appropriate if we expect the impact on the likelihood of a child being uninsured to depend only on the type of expansion used, and not on the magnitude of the expansion itself. In reality, there is considerable variation across states in the increased generosity resulting from the expansions. In addition, there is variation across states in the timing of the expansions. Finally, there is variation within states in terms of both initial Medicaid generosity and the size of the expansion for children of different ages. By using the upper eligibility thresholds, defined as a percentage of the FPL and based on the age of the child, the state, and the year of the observation, we can account for changes in eligibility thresholds based on all sources of variation.

In the probit equation (1), the variable 'MLim' is defined as the upper threshold for Medicaid eligibility (expressed as a decimal), based on the child's age, state, and year, and the Medicaid policies in effect in the year of the observation. In states that do not increase their Medicaid eligibility limits, 'MLim' remains at the same level existing in the base year, 1996. In states that do increase their Medicaid eligibility limits, 'MLim' will identify not only that a change in eligibility took place, but also the magnitude of the change. In a year in which an eligibility expansion takes place, 'MLim' is defined as the average eligibility limit over the year. In other words, 'MLim' is increased by the increase in eligibility times the share of the year that it was in effect. This adjustment is done because increases in eligibility which occur late in the year would not be expected to have the same effect as increases in eligibility which occur earlier in the year.

The variable 'SDif' is defined as the difference between the eligibility threshold of the new state program and Medicaid (expressed as a decimal), based on the child's age and state, and the year of the observation. For states without a new separate state program, this would be defined as zero; for states that do have a separate state program, 'SDif' would identify not only the presence of a separate plan, but also the increased generosity of the plan, relative to the state's Medicaid program. New York and Pennsylvania offered state-wide comprehensive coverage for children under state-sponsored plans prior to Title XXI legislation[1]. These state programs will be treated identically to any new state programs. In these cases, the value of 'SDif' will reflect the eligibility limits of the existing state plan, minus those of Medicaid, based on the child's age and state, even in the base year observations for 1996. Failure to include these plans would overstate the true expansion in the availability of public insurance prior to Title XXI, and may lead to biased estimates. Again, in order to adjust for differences in the

[1] Florida also had a comprehensive state plan prior to Title XXI, though it was not yet available state-wide.

timing of the expansions, 'SDif' is calculated as the average increase in eligibility limits through state programs over the year.

A simpler specification would be to replace 'MLim' and 'SDif' with a single variable which measured the maximum eligibility threshold of either a new state program or the state's Medicaid program (for states that do not have a separate state program in that year). This approach would assume that expanded eligibility through a new state program has the same effect as an equivalent expansion in Medicaid eligibility.

For example, suppose there are two states in which the Medicaid eligibility threshold before the expansions is 100% FPL. State A expands its Medicaid eligibility threshold to 200% FPL, while state B leaves its Medicaid program unchanged and creates a new state program which covers children with family incomes between 100% and 200% FPL. In the year in which the expansions took place, my model would define 'MLim' as 200 for state A and 100 for state B; 'SDif' would equal 0 for state A and 100 for state B. Under the simpler specification, there would be only one variable equal to 200 for both states. This alternate specification is identical to my specification only when β_2 is assumed to be equal to β_3, in equation (1). That is, the alternative specification would look at the average effect of any type of expansion, whereas my specification allows for different effects of Medicaid expansions and state program expansions. Likelihood ratio tests are performed in order to determine if there truly is a difference in the effects of increased Medicaid eligibility, compared to increased eligibility through state programs.

5.2 Possible Sources of Endogeneity

One potential problem with the model described is the possibility of endogenous regressors. In the Probit estimation model, the key relationship addressed is the effect of changes in eligibility limits, through Medicaid or a separate state insurance program, on the uninsurance rates for children. However, these policy changes may have been made in response to the states' previous experience with uninsurance rates. If this is the case, then the policy changes, themselves, are endogenous leading to biased estimates of the changes in uninsurance rates. Cutler and Gruber discuss this possibility, but do not correct for it, since most of the changes in eligibility are in response to federal mandates, not to state options. However, the expansions made under Title XXI are not due to federal mandates. As a result, analysis of Title XXI expansions may be more susceptible to policy endogeneity than previous expansions to Medicaid.

In order to test if there is evidence of such endogeneity, it would be necessary to find an instrument which is partially correlated with the state policy changes, after controlling for all exogenous regressors, but which does not, itself, influence the probability of being uninsured. In the absence of such an instrument, I rely on a more observational approach. While this is not a strictly valid test for endogeneity, it does provide some insight into whether the policy changes are likely to have been chosen in response to states' previous experience with children's uninsurance rates.

I use two measures of states' previous experience with children's uninsurance rates. Using data from 1994 to 1996, I estimate each state's average uninsurance rate over the three-year period, as well as the change in uninsurance rates from 1994 to 1996.

The policy variables measure the change in eligibility limits through either Medicaid or through a separate state program. Combination states are identified only by the fact that they have changes in both types of eligibility limits. Further, many of the states that only used SCHIP expansions did have some increases in the Medicaid eligibility limits, due to the phasing in of coverage for older children.

In order to assess if policy endogeneity is likely to exist, I test whether there is a significant correlation between either the prior average uninsurance rates or the prior uninsurance trends and the changes in eligibility through Medicaid and through separate state programs. The eligibility changes are defined as the average eligibility change, across all ages, for each state based on the policies in effect in 2000 (the last year in the sample). These correlations, along with the p-values measuring significance, are listed in table 2.

Table 2: Correlations Between Prior Uninsurance Rates/Trends and Total Eligibility Changes

	Average Uninsurance Rate	Uninsurance Trends
Medicaid Change	0.0613	-0.0633
	(0.67)	(0.66)
State Program Change	0.0428	0.1359
	(0.77)	(0.35)
Total Change	0.1036	0.0925
	(0.47)	(0.52)

p-values measuring significance shown in parentheses.

Based on these correlations, there is no evidence that states' prior experience with uninsured children is related to the size of the expansions, either through Medicaid, separate state programs, or the combined change in eligibility limits. However, some of the changes in Medicaid would have taken place without any expansions through Title XXI, due to the phasing in of eligibility limits for older children. Additionally, some of the expansions through separate state programs were in effect prior to the Title XXI legislation. Table 3 summarizes the correlations if only the expansions related to Title XXI legislation are considered.

Table 3: Correlations Between Prior Uninsurance Rates/Trends and Title XXI Eligibility Changes

	Average Uninsurance Rate	Uninsurance Trends
Medicaid Change	-0.0257	-0.0724
	(0.86)	(0.62)
SCHIP Change	0.0668	0.1959
	(0.65)	(0.17)
Total Change	0.0500	0.1494
	(0.73)	(0.30)

p-values measuring significance shown in parentheses.

Again, there is no statistical evidence that states' past experience with uninsured children is related to the Title XXI eligibility expansions. As noted earlier, this is not the ideal measure of whether the policy variables are, in fact, endogenous. However, it does provide some evidence that differences in state policy changes are not a result of differences in the states' previous uninsurance rates or trends.

Another possible source of endogeneity exists if changes in eligibility limits for public health insurance affect not only the insurance status of children, but also their observed demographic characteristics. While this would obviously not affect the child's characteristics such as gender, age, or race, it is possible that there is an effect on the family income. For children who gain access to public insurance, this may affect the labor-force participation or work effort of their parents. Thus, changes in eligibility for public health insurance may affect both family income and a child's health insurance status simultaneously.

Testing for such endogeneity requires an instrument which is partially correlated with the measures of family income, after controlling for all other regressors, but not otherwise correlated with the children's likelihood of being uninsured. In the absence of such an instrument, another correction to remove bias from the coefficients on the policy variables is to use an instrument partially correlated with the policy variables, after controlling for all other regressors, but not correlated with family income. This would still potentially result in biased estimates for other coefficients, though these are not of primary interest in interpreting the results. Since the policy variables are set at the state level, the state indicators included in the probit equation will control for any differences in the demographic make-up of each state, such as differences in the average family income across states. To the extent that the state indicators are not correlated with individual characteristics, their inclusion should help to control for the possible endogeneity of any of the individual demographic characteristics, including family income. However, state indicators are not sufficient if state-level differences in the change in eligibility limits generate state-level differences in the change in average family income.

Cutler and Gruber control for the possibility of endogenous demographic variables using their instrument based on state-level variation in eligibility. Thus, the technique used to remove bias in their study is essentially the same as that used here.

5.3 Simulated Changes in Uninsurance Rates Resulting From Title XXI Expansions

Once the probit model from equation (1) has been estimated, the results can be used to simulate the change in the probability of being uninsured, resulting from the Title XXI expansions. This is done by obtaining the coefficients from equation (1), based on the entire sample of children, including sample weights to ensure a nationally representative sample. The change in the probability of being uninsured for each individual in the 2000 sample can then be calculated as:

$$prob(U = 1 \mid 2000 policy) - prob(U = 1 \mid 1996 policy) \qquad (2)$$

The first term is found by using the estimated coefficients from equation (1) to calculate the estimated probability of being uninsured, based on the demographic characteristics and the Medicaid and state program eligibility rules in effect for each child in the 2000 sample. The second term is found by repeating this estimation, but substituting the values of 'Mlim' and 'SDif' that would have applied to each child, based on the child's age and state and the policy rules present in 1996[1]. This results in the estimated probability of being uninsured, based on the demographic characteristics and the Medicaid and state program eligibility rules in place in the base year, 1996, for each child in the 2000 sample. In other words, this is an estimate of the probability of being uninsured that would have existed in the absence of Title XXI expansions. The average difference between these two estimates is then the estimated change in the probability of being uninsured due to the changes in eligibility rules and introduction of new state programs, holding all other variables constant.

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[1] Because Medicaid eligibility would have been phased in for many children born after 10/1/1983, I do not treat these increases in eligibility as part of the relevant change in eligibility limits. However, I do not distinguish between Title XXI expansions in Medicaid eligibility limits and other expansions in Medicaid

After finding the average effect of the Title XXI expansions across all states, this process is then repeated to find the average effect of the Title XXI expansions, based on the type of expansion used. This is accomplished by using the estimation procedure above to calculate the average effect of the expansions across: 1) children in states with only Medicaid expansions, 2) children in states with only state program expansions, and 3) children in states with combination expansions.

As stated earlier, for states which had not previously increased their Medicaid eligibility limits for older children, beyond the AFDC eligibility limits, Medicaid eligibility would have increased for older children regardless of the Title XXI expansions. In many of these states, these increases in Medicaid eligibility for older children were accelerated in response to Title XXI legislation. By the year 2000, though, eligibility for these children would have been fully phased in under the previous federal requirements. In defining the type of expansion used, the accelerated phase-in of previous federal requirements are not treated as a Medicaid expansions, since they do not affect the eligibility levels observed in 2000, the last year of the sample used.

Standard errors for these estimates are computed as follows. The estimator of interest is:

$$F(\hat{\beta}) = \frac{1}{N}\sum_{i=1}^{n} D_i(\hat{\beta}) \tag{3}$$

where $D_i(\hat{\beta}) = \Phi(X_{1i}\hat{\beta}) - \Phi(X_{2i}\hat{\beta})$

X_{1i} is based on the actual observed values of the regressors for the 2000 sample, while X_{2i} substitutes the policy variables based on the 1996 policy rules. Φ is the

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eligibility which took place after 1996, such as Arkansas' Section 1115 expansion to Medicaid eligibility effective September, 1997.

standard normal cumulative distribution function and ϕ, used below, is the standard normal probability density function. The conditional variance of $F(\hat{\beta})$ arises from the fact that this statistic is based on the estimated probit coefficients. Using the delta method[1], the estimated conditional variance is defined as:

$$Var[F(\hat{\beta})] = CVC' \qquad (4)$$

where $C = \dfrac{\partial F(\hat{\beta})}{\partial(\hat{\beta})'} = \dfrac{1}{N}\sum_{i=1}^{n}[\phi(X_{1i}\hat{\beta})X_{1i} - \phi(X_{2i}\hat{\beta})X_{2i}]$

and $V = Var(\hat{\beta})$, the variance-covariance matrix from the probit regression.

This same procedure is used to find the estimated conditional variance of $F(\hat{\beta})$ for each type of policy change. The estimated unconditional variance of $F(\hat{\beta})$ could also be found by adding the variance of the mean value of the differences in the likelihood of being uninsured, $D_i(\hat{\beta})$, to the estimated conditional variance of $F(\hat{\beta})$. Due to the large sample size, though, this has a negligible effect on the size of the variance.

The comparisons of the simulated effects resulting from each type of expansions are mainly included for descriptive purposes. The resulting differences in the reduction in the number of uninsured children, based on the type of expansion used, can result from two distinct sources. First, they may be due to differences in the estimated effect of increases in eligibility through Medicaid, compared to increased eligibility through state programs. Second, there may also be differences in the average size of the expansions themselves.

[1] See Greene (1993), page 297, for a description of the delta method.

As a descriptive measure, the simulations estimate the actual average change in the probability of being uninsured arising from both sources. However, the results of option, Medicaid versus state program expansions, is more effective in reducing the number of uninsured children. For example, suppose that a one percent FPL increase in Medicaid eligibility limits results in a greater reduction in the probability of being uninsured, compared to a one percent FPL increase in state program eligibility limits. If eligibility limits increase by the same amount in all states, then we would expect the greatest reduction in uninsurance rates in states using only Medicaid expansions and the least change in uninsurance rates in states using only state program expansions. States using combination expansions would fall somewhere in the middle, with the estimated change in uninsurance rates depending on the relative share of the total expansion which takes place through both Medicaid and state program expansions. However, it is possible that states using only state program expansions could achieve similar results in the change in uninsurance rates if the average size of their expansions exceed the average size of the expansions in states using only Medicaid expansions.

5.4 Average Partial Effects of Medicaid and State Program Expansions:

The simulations described in Section 5.3 are based on a two-step process. In the first step, the change in the likelihood of being uninsured is estimated for each child in the 2000 sample, based on the actual eligibility limits in place in 2000, compared to the counterfactual eligibility limits that would have existed in the absence of the Title XXI expansions. Second, the average change in the likelihood of being uninsured is calculated across: 1) all children; and 2) children in states with each type of expansion.

The two sets of eligibility limits used for each child, in the first step, vary for children in different states, and for children of different ages within a particular state. Thus, it would be nearly impossible to objectively adjust these sets of eligibility limits so that the average change in eligibility limits is equal for each type of expansion.

In order to present a more direct comparison between the estimated effects of increased Medicaid eligibility, versus increases in eligibility through state programs, the average partial effect of each type of expansion is calculated[1]. Rather than taking the average of the estimated change in the likelihood of being uninsured for each individual child in the 2000 sample, the average partial effects estimate the change in the likelihood of being uninsured for a representative child with average characteristics. Since this estimate is based on a single observation, derived from the average characteristics of all children in the sample, it is much easier and straight-forward to compare the effects of increases in Medicaid eligibility, versus an equivalent increase in eligibility through a state program.

Average partial effects are usually found by computing the expected change in the probability of being uninsured, given a change in one of the independent variables, based on the estimated probit coefficients evaluated at the mean value of all other regressors. In general, for a discrete change in an independent variable, Z, the average treatment effect is computed as:

$$\Phi(\overline{X}\hat{\beta} + Z_1\hat{\gamma}) - \Phi(\overline{X}\hat{\beta} + Z_2\hat{\gamma}) \qquad (5)$$

where Z_1 and Z_2 are the two discrete values for Z, $\overline{X}$ is the vector of means for all other regressors, and $\hat{\beta}$ and $\hat{\gamma}$ are the estimated coefficients. In applying this procedure to the

[1] See Wooldridge (2002), pages 458-459 for a description of average partial effects.

model at hand, we use the fact that the average eligibility limit in 2000 is 205.75% FPL (including both Medicaid expansions and state program expansions). The average eligibility limit that would have existed in 2000, based only on the required expansions for older children, is 131.62% FPL, a difference of 74.13% FPL. Thus, if the Title XXI expansions had all taken place through Medicaid expansions, the average partial effect can be simulated by using 131.62 and 205.75 for the two discrete values of 'MLim,' defining 'SDif' equal to zero, and by using the mean value of all other independent variables. Similarly, if the Title XXI expansions had all occurred through state program expansions, the average treatment effect can be simulated using 0 and 74.13 for the two discrete values of 'SDif,' defining 'MLim' equal to 131.62, and using the average value of all other regressors.

Standard errors for the average partial effects are again computed using the delta method. The statistic of interest is:

$$F(\hat{\beta}) = \Phi(\overline{X}_1\hat{\beta}) - \Phi(\overline{X}_2\hat{\beta}) \qquad (6)$$

where $\overline{X}_1$ and $\overline{X}_2$ are the vector of means for each of the non-policy variables, and the policy variables are defined as described above. The estimated asymptotic variance is defined as:

$$Var[F(\hat{\beta})] = CVC' \qquad (7)$$

where $C = \dfrac{\partial F(\hat{\beta})}{\partial(\hat{\beta})} = \phi(\overline{X}_1\hat{\beta})\overline{X}_1 - \phi(\overline{X}_2\hat{\beta})\overline{X}_2$, and $V = Var(\hat{\beta})$, the variance-covariance matrix from the probit regression.

This procedure is a variation of the standard transformation used to estimate the marginal effects of a change in an independent variable on the change in the probability of observing a positive outcome in the dependent, binary variable. The probit coefficients in Equation (1) measure the change in the value of the linear projection, Xβ, given a one-unit increase in each independent variable, holding all other variables constant. This is not the same as the change in the probability of being uninsured, Φ(Xβ), given a one-unit increase in each independent variable, which is usually of more interest than the coefficient itself. The change in the probability of being uninsured, given a one-unit increase in each dependent variable can be found as:

$$\frac{d[\Phi(X\beta)]}{dX} = \phi(X\beta)\beta$$

This expression depends not only on the coefficient, but the value of the linear projection, Xβ, which depends on the value of each independent variable. This expression is most often estimated at the mean value of each of the independent variables. Thus, it is interpreted as the average marginal effect of a one-unit increase in an independent variable on the probability of a positive outcome.

This type of transformation is used in the results of Chapters 6 and 7 to show the average marginal effect of a one-unit increase in either Medicaid eligibility limits or state program eligibility limits, on the probability of being uninsured. The only difference in the average partial effects is that it looks at the average partial effect of an average-sized increase in either Medicaid or program eligibility limits on the probability of being uninsured, rather than a one-unit change in eligibility limits.

5.5 Expanding the Model to Allow for Different Effects Across Poverty Level Groups

In Chapter 7, the basic model is expanded to allow for different policy effects for children in families from different socioeconomic backgrounds. Specifically, the policy effects are allowed to vary for children within different poverty-level groups, based on the ratio of family income to poverty level ratio.

Rather than having a single set of policy variables, which capture the average effect across all children, the policy variables are interacted with the poverty level group indicators. This creates a set of policy variables specific to each poverty-level group. In order to determine the optimal specification for the policy variables, interacted with the poverty-level indicators, the following process is used.

First, a set of policy variables is defined for children in families with income less than 50% FPL, and a second set of policy variables is defined for all other children. A likelihood ratio test is performed to determine if the added policy variables generate a significant improvement in the model. Under the null hypothesis, there is no significant difference in the effects of the policy variables on children in families with income less than 50% FPL, compared to all other children. This process is then repeated, adding additional policy variables for each poverty level group, as long as the null hypothesis is rejected from the likelihood ratio test. This provides us with the poverty-level groups for which there was a different effect of the policy variables, compared to wealthier children.

Second, likelihood ratio tests are performed to determine if there is a significantly different effect for Medicaid expansions and state program expansions for each specific poverty-level group identified in the first set of likelihood ratio tests. That is, the first set of likelihood ratio tests identify poverty-level groups for which there is a significantly different effect of the policy variables on the likelihood of being uninsured; the second set of likelihood ratio tests identify if there is any different marginal effect for Medicaid expansions, compared to state program expansions. This is accomplished by combining the 'MLim' and 'SDif' variables for a specific poverty-level group, thus imposing the restriction that either type of expansion has the same effect. A likelihood ratio test is then used to determine if the restriction has a significant effect on the model. Under the null hypothesis, there is no significant difference in the effect of Medicaid expansions and that of state program expansions on the likelihood of being uninsured.

Finally, the policy variables from adjacent poverty-level groupings are combined, where feasible to determine if the effects of the policy variables are significantly different between the adjacent groups. A likelihood ratio test is performed to test the null hypothesis that the policy effects are the same across the two groups. For example, suppose we are testing if the policy changes have a different effects for the 0-50% FPL group and the 50-100% FPL. The unrestricted model would contain separate policy variables for each poverty-level group identified in the first set of likelihood ratio tests. The restricted model would combine the policy variables for all children in families with incomes 0-100% FPL. The likelihood ratio test would compare the log likelihood of the unrestricted and restricted models to determine if the restriction has a significant effect on the model. Such a combination is not performed if the policy variables are defined differently for the two adjacent poverty-level groups. For example, if it is found that there is not a significant difference in the effects of Medicaid and state program expansions for the 0-50% FPL group, but there is a significant difference for the 50-100% FPL group, the policy variables cannot be easily combine for these two groups.

The choice of first testing whether to combine the policy variables within each poverty-level group and then whether to combine the policy variables across two adjacent groups is somewhat arbitrary. If the tests were performed in the opposite order, the results may not be same. For example, it is possible that combining the policy variables, 'MLim' and 'SDif' for the 0-50% FPL and 50-100% FPL groups would not impose a significant restriction on the model. However, in the previous example, this test would not be performed due to the fact that the 'MLim' and 'SDif' variables had already been combined into a single policy variable for the 0-50% FPL group, but not for the 50-100% FPL group. The chosen order to perform these tests, although arbitrary, is based on the fact that this research is primarily interested in whether there is a different effect from Medicaid expansions, compared to state program expansions. Thus, it is reasonable to first test if such a difference exists within each poverty-level group, prior to restricting the policy variables across adjacent poverty-level groups.

Once the probit model has been estimated, the simulated effects of the expansions are found using the same methods described in section 5.3. Now, however, the simulated effects are estimated for: 1) all children combined; 2) children living in states with each type of expansion; 3) children in each poverty-level group; and 4) children in each poverty-level group living in states with each type of expansion. Again, these estimated effects may vary not only because of differences in the estimated coefficients on the policy variables, but also because of different average magnitudes of the expansions, for states with each type of expansion.

Finally, the average partial effects are estimated using the same procedure described in section 5.4. Now, however, the average partial effects are estimated for each poverty-level group separately. The changes in policy variables are identical to those described in section 5.4, but the mean value of all other regressors is identified separately for each poverty-level group. This allows for the estimation of the average partial effects of Medicaid expansions versus state program expansions, based on the average characteristics of a child from a specific poverty-level group, rather than the average characteristics of a child from the entire population.

5.6 Robustness of the Model

In order to test that the model really is identifying the change in the likelihood of being uninsured, I exploit the fact the Title XXI expansions typically apply only to children, and not to their parents. As a result, we would not expect the changes in Medicaid or state program eligibility limits to decrease the parents' likelihood of being uninsured. If anything, the parents would be more likely to be uninsured through indirect crowding out. For example, parents may choose to drop family health insurance coverage if their child (or children) can be enrolled in the public insurance program, leaving non-eligible family members uninsured. In order to test this, the dependent variable identifying whether each child is uninsured is replaced by a variable indicating if they have an uninsured parent. Children not living with their parents are necessarily excluded from the sample. Children whose parents are under the age of 19 are excluded as well, since the Title XXI expansions would apply to these parents, as well as their children. Additionally, children with parents age 65 or older are excluded since their parents would be insured through Medicare.

CHAPTER 6

IMPACT OF TITLE XXI EXPANSIONS ON AVERAGE UNINSURANCE RATES

6.1 Model Specification

In order to find the best specification for the model, it is important to find if there is a significant difference in the effects of increased eligibility through either Medicaid expansions or state program expansions. If there is not, then it is more appropriate to replace the two policy variables with the sum of these variables, representing the maximum eligibility threshold.

Expansions through either existing Medicaid programs or through separate state programs make these programs available to children in families with higher incomes. The children who become eligible due to the expansions may be more likely to have access to private health insurance policies. As stated earlier, Medicaid typically does not allow any cost-sharing measures or waiting periods for eligibility, while these are often included in separate state programs. As a result, we might expect that the marginal effect of Medicaid expansions is different from the marginal effect of expansions through separate state programs.

In order to test if there is any difference in the effect of Medicaid expansions and expansions through separate state programs, likelihood ratio tests are performed. The model is first estimated with separate 'MLim' and SDif' variables, representing the unrestricted model.

The model is then re-estimated imposing the restriction that the coefficients for 'MLim' and 'SDif' are equal. This is accomplished by replacing these two variables with their sum (the total eligibility through either type of expansion). The results are shown in Table 4. Based on the likelihood ratio comparing the unrestricted and restricted models, there is weak evidence ($p<0.10$) for rejecting the hypothesis that expansions of either type have the same marginal effect.

Table 4: Comparison of Probit Model Specifications

	I	II
Policy Variable:	Coefficient (Standard Error)	
MLimit	-0.0856007 (0.026623)	-0.0006765 (0.0003209)
SDif	-0.0454054 (0.021085)	
Log-Likelihood	-63059.641	-63061.465

LR Statistic (one restriction): 3.648*

*Statistically significant at the 90% confidence level

Model I is the unrestricted model
Model II is restricted so that the coeffients on MLim and SDif are equal

6.2 Probit Model Estimation Results

The probit coefficients for selected variables of the estimated model are presented in Table 5. The complete results can be found in Appendix B. For comparison purposes, the coefficients have also been estimated under the linear probability model. These are shown in Appendix C. The interpretation of the linear probability model coefficients are analagous to the estimated marginal effects from the probit estimation.

As discussed in Section 5.4, page 49, the coefficients themselves are difficult to interpret, except for the fact that the sign of the coefficients indicate the direction of the change. The last column in table 5 shows the marginal effects of a change in the

Table 5: Probit Regression Results, Selected Variables

	Coefficient	Standard Error	Mean Value	Marginal Effect at Mean Value
Policy Variables:				
MLim	-0.0856007***	0.026623	1.34	-0.016568
SDif	-0.0454054**	0.021085	0.35	-0.008788
Demographic Variables:				
Female	-0.006155	0.008943	0.49	-0.001191
White	-0.099542**	0.049429	0.79	-0.019934
Black	-0.124164**	0.052383	0.16	-0.022822
Asian	0.034132	0.060298	0.04	0.006731
Both Parents	-0.907113***	0.030535	0.69	-0.213352
Mother Only	-1.008200***	0.031347	0.23	-0.142731
Father Only	-0.689998***	0.037684	0.04	-0.089274
# in Family	-0.130560***	0.022114	4.27	-0.025270
# in Family Squared	0.012723***	0.002142	20.31	0.002463
Family Earnings	-0.0721202***	0.006511	5.05	-0.013959
Family Earnings Squared	0.0015065***	0.000148	54.92	0.000292
Other Income	-0.1761351***	0.015570	0.47	-0.034091
Other Income Squared	0.0082334***	0.001348	1.52	0.001594
0-50% FPL	0.290364***	0.047792	0.08	0.064710
50-100% FPL	0.401608***	0.042003	0.11	0.092904
100-150% FPL	0.480721***	0.037482	0.11	0.114598
150-200% FPL	0.377220***	0.034030	0.11	0.086366
200-250% FPL	0.257503***	0.031648	0.10	0.056157
250-300% FPL	0.077069**	0.029842	0.09	0.015489
300-350% FPL	0.048875	0.030016	0.08	0.009696
Pseudo R-Squared		0.1082		
Log-Likelihood		-63059.641		

*Statistically significant at the 90% confidence level
**Statistically significant at the 95% confidence level
***Statistically significant at the 99% confidence level

Standard errors are robust to heteroskedasticity and correlation between observations within the same household.

Sample Size is 174,004 observations. Estimation performed using sample weights. The complete results for the probit coefficients are shown in Appendix C.

probability of being uninsured, given a change in the independent variable. These values depend on the entire vector of independent variables; they are evaluated at the average value of each variable. The direction of the change and statistical significance is identical to that of the estimated coefficients. However, the magnitude of the marginal effects is easier to interpret since they are expressed as changes in the probability of being uninsured.

The coefficient on 'MLim' is negative and highly significant, meaning that children living in states with greater changes in Medicaid eligibility thresholds were less likely to be uninsured. The coefficient on 'SDif' is negative and significant, meaning that increases in eligibility through separate state programs also result in a decrease in the likelihood of being uninsured, as expected.

The coefficients on the earnings and non-earned income variables indicate that higher income results in a decreased likelihood of being uninsured. The quadratics of these variables indicate that this has a diminishing effect. A similar quadratic effect is observed for the number of people in the family.

The poverty level groupings show that the likelihood of being uninsured increases from the lowest poverty-level group through the 100-150%FPL poverty-level group. This likely reflects the fact that children in the lowest poverty-level groups are more likely to be eligible for Medicaid. For each poverty-level group beyond the 100-150%FPL, the likelihood of being uninsured is decreasing. This likely reflects the fact that children in higher income families are more likely to have access to employer-sponsored coverage and are more likely to be able to afford non-group coverage.

6.3 Simulated Effects of Title XXI Expansions on Uninsurance Rates

In order to assess the impact of these expansions, I use the average predicted probability of being uninsured, for all children in the 2000 sample [P(U|2000 policy)]. I then recalculate the average predicted probability of being uninsured, substituting the values of the policy variables with the 1996 policy variables, based on the child's age and state [P(U|1996 policy)]. This second prediction estimates what the probability of being uninsured would have been in the absence of Medicaid and state program expansions. The results are shown in Table 6.

The column labeled Actual P(U) shows the actual percentage of uninsured children in the 2000 sample. The column labeled P(U|2000) shows the average predicted probability of being uninsured. The column labeled P(U|1996) shows the average predicted probability of being uninsured, based on 1996 policy variables. Thus, the difference between P(U|2000) and P(U|1996), or %pt. ΔP(U), is the percentage-point change in the probability of being uninsured that can be attributed to the Medicaid and state program expansions.

Table 6: Simulated Effects of Medicaid and State Program Expansions

Exp. Type	Actual P(U)	P(U|2000)	P(U|1996)	% pt. ΔP(U)	95% Confidence Interval Lower	Upper
All	12.64%	12.70%	13.49%	-0.80** (0.28497)	-0.24	-1.35
Medicaid Only	10.86%	10.85%	11.82%	-0.97** (0.30348)	-0.37	-1.56
State Only	14.14%	14.50%	15.24%	-0.74* (0.34655)	-0.06	-1.42
Combination	11.37%	10.56%	11.41%	-0.85** (0.30934)	-0.25	-1.46

*Statistically significant at the 95% confidence level
**Statistically significant at the 99% confidence level
Sample size is 33,459 observations.

For all states combined (from the first row of Table 6), this indicates that the percentage of uninsured children is 0.80 percentage points lower due to the Medicaid and state program expansions. This translates to a 5.92% reduction in the number of uninsured children which can be attributed to the expansions[1].

For states with only Medicaid expansions, these expansions can be attributed to a 0.97 percentage point, or 8.19%, reduction in uninsured children. For states with only state program expansions, there was a 0.74 percentage point, or 4.95%, reduction in uninsured children. For states with a combination of Medicaid and state program expansions, there was a 0.85 percentage point, or 6.97%, reduction in uninsured children.

The percentage-point reduction in the uninsurance rates is highly significant for all states combined, as well as for each type of expansion. Thus, we can conclude that all three types of expansion had a significant impact on reducing the number of uninsured children. However, we cannot conclude that any expansion type has a significantly different effect from the others, based on the 95% confidence intervals. The differences are also not statistically different at the 90% confidence level.

Based on the point estimates, it appears that the best results were achieved through only Medicaid expansions and the worst through only state program expansions. However, this result does not take into account the fact that the average change in eligibility limits differs, based on the type of expansion used. Table 7 shows the average eligibility threshold which would have existed in 2000, based on the 1996 policy rules, the actual average eligibility threshold in 2000, and the percent change in these thresholds for each type of expansion. The average eligibility thresholds are calculated using the threshold for a child of each age, 0-17, for each group of states using the same type of expansion.

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[1] The percentage change is calculated by dividing the estimated change in the probability of being uninsured, %pt. $\Delta P(U)$, by the estimated percent of uninsured children in the absence of the expansions,

Table 7: Changes in Eligibility Limits for the 2000 Sample, 1996 Rules versus Actual 2000 Rules

Expansion Type	Elig. Limit (1996 rules)	Actual Elig. Limit	%Change
All	131.62	205.75	56.32%
Medicaid Only	133.97	200.64	49.77%
State Only	129.47	196.60	51.85%
Combination	132.75	206.73	70.79%

The percent changes in the eligibility limits are used to calculate an elasticity of uninsurance rates for children with respect to eligibility limits. This provides a measure which can be more easily compared across expansion types. This elasticity is -0.1646 for children in Medicaid-only states, -0.0954 for children in states using only state program expansions, and -0.0985 in combination states. Thus, the response of uninsurance rates to changes in eligibility limits appears to be greatest in states using Medicaid-only expansions, and similar in states using only state program expansions and states using combination expansions. However, we cannot statistically conclude that there were any differences in these elasticities for any of the expansion types.

6.4 Average Partial Effects of Medicaid and State Program Expansions

As noted in Chapter 5, differences in the simulated effects discussed in section 6.3 may arise from differences in the estimated probit coefficients on the policy variables or differences in the average magnitude of the expansions, based on the type of expansion used. In order to present a more even comparison of the effects of Medicaid expansions versus state program expansions, the average partial effects of each type of expansion are estimated. These estimates are based on the change in the probability of being uninsured,

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Actual P(U) + %pt.ΔP(U).

given the average change in eligibility through either Medicaid or state programs expansions. All other non-policy variables are evaluated at their mean value. The average partial effects are presented in table 8:

Table 8: Average Partial Effects from Medicaid Expansions versus State Program Expansions

Expansion Type	Change in P(U)	Standard Error	95% Confidence Interval Lower	95% Confidence Interval Upper
Medicaid	-1.21**	0.3634	-0.50	-1.92
State Program	-0.65*	0.3026	-0.06	-1.25

*Statistically Significant at the 95% confidence level
**Statistically significant at the 99% confidence level

The average partial effect from either type of expansion is statistically different than zero, meaning that both have a significant impact on uninsurance rates. Based on the point estimates of the change in the probability of being uninsured, the average partial effect of a Medicaid expansion is nearly double the average partial effect of an equivalent state program expansion. However, there is no evidence that the average partial effects from Medicaid expansions are statistically different from those of state program expansions.

The difference in the effect on uninsurance rates, comparing Medicaid expansions and state program expansions, is more pronounced in the average partial effect estimates, compared with the simulations in Section 6.3. This results, in part, from the fact that the same change in eligibility is used for each of the average partial effect estimates. For the estimated simulated effects, recall that the average change in Medicaid eligibility limits was less than the average overall change in eligibility limits. The average change in eligibility limits for states using only state program expansions was similar to the overall average change. Thus, the average partial effect estimates, for Medicaid expansions, are based on a somewhat larger average change in eligibility limits, compared to that used in the simulations.

CHAPTER 7

EFFECTS OF TITLE XXI EXPANSIONS ON DIFFERENT POVERTY-LEVEL GROUPS

7.1 Model Specification

This chapter explores the differences in the effects of Title XXI expansions on children in different poverty-level groupings, based on the ratio of family income to the federal poverty level.

A priori, we would expect that children in wealthier families are not affected by Title XXI expansions, since they are likely not eligible for public insurance programs at all. In addition, we would also expect that there may be little effect on children in the lowest poverty-level groups since they are likely eligible for Medicaid, regardless of the Title XXI expansions. However, there may be an effect on these children if the Title XXI expansions increase awareness of public insurance programs for those who were previously eligible for Medicaid, but had not enrolled. In addition, there may be a reduction in the perceived stigma of public insurance programs resulting from Title XXI expansions. This effect may be larger for states using state program expansions, as opposed to Medicaid, though most states have also renamed their Medicaid programs in an effort to reduce the perceived stigma. The reduced stigma may increase enrollment even for those previously eligible for Medicaid, since most states that have state programs use a common application for both Medicaid and the state program. The greatest effect on uninsurance rates would be expected for children with family income 100-200% FPL, since these are the most likely to gain eligibility for public insurance as a result of Title XXI expansions. In addition, there may be an effect on children with family income above 200% FPL due to the fact that some states have expanded eligibility beyond this level.

Beginning with the model used in Chapter 6, the policy variables are defined separately for children with family income at or below 50% FPL and those with income above 50% FPL. A likelihood ratio test is used where the unrestricted model contains the policy variables defined separately for children in the lowest poverty-level group, and the restricted model has one set of policy variables applying to all children. Under the null hypothesis, the restriction does not significantly affect the estimation, meaning that there is not a different effect on very poor children, compared to children in higher income families.

This process is then repeated by adding an additional set of policy variables for children with family income 50-100% FPL, 100-150% FPL, 150-200% FPL, etc. Additional policy variables are added as long as the null hypothesis is rejected under the likelihood ratio test. The results of these tests are shown in Table 9.

Table 9: Likelihood Ratio Tests for Adding Policy/Poverty-Level Group Interactions

	Model Description	Log-Likelihood	Model Comparison	Restrictions	LR Statistic
I	No Interactions	-63059.641			
II	Add Interactions for <50% FPL	-63051.351	I vs. II	2	16.58*
III	Add Interactions for 50-100% FPL	-63016.757	II vs. III	2	69.188*
IV	Add Interactions for 100-150%FPL	-62975.627	III vs. IV	2	82.26*
V	Add Interactions for 150-200%FPL	-62945.124	IV vs. V	2	61.006*
VI	Add Interactions for 200-250%FPL	-62936.465	V vs. VI	2	17.318*
VII	Add Interactions for 250-300%FPL	-62934.910	VI vs. VII	2	3.11

*Statistically Significant at the 99% confidence level

Based on these results, there is a significant improvement in the model for each additional set of policy variables up to 250% FPL. Comparing model VI and model VII, however, we find that there is not a significantly different effect of the policy variables for children with family income 200-250%FPL, compared to children with family income above 250% FPL.

Table 10: Probit Regression Results for Policy Variables with Policy/Poverty-Level Interactions

	Coefficient	Std. Error	mean value	Marginal Effect at mean value
Policy Variables:				
MLim: 0-50% FPL	-0.17837***	0.04689	0.1017	-0.03452
MLim: 50-100% FPL	-0.21901***	0.04858	0.1394	-0.04238
MLim: 100-150% FPL	-0.17790***	0.04430	0.1447	-0.03443
MLim: 150-200% FPL	-0.15211***	0.05080	0.1430	-0.02944
MLim: 200-250% FPL	-0.07334*	0.04115	0.1325	-0.01419
MLim: >250% FPL	0.02237	0.03027	0.6744	0.00433
SDif: 0-50% FPL	-0.11104**	0.05203	0.0244	-0.02149
SDif: 50-100% FPL	-0.13708***	0.04067	0.0378	-0.02653
SDif: 100-150% FPL	-0.15422***	0.03784	0.0392	-0.02984
SDif: 150-200% FPL	-0.07604**	0.03865	0.0357	-0.01471
SDif: 200-250% FPL	0.04512	0.04459	0.0337	0.00873
SDif: >250% FPL	0.04022	0.02587	0.1820	0.00778
Pseudo R-Squared		0.11		
Log-Likelihood		-62936.465		
Actual Percent Uninsured		14.08%		
Predicted % Uninsured, at Mean Values		11.45%		

*Statistically significant at the 90% confidence level
**Statistically significant at the 95% confidence level
***Statistically significant at the 99% confidence level

Standard errors are robust to heteroskedasticity and correlation between observations within the same household.
Sample Size is 174,004 observations. Estimation performed using sample weights. The complete results for the probit coefficients are shown in Appendix B.

Table 10 presents the estimated coefficients for the policy variables, defined separately for each of the poverty-level groups identified as having a significantly different response: 0-50% FPL, 50-100% FPL, 100-150% FPL, 150-200% FPL, 200-250% FPL, and greater than 250% FPL.

Based on the coefficients in Table 10, Medicaid expansions have a significant effect in reducing the likelihood of being uninsured, for children in all poverty-level groups up to 250% FPL. The magnitude of the coefficients increases from the 0-50% FPL group to the 50-100% FPL group, and declines thereafter. The lesser response of the 0-50% FPL group is likely due to the fact that these children were most likely to be eligible, regardless of the Title XXI expansions. In spite of this, there was a significant effect for the 0-50% group, which is likely attributable to increased awareness of public insurance programs, as a result of Title XXI expansions, rather than expanded eligibility.

Again, based on the coefficients in Table 10, the state program expansion have a significant effect in reducing the likelihood of being uninsured for all children in poverty-level groups up to 200% FPL. The magnitude of the coefficients increases from the 0-50% FPL group through the 100-150% FPL group, and declines thereafter. Again, the significant response of the lowest poverty-level group is likely attributable to increased awareness of public insurance programs, as a result of Title XXI expansions, rather than expanded eligibility.

The likelihood ratio tests shown in Table 9 determine if there is a different effect for each additional set of policy variables, compared to children with higher family income relative to poverty. However, this does not test 1) whether there is any difference in the marginal effect of Medicaid expansions, compared to state program expansions, or 2) whether there is a significantly different effect between adjacent poverty-level groups.

Likelihood ratio tests are performed to test if there is any statistical difference in the marginal effects of Medicaid expansions, compared to state program expansions for each of the poverty-level groups identified as having a significantly different effect in Table 9. The results are shown in table 11.

Under the null hypothesis, the effects of Medicaid expansions are not statistically different from the effects of state program expansions. Based on the likelihood ratio tests in Table 11, there is evidence that the effects of Medicaid expansions are different from state program expansions for all children with family income 0-100% FPL and 150-250% FPL. However, there is no evidence of a different effect for children with family income 100-150% FPL and above 250% FPL. The fact that Medicaid and state program expansions have similar effects for children with family income above 250% is not surprising, given that the neither coefficients on the policy variables for this group are significant.

Table 11: Likelihood Ratio Tests for Different Effects from Medicaid versus State Program Expansions

	Model Description	Log-Likelihood	Model Comparison	Restrictions	LR Statistic
I	Unrestricted Model	-62936.465			
II	MLim=SDif for 0-50%FPL	-62937.967	I vs. II	1	3.004*
III	MLim=SDif for 50-100% FPL	-62939.595	I vs. III	1	6.260**
IV	MLim=SDif for 100-150% FPL	-62936.739	I vs. IV	1	0.548
V	MLim=SDif for 150-200%FPL	-62939.074	I vs. V	1	5.218**
VI	MLim=SDif for 200-250%FPL	-62942.190	I vs. VI	1	11.450***
VII	MLim=SDif for >250%FPL	-62936.730	I vs. VII	1	0.530
VIII	MLim=SDif for 100-150% FPL MLim =SDif for >250% FPL	-62936.841	I vs. VIII	1	0.752

*Statistically significant at the 90% confidence level
**Statistically significant at the 95% confidence level
***Statistically significant at the 99% confidence level

For children with family income below the poverty level, there is a significantly different effect of Medicaid expansions, compared to state program expansions, on the likelihood of being uninsured. The coefficients in Table 10 indicate that the Medicaid expansions have had a stronger effect for these groups, compared to state program expansions. This is somewhat surprising given the fact that most of these children would have gained eligibility for Medicaid, regardless of the Title XXI expansions. The stronger effect of Medicaid expansions on children with family income below the poverty level is explained by the fact that Medicaid eligibility was still being phased in for older children in many states during the study period. As noted earlier, much of the response to increased eligibility, for children with family income below the poverty level, was likely due to increased awareness of public insurance programs. While promotion of a new state program might be expected to generate greater awareness, similar promotion likely took place in states that only expanded Medicaid eligibility. States are required to enroll Medicaid-eligible children in their Medicaid program, regardless of the presence of a state program. Families whose children are eligible for Medicaid, though, may be less responsive to promotion of a state program, compared to promotion of a Medicaid expansion, if they perceive that cost-sharing measures in the state program will apply to them. This may also explain the stronger effect of Medicaid expansions, compared to state program expansions, on the likelihood of being uninsured for children with family income below the poverty level.

In order to test if the policy variables have a significantly different effect for adjacent poverty-level groups, a final set of likelihood ratio tests is performed. Since this builds off the previous restrictions, the unrestricted model does impose the restrictions that the coefficients on 'MLim' and 'SDif' are equal for the 100-150% FPL group and the greater-than 250% FPL group. Because of these previous restrictions, the policy variables cannot be combined between these groups and the adjacent poverty-level groups. Thus, the two restrictions to be tested is whether the policy variables have a different effect for the 0-50% FPL group, compared to the 50-100% FPL group, and for the 150-200% FPL group, compared to the 200-250% FPL group. The results are shown in Table 12.

Table 12: Likelihood Ratio Tests to Combine Policy Variables for Adjacent Poverty-Level Groups

	Model Description	Log-Likelihood	Model Comparison	Restrictions	LR Statistic
I	Unrestricted Model	-62936.465			
II	Same Policy Effects for 0-100%FPL	-62937.812	I vs. II	2	1.942
III	Same Policy Effects for 150-250%FPL	-62945.383	I vs. III	2	17.084*

*Statistically significant at the 99% confidence level

Based on the likelihood ratio tests in Table 12, there is no statistical evidence that the effect of the policy variables are different for the 0-50% FPL group, compared to the 50-100% FPL group (comparing model I to model II). However, there is evidence that the effect is uniquely different for both the 150-200% FPL group and for the 200-250% FPL group, since the null hypothesis is rejected when comparing Models I and III.

Based on the results of all of the likelihood ratio tests, the model is defined as follows for all remaining estimates in this chapter. Policy variables are defined for each of the following poverty-level groups: 0-100% FPL, 100-150% FPL, 150-200% FPL, 200-250% FPL, and greater than 250% FPL. Further the policy variables are restricted so that the coefficient for 'MLim' is equal to the coefficient for 'SDif,' for the 100-150% FPL and greater than 250% FPL groups.

As noted in Chapter 5, the final choice of restrictions on the model depends on the order the restrictions are imposed. If the order is reversed, so that we first test for different policy effects between adjacent poverty-level groups, and then for differences between Medicaid and state program expansions within each poverty-level group, the results do, in fact, differ. In this case the model would have been defined so that there is a separate set of policy variables for children with family income of 0-150% FPL, 150-200% FPL, 200-250% FPL, and greater than 250% FPL. The only restriction that the coefficients on 'MLim' and 'SDif' are equal would be made for those with family income greater than 250% FPL.

Again, the chosen order seems more reasonable since one of the central questions of this research is whether there is a different effect on the likelihood of being uninsured resulting from Medicaid expansions, compared to state program expansions.

Additionally, the results achieved under this approach more closely represent the results achieved without imposing any of the restrictions, shown in Table 10. Namely, as seen in the next section, the magnitude of the coefficient on Medicaid expansions is greatest for the 0-100% FPL group and declining for children in higher poverty-level groups; the magnitude of the coefficients on state program expansions are greatest for the 100-150% FPL group, and declining for children in higher poverty-level groups. This is consistent with the results in Table 10, where no additional restrictions are imposed. If the order of restrictions were reversed, this difference in where the magnitude of the coefficients peak is lost, since all children with family income 0-150% FPL would be combined into the same group.

7.2 Probit Model Results

Results from the Probit estimation are presented in Table 13. The complete results can be found in Appendix D. Again, for comparison purposes, the coefficients have also been estimated under the linear probability model. These are shown in Appendix E.

The policy variables are significant for family incomes up to 200% FPL, regardless of the type of expansion used, with stronger point estimates for the Medicaid expansions. Medicaid expansions also have a weakly significant effect for children with family incomes of 200-250% FPL. Based on the likelihood ratio tests in section 7.1, there is a significant difference in the effects of Medicaid expansions, compared to state program expansions, within each poverty-level group up to 250% FPL. The coefficient on the policy variable for children with family income above 250% FPL is not statistically significant.

The fact that there is no significant effect for wealthier children supports the expectation that the Title XXI expansions should not have had an effect on children in higher-income families, who are not likely to be eligible for any public health insurance. However, the strong effect on the lowest poverty-level group is somewhat unexpected. The effect of the Title XXI expansions on uninsurance rates for children is largest for the poorest children and is not statistically different for children with family income of 0-50%FPL, compared to the 50-100% FPL and 100-150% FPL. The poorest children were most likely to be eligible for Medicaid, regardless of the Title XXI expansions. In fact,

Table 13: Probit Regression Results with Restricted Policy/Poverty-Level Interactions, Selected Variables

	Coefficient	Std. Error	mean value	Marginal Effect at mean value
Policy Variables:				
MLim: 0-100% FPL	-0.1939005***	0.035728	0.2411	-0.0375246
SDif: 0-100% FPL	-0.1309103***	0.033286	0.0622	-0.0253344
Limit: 100-150% FPL	-0.1642206***	0.030784	0.1839	-0.0317808
MLim: 150-200% FPL	-0.1430038***	0.049810	0.1430	-0.0276748
SDif: 150-200% FPL	-0.0797135**	0.038106	0.0357	-0.0154266
Mlim: 200-250% FPL	-0.0644463*	0.038016	0.1325	-0.0124720
SDif: 200-250% FPL	0.0413475	0.044168	0.0337	0.0080018
Limit: >250% FPL	0.0333071	0.020904	0.8565	0.0064458
Demographic Variables:				
Female	-0.0064609	0.008950	0.4866	-0.0012502
White	-0.1034218**	0.049572	0.7866	-0.0207356
Black	-0.1300948**	0.052516	0.1578	-0.0238497
Asian	0.0275784	0.060413	0.0430	0.0054183
Both Parents	-0.9061552***	0.030585	0.6913	-0.2130654
Mother Only	-1.007975***	0.031424	0.2342	-0.1426861
Father Only	-0.6877146***	0.037696	0.0443	-0.0890846
# in Family	-0.1300754***	0.022216	4.2725	-0.0251729
# in Family Squared	0.0126613***	0.002156	20.3136	0.0024503
Family Earnings	-0.0728045***	0.006480	5.0459	-0.0140895
Family Earnings Squared	0.001517***	0.000147	54.9178	0.0002936
Other Income	-0.1770445***	0.015564	0.4679	-0.0342626
Other Income Squared	0.008316***	0.001347	1.5208	0.0016109
0-50% FPL	0.6409523***	0.068885	0.0763	0.1653409
50-100% FPL	0.7530474***	0.063871	0.1062	0.1983624
100-150% FPL	0.8072689***	0.061755	0.1096	0.2158829
150-200% FPL	0.6472169***	0.072865	0.1070	0.1643618
200-250% FPL	0.3842641***	0.061267	0.0994	0.0884903
250-300% FPL	0.0788431***	0.029726	0.0893	0.0158574
300-350% FPL	0.0530599*	0.029930	0.0772	0.0105471

Pseudo R-Squared	0.11
Log-Likelihood	-62937.812
Actual Percent Uninsured	14.08%
Predicted % Uninsured, at Mean Values	11.45%

*Statistically significant at the 90% confidence level
**Statistically significant at the 95% confidence level
***Statistically significant at the 99% confidence level

Standard errors are robust to heteroskedasticity and correlation between observations within the same household.
Sample Size is 174,004 observations. Estimation performed using sample weights.

by 2000, all children with family income below 100% FPL are eligible for Medicaid, regardless of their age, state, or the presence of a state program. Thus, it appears that part of the success in reducing children's uninsurance rates was through increased enrollment for those already eligible for Medicaid. This likely is in response to increased awareness of public insurance programs as well as a reduction in the stigma associated with Medicaid.

7.3 Simulated Effects of Title XXI Expansions on Uninsurance Rates

Table 14 shows the estimated average changes in uninsurance rates for each of the following groups: 1) all children combined; 2) children in states with each type of expansion; 3) children in each poverty level group; and 4) children in each poverty level group for each type of expansion. This is based on the same calculation used in section 6.3, except that the policy variables are allowed to vary for different poverty-level groups, as defined in Section 7.2. For all children combined, there was an estimated 0.90 percentage point, or 6.62%, decrease in the number of uninsured children, as a result of the Title XXI expansions. This is slightly above the estimated 0.80 percentage point, or 5.92%, decrease found in section 6.3, though this difference is not significant based on the 95% confidence intervals for the two estimates.

For states using only Medicaid expansions, there was an estimated 1.00 percentage point, or 8.45%, decrease in the number of uninsured children. Again, this is very similar to the estimated 0.97 percentage point, or 8.19% decrease found in section 6.3. For states using only state program expansions, there was an estimated 0.93 percentage point, or 6.17%, decrease in the number of uninsured children. This is somewhat higher than the estimated 0.74 percentage point, or 4.95% decrease found in section 6.3, though this difference is, again, not significant based on the 95% confidence intervals for these estimates. Finally, for states using combination expansions, there was an estimated 0.82 percentage point, or 6.76%, reduction in the number of uninsured children, which is similar to the estimated 0.85 percentage point, or 6.97%, decrease found in section 6.3.

Table 14: Simulated Effects of Medicaid and State Program Expansions: Overall, By Expansion Type, and by Poverty-Level Group

					95% Confidence Interval	
	Actual P(U)	P(U|2000)	P(U|1996)	% pt. □P(U)	Lower	Upper
All	12.64%	12.64%	13.54%	-0.90***	-1.43	-0.36
				(0.2720)		
By Expansion Type:						
Medicaid Only	10.86%	10.87%	11.87%	-1.00***	-1.46	-0.55
				(0.2320)		
SCHIP Only	14.14%	14.38%	15.31%	-0.93***	-1.52	-0.33
				(0.3037)		
Combination	11.37%	10.59%	11.41%	-0.82***	-1.40	-0.25
				(0.2921)		
By Poverty Level:						
0-100% FPL	22.06%	21.08%	24.19%	-3.11***	-4.38	-1.85
				(0.6469)		
100-150%FPL	20.81%	21.58%	25.46%	-3.88***	-5.32	-2.45
				(0.7323)		
150-200% FPL	15.42%	17.98%	19.84%	-1.86***	-3.23	-0.49
				(0.6989)		
200-250% FPL	15.60%	14.97%	14.57%	0.39	-0.90	1.68
				(0.6582)		
<250% FPL	7.04%	6.80%	6.47%	0.33	-0.07	0.73
				(0.2035)		

Statistical Significance: *90% Confidence, **95% Confidence, ***99% Confidence
Sample size: 33459 observations

Based on the percent changes in the number of uninsured, the ranking is the same as before, with the largest impact in states using only Medicaid expansions and the smallest impact in states using only state program expansions.

While there is no evidence that any of these estimates are significantly different from those found in section 6.3, it is surprising that the estimated percentage-point change in the number of uninsured children is now higher in states using only state program expansions, compared to states using combination expansions. Based solely on the probit coefficients, the effect is always stronger for changes in Medicaid eligibility than for changes in state program eligibility. Thus, we would expect a smaller effect in states using only state program expansions, compared to states using both Medicaid and state program expansions. The fact that we now estimate a larger effect for state program expansions than for combination expansions results from differences in the distribution of children in each poverty level grouping. Table 15 shows the distribution of children in each poverty-level group for years 1996-2000.

In each year, the percent of children in the lowest poverty-level group is greatest in the states using only state program expansions. Similarly, the percent of children in the highest poverty-level group is lowest in states using only state program expansions. The simulations in table 13 are based only on the 2000 sample. Because the effects of the Title XXI are greatest for the lowest poverty-level group, the change in the uninsurance rates are more pronounced in the states using only state program expansions, since there is a greater percentage of children in the lowest poverty-level group. Similarly, since there is not a significant effect for children in the highest poverty-level group, this effect is less pronounced in states using only state program expansions. This explains why the estimated percentage point changes are greater in states using only state program expansions, compared to states using combination expansions, even though we would predict the opposite, based solely on the probit coefficients.

Table 15: Percent of Children in each Poverty-Level Group, by Type of Expansion:1996-2000

	1996	1997	1998	1999	2000
Medicaid Only:					
0-100% FPL	18.9%	18.6%	18.2%	16.4%	15.8%
100-150% FPL	11.4%	9.7%	8.8%	10.8%	10.2%
150-200% FPL	12.3%	11.7%	10.9%	11.0%	10.7%
200-250% FPL	10.8%	11.0%	9.9%	11.2%	9.5%
>250% FPL	46.6%	49.1%	52.2%	50.6%	53.8%
State Program Only					
0-100% FPL	22.0%	21.8%	20.8%	18.4%	17.2%
100-150% FPL	12.0%	11.5%	11.5%	12.0%	11.9%
150-200% FPL	11.4%	10.7%	10.9%	10.9%	10.3%
200-250% FPL	10.0%	9.4%	9.6%	9.7%	10.6%
>250% FPL	44.6%	46.6%	47.2%	49.0%	50.0%
Combination:					
0-100% FPL	17.5%	16.3%	15.0%	13.7%	13.3%
100-150% FPL	10.0%	9.6%	10.1%	10.3%	9.2%
150-200% FPL	10.2%	9.8%	9.6%	9.2%	10.2%
200-250% FPL	10.2%	10.1%	10.0%	9.6%	8.5%
>250% FPL	52.2%	54.3%	55.3%	57.2%	58.7%

Source: Author's calculations based on the March 1997-2001 CPS

Looking at the estimates for children in each poverty-level grouping, the estimated percentage-point changes in uninsured children are highly significant for those with family income below 200% FPL, as shown in Table 16.

There was a 3.11 percentage point, or 12.37%, reduction for those with family income below the poverty level. There was also a 3.88 percentage point, or 15.72%, reduction for those with family income of 100-150% FPL, and a 1.86, or 10.75%, reduction for those with family income of 150-200% FPL. The estimates for children in wealthier families are slightly positive, though not significantly different from zero. Additionally, the estimated effect for children with family income below 150% FPL is significantly greater than that for children with family income above 200% FPL.

Table 16: Simulated Effects of Medicaid and State Program Expansions: Overall, By Expansion Type, and by Poverty-Level Group

	Actual P(U)	P(U|2000)	P(U|1996)	% pt. □P(U)	95% Confidence Interval Lower	Upper
By Expansion Type and Poverty Level:						
Medicaid:						
0-100% FPL	17.86%	18.80%	22.11%	-3.31***	-4.49	-2.13
				(0.6027)		
100-150%FPL	19.36%	19.95%	22.93%	-2.98***	-4.09	-1.86
				(0.5675)		
150-200% FPL	12.05%	15.09%	17.23%	-2.14***	-3.56	-0.72
				(0.7253)		
200-250% FPL	16.46%	12.48%	13.31%	-0.83*	-1.77	0.12
				(0.4836)		
<250% FPL	5.97%	5.69%	5.45%	0.25	-0.05	0.54
				(0.1520)		
State Program:						
0-100% FPL	24.49%	22.94%	25.94%	-2.99***	-4.48	-1.51
				(0.7597)		
100-150% FPL	23.03%	23.23%	27.30%	-4.07***	-5.58	-2.56
				(0.7699)		
150-200% FPL	17.14%	20.20%	21.95%	-1.74**	-3.38	-0.11
				(0.8336)		
200-250% FPL	15.52%	16.58%	15.84%	0.74	-0.82	2.30
				(0.7947)		
<250% FPL	7.56%	7.67%	7.33%	0.34	-0.07	0.76
				(0.2114)		
Combination:						
0-100% FPL	20.90%	18.97%	22.55%	-3.58***	-5.04	-2.12
				(0.7457)		
100-150% FPL	17.57%	19.19%	23.48%	-4.29***	-5.87	-2.71
				(0.8045)		
150-200% FPL	15.04%	16.25%	18.30%	-2.05***	-3.58	-0.52
				(0.7814)		
200-250% FPL	15.76%	13.19%	12.72%	0.47	-1.02	1.95
				(0.7559)		
<250% FPL	6.95%	5.97%	5.60%	0.37	-0.08	0.83
				(0.2321)		

Statistical Significance: *90% Confidence, **95% Confidence, ***99% Confidence
Sample size: 33459 observations

These results are very similar when looking at the estimated effect for each poverty-level group, given the type of expansion used, as shown in Table 16. Again, there was a significant effect for all children with family income below 200% FPL, regardless of the type of expansion used. The estimated percentage-point change in uninsured children is also significantly greater for those with family income below 150% FPL, compared to those with family income above 200% FPL, regardless of the type of expansion used, based on the 95% confidence intervals. For children in states using only Medicaid expansions, there was also a weakly significant effect for children with family income 200-250% FPL, which is not seen when looking at the effects on each poverty-level group for all states, combined.

All of these simulated effects are presented mostly for descriptive purposes. While the differences are due in part to the difference in the probit coefficients for the policy variables, they are also due to differences in the average size of the expansions and differences in the distribution of children across poverty-level groups, for each type of expansion.

7.4 Average Partial Effects

Again, in order to present a comparison of the effect of Medicaid expansions, relative to an equivalent state program expansion, average partial effects of each type of policy change are estimated separately for each poverty-level group used in the probit model. The interpretation of these estimates is more straightforward than for the estimates in section 7.3, since they vary only with the type of expansion used, and not because of differences in the size of the expansion or the distribution of children across poverty-level groups. The policy variables are defined as in section 6.4 for a particular poverty-level group, and set to zero for all other poverty-level groups. The mean value of all other regressors is defined as the mean value observed in the sample for that particular poverty-level group. This allows us to compare the average effect of the policy changes on an individual with average characteristics within each poverty level group. The results are presented in Table 17. The average partial effects of both Medicaid and state program expansions are significant for children with family income below 200% FPL. Medicaid expansions also have a weakly significant effect for children with family income of 200-250% FPL. Neither type of expansion is significant for children with family income above 250% FPL.

Table 17: Average Partial Effects, by Poverty-Level Group

Poverty Level Group	Policy Change	Change in P(U)	Std. Error	95% Confidence Interval	
				Upper	Lower
0-100% FPL	Medicaid Expansion	-4.16***	0.7279	-5.59	-2.74
	State Expansion	-2.86***	0.7253	-4.29	-1.44
100-150% FPL	Either Expansion	-3.72***	0.6924	-5.08	-2.37
150-200% FPL	Medicaid Expansion	-2.80***	0.9257	-4.61	-0.98
	State Expansion	-1.59**	0.7597	-3.08	-0.10
200-250% FPL	Medicaid Expansion	-1.03*	0.5969	-2.20	0.14
	State Expansion	0.69	0.7409	-0.76	2.14
>250% FPL	Either Expansion	0.29	0.1837	-0.07	0.65

*Statistically significant at the 90% confidence level
**Statistically significant at the 95% confidence level
***Statistically significant at the 99% confidence level

Comparing Medicaid and state program expansions within each poverty-level group, the point estimates for Medicaid expansions are greater than those for state program expansions. Based on the 95% confidence intervals, however, there is no evidence of a significant difference between Medicaid expansions and state program expansions, within a particular poverty-level group. For those with family income of 100-150% FPL and those with family income above 250% FPL, specification tests in section 7.1 have shown that there is not a significant difference between the two policy approaches. Thus, only one average partial effect statistic is estimated which applies to either type of expansion.

Comparing the results across groups, the effects of both types of expansions are stronger for the 0-100% FPL and 100-150% FPL groups compared to those with family incomes above 250% FPL. This is a stronger result than simply stating that there was a significant effect for these groups, due to the fact that the confidence interval for the over 250% FPL group includes some negative estimates. The effects of Medicaid expansions are also significantly greater for the 0-100% FPL and 100-150% FPL groups compared to either type of expansion for the 200-250% FPL group; the effects of state program expansions are significantly greater for the 0-100% FPL and 100-150% FPL groups, compared to state program expansions for the 200-250% FPL group.

Chapter 8

Effects on Parents' Insurance Status

8.1 Regression on the Likelihood of Having an Uninsured Parent

In Section 6.2, the probit model is used to estimate the average effect of the Medicaid and state program expansions on the likelihood of being uninsured. In Section 7.2, the model is expanded to estimate the average effect of the Medicaid and state program expansions on the likelihood of being uninsured within different poverty-level groups. The probit coefficients found in section 6.2 indicate that the eligibility expansions, both through Medicaid and through state programs, had a significant impact on reducing children's likelihood of being uninsured. In Section 7.2, the probit coefficients indicate that the Medicaid expansions had a significant impact on reducing the children's likelihood of being uninsured for children with family income less than 250% FPL, while the state program expansions had a significant impact for children with family income less than 200% FPL. For children with family income above these thresholds, there was no significant effect on the likelihood of being uninsured.

In order to test that the change in uninsurance rates is truly a result of Title XXI expansions, the probit model is used to test if the Title XXI expansions had an effect on children's likelihood of having an uninsured parent. Thus, the dependent variable indicating whether a child is uninsured is replaced with a variable indicating whether he or she has an uninsured parent. If the effect of the Title XXI expansions is unique to children, then the policy variables should have no effect.

This test is first implemented by using a single set of policy variables for all children, as in Chapter 6. This addresses the question of whether there is any average effect on the likelihood of having an uninsured parent, based on the full sample of children from all poverty-level groups combined. The model is first estimated using separate variables for Medicaid eligibility thresholds ('MLim') and increases in eligibility thresholds for state programs ('SDif'). The model is then re-estimated imposing the restriction that Medicaid and state program expansions have the same marginal effect, so that 'MLim and 'SDif' are replaced with a single variable equal to their sum, or the total eligibility threshold from either type of program. A likelihood ratio test is used to determine if there is any evidence of a different marginal effect from Medicaid expansions, compared to state program expansions. The results are shown in Table 18, below.

Table 18: Comparison of Probit Model Specifications for the Likelihood of Having an Uninsured Parent

	I	II
Policy Variable:	Coefficient (Standard Error)	
MLim	-0.00869 (0.02865)	-0.02594 (0.01828)
SDif	-0.03485 (0.02147)	
Log-Likelihood	-68555.972	-68556.789
LR Statistic (one restriction): 1.634		
Coefficients and LR Statistic not Statistically Significant Model I is the unrestricted model Model II is restricted so that the coeffients on MLim and SDif are equal		

In the unrestricted model, the estimated coefficients for 'MLim' and 'SDif' are not significantly different from zero, as shown in Table 18. When the model is restricted so that both Medicaid and state program expansions have the same marginal effect, the resulting coefficient on the combined policy variable is also not significantly different from zero. Based on a likelihood ratio test, we cannot reject the null hypothesis that there is any difference in the effect of changes in Medicaid eligibility, compared to changes in state program expansions. Thus, there is no evidence that neither type of expansion has had a significant effect on the likelihood of having an uninsured parent.

These results support the hypothesis that the estimated changes in uninsurance rates for children, shown in Chapter 6, were in response to changes in the eligibility for public insurance. This is due to the fact that the expansions do have a significant effect on children's likelihood of being uninsured, but not on their likelihood of having an uninsured parent.

8.2 Regression on the Likelihood of Having an Uninsured Parent using Policy/Poverty-Level Group Interactions

The basic probit model is expanded to allow the effects of the policy variables, on the likelihood of having an uninsured parent, to differ across poverty-level groups. Surprisingly, increases in children's eligibility limits are correlated with a reduction in the likelihood of having an uninsured parent, for children with family income below the poverty-level. Based on likelihood ratio tests, there is no significant difference in the marginal effect of increased Medicaid eligibility, compared to increased eligibility through state programs, for children with family income below 50% FPL. For children with family income of 50-100% FPL, there is a statistically significant difference in the marginal effect of Medicaid expansions, compared to state program expansions. For those with family incomes above the poverty level, there is no significant effect for either type of expansion, nor is there a difference between Medicaid and state program expansions.

As shown in Table 19, both Medicaid and state program expansions appear to have a significant impact in reducing the likelihood of having an uninsured parent, for children with family income below the poverty level.

Table 19: Probit Regression Results for the Likelihood of Having an Uninsured Parent with Policy/Poverty-Level Interactions

	Coefficient	Std. Error	mean value	Marginal Effect at mean value
Policy Variables:				
Limit: 0-50% FPL	-0.09246**	0.03702	0.12246	-0.02118
MLim: 50-100% FPL	-0.07251*	0.04005	0.13491	-0.01661
SDif: 50-100% FPL	-0.16859***	0.03901	0.03657	-0.03862
Limit: >100% FPL	0.00861	0.01926	1.39503	0.00197
Pseudo R-Squared		0.1534		
Log-Likelihood		-62843.353		
Actual Percent Uninsured		18.65%		
Predicted % Uninsured, at Mean Values		14.61%		

*Statistically significant at the 90% confidence level
**Statistically significant at the 95% confidence level
***Statistically significant at the 99% confidence level

Standard errors are robust to heteroskedasticity and correlation between observations within the same household.
Sample Size is 167,535 observations. Estimation performed using sample weights. The complete results for the probit coefficients are shown in Appendix C.

Recall that there was a significant effect on the children's likelihood of being uninsured for those with family incomes up to 250% FPL for Medicaid expansions and up to 200% FPL for state program expansions. Thus, even though there is an effect for low-income parents, it is not identical to the effect for children. However, these results may indicate there is something affecting both the children's and the parents' insurance choices which is not identified in the model.

One explanation for this effect on parent's insurance status is the fact that parents in low-income families are, in fact, eligible for Medicaid, though the Medicaid eligibility limits for parents are typically not nearly as great as those for children. States are required to extend Medicaid eligibility to all parents who meet the AFDC income, resource, and family composition eligibility rules in place as of July 16, 1996. In addition, states have the option to expand Medicaid eligibility limits for parents beyond this limit and to relax the family composition rules which excluded married couples from coverage under AFDC. Some states have also expanded Medicaid eligibility for parents further under approved waivers from the Health Care Financing Agency. In fact, by June 2001, 19 states provided Medicaid eligibility for parents with incomes at or above the poverty level (Guyer, 2002).

While the Title XXI expansions did not apply directly to parents, it is likely that they were somewhat correlated with expanded eligibility for parents. In addition, the large response for children who were likely eligible for Medicaid, even in the absence of Title XXI expansions, indicates that part of the response was simply due to increased awareness of public insurance programs, or reduced welfare stigma. Thus, there may have been an increased awareness of public health insurance availability for parents as well as for their children. Finally, in states that expanded eligibility for both children and parents, though not necessarily to the same extent, this may have resulted in a stronger impact for both children and parents than would be expected from either type of expansion alone.

Since the model does not identify changes in eligibility for the parents, it is not surprising that any resulting effect on both the parents' and children's insurance status is picked up by the Title XXI policy variables. The fact that the probit regression for children resulted in changes in uninsurance rates for children up to 200% FPL, rather than up to the poverty level, shows that the model is identifying the effects of Title XXI expansions, beyond the effects of changes in parents' eligibility limits.

CHAPTER 9

CONCLUSIONS AND POLICY IMPLICATIONS

Changes in the availability of public insurance, following passage of Title XXI legislation, have had a significant impact on reducing the number of uninsured children. Overall, Medicaid and state program expansions have led to a small, but significant reduction in the number of uninsured children. Comparing results by the type of expansion used, all three approaches had a significant effect on reducing the number of uninsured children, with the greatest effect consistently seen in states using only Medicaid expansions, although this difference is not statistically significant.

The estimated effects, from Chapter 6, of the Title XXI expansions are that they resulted in a 5.9% decrease in the number of uninsured children, based on the simulations performed on the 2000 sample. For states using only Medicaid expansions, the estimated decrease is 8.2%, compared to 5.0% in states using only state program expansions and 7.0% in states using combination expansions.

Comparing the effects of the Title XXI expansions across different poverty level groups, states using only Medicaid expansions had the largest impact on uninsurance rates for children below the poverty level. For states using only state program expansions or combination expansions, the largest impact on uninsurance rates is observed for children with family income of 100-150% FPL, though there is a similarly large impact on children with income below the poverty level as well. The strong impact of the Title XXI expansions on children with income below the poverty level is somewhat surprising, given that all of these children would have gained eligibility by 2000 (the last year included in the study), even in the absence of Title XXI expansions. This effect is explained in part by the increased awareness of public health insurance programs as states promoted their expansions. However, the response of children with family income below the poverty level is also likely due to the fact that their parents were also more likely to become eligible for Medicaid over the study period, though not as a direct result of Title XXI expansions.

There was also a significant impact for all children with family income up to 200% FPL, across all states, and for children with income up to 250% FPL in states using Medicaid expansions. As would be expected, neither Medicaid nor state program expansions had a significant impact on children in wealthier families.

Based on the specification tests in sections 6.1 and 7.1, there is some evidence that increases in eligibility through Medicaid may have had a larger effect than increases in eligibility through state programs. This difference may be due in part to the fact that the state programs typically use cost-sharing measures, while the Medicaid programs typically do not. The presence of cost-sharing measures may create a deterrent for enrollment in the newer state programs.

Another difference in the state programs, compared to Medicaid, is the use of waiting periods for eligibility. These waiting periods only apply to children with recent coverage under private health insurance policies. The intent of these waiting periods is to discourage crowding out of private insurance. However, if families are willing to drop private health insurance coverage for children in order to enroll them in the public program in the future, there may be a transitory *increase* in the number of uninsured children due to the use of waiting periods. This may partially explain the different effects of a marginal increase in Medicaid eligibility, relative to a marginal increase in eligibility through the state programs.

The estimated average partial effects offer a direct comparison of the effect of an average-sized expansion through either Medicaid or through a state program. Overall, it is estimated that an average sized Medicaid expansion would result in a 1.21 percentage-point reduction in the number of uninsured children, compared to a 0.65 percentage-point reduction through an equivalent state program expansion. For children below the poverty level, it is estimated that an average sized Medicaid expansion would result in a 4.16 percentage-point reduction in the number of uninsured children, compared to a 2.86 percentage-point reduction for an equivalent state program expansion. For children between 100-150% FPL, it is estimated that an average sized expansion, through either Medicaid or a state program, would result in a 3.72 percentage-point reduction in the number of uninsured children. For children between 150-200% FPL, it is estimated that an average sized Medicaid expansion would result in a 2.80 percentage-point reduction in the number of uninsured children, compared to a 1.59 percentage-point reduction for an equivalent state program. For children with family income of 200-250% FPL, an average sized Medicaid expansion results in a weakly significant reduction in the number of uninsured children, with no significant change resulting from an equivalent state program expansion. For all children above 250% FPL, there is not a significant effect from either program. However, none of the differences between Medicaid and state program expansions is significant.

From a state policy perspective, these results indicate that Medicaid expansions may be the better alternative, since they seem to have the largest effect on reducing uninsurance rates. However, this alone does not necessarily mean that Medicaid expansions are the optimal policy choice. From a social welfare perspective, a true measure of cost-effectiveness would compare the social cost of the Title XXI expansions and the social gains resulting from the expansions.

The social gains would ideally reflect the gains in welfare for the participants and their families. This would include any resulting improvement in health outcomes as well as the reduction in expenditures used for health services which can be put to other use. For families with children who would have been uninsured in the absence of the Title XXI expansions, their welfare is increased by the fact that the children have access to health services, which hopefully results in improved health, without having to sacrifice other expenditures. For families that drop private insurance coverage in order to enroll their children in public programs, there is also likely an increase in welfare even if there is no effect on the utilization of health services, since expenditures on health insurance or health care, if any, would be freed up for other uses. This may be offset, though if indirect crowding out leaves other family members uninsured. From a revealed preference perspective, families whose children are enrolled in the public programs must be somewhat better off, or they would not have enrolled in the first place.

The social cost of the Title XXI expansions would reflect not only the state and federal expenditures for these programs, but also any resulting losses in economic efficiency. The additional state and local expenditures would need to be financed through increased tax revenues, resulting in excess burden, or through reduced spending elsewhere, resulting in welfare losses for those affected by the spending cuts.

In determining the best policy option, states must consider the both the benefits and cost of the policy change, although both of these may be difficult to infer. This research provides a first step in understanding the benefits of the Title XXI expansions, in terms of the reduction in uninsured children. The implicit assumption is that such a reduction will lead to better health for those children who would otherwise have been uninsured. In terms of the financial benefits to participating families, the greatest benefit likely goes to those who would otherwise have been uninsured. However, there are also likely financial benefits to all participating families, including those that crowd out private insurance. Since the estimates presented in this research identify the decrease in uninsured children as a result of Title XXI expansions, this provides some insight of the number of children whose families are likely to gain the most benefit from the policy changes. However, this presents an incomplete picture of the total number of children whose families receive at least some benefit from the programs.

From the perspective of state policy makers, state programs have some potential cost advantages. First, if state program expansions are successful in reducing the magnitude of crowding out, compared to Medicaid expansions, this will reduce the number of individuals whose health insurance is provided by public funds. Second, the inclusion of cost-sharing measures in state programs may lead to reduction in the use of medical services. Finally, state programs are not required to be as generous as Medicaid in terms of covered services, though they do have to be at least equivalent to certain benchmark policies, as discussed in section 2.4. Thus, the two types of public programs may have different effects on health or the value to beneficiaries.

Since there is no conclusive evidence that Medicaid and state program expansions have a different effect on reducing uninsurance rates, the key advantages to state programs is the likely reduction in the prevalence of crowding out, and the potential for cost savings, on average, per enrollee. However, state programs would have additional start-up costs in designing and implementing the program, which would not be necessary to simply expand the existing Medicaid programs. State programs may also result in some additional costs due to duplication of administrative costs.

It is important to note that any cost-saving through state programs, compared to Medicaid programs, is limited to insuring those who gain eligibility through the Title XXI expansions. Title XXI specifies that any children eligible for Medicaid, based on the eligibility limits in place as of March 31, 1997, are enrolled in the state's Medicaid program; states will only receive the regular Medicaid FMAP for insuring these children, not the enhanced FMAP under Title XXI. The results in chapter 7 indicate that the largest response to Title XXI expansions was in children with family incomes below 150% FPL. Since many of these children would have been eligible without the Title XXI expansions, such children would be enrolled in Medicaid, regardless of the expansion type used, and states would receive only the FMAP for Medicaid in insuring these children. Thus, any potential cost savings through state programs would not be achieved in insuring these children, nor would the states benefit from the enhanced FMAP.

One potential danger in using state program expansions is the fact that federal funds available under the enhanced FMAP are capped. Once these funds have been exhausted, states do not receive any additional funds for providing insurance through the state programs. In this event, states would be left with the decision to continue coverage using only state funds, or to freeze enrollment in the state program. States using Medicaid expansions have more protection from this occurrence, since they can continue to receive the FMAP for Medicaid, once the enhanced FMAP has been exhausted.

In designing an optimal state policy, the results of this research indicate that the reduction in the number of uninsured are not significantly different for Medicaid expansions and state program expansions. While this is not an ideal measure, it does indicate that the benefits resulting from either type of expansion are likely similar. Thus, the use of state programs may be preferred if they succeed in reducing the public cost of providing coverage. Such a reduction would also limit the excess burden associated with financing the additional public expenditures. However, due to the funding cap on federal contributions for Title XXI expenditures, states would not want to make their state program expansions so generous that they risk losing the federal match on a portion of their expenditures. For large eligibility expansions, a safer route, from the state perspective, is to use a combination expansion, since Medicaid expenditures will be reimbursed at the regular Medicaid FMAP once the federal cap for Title XXI funds has been reached.

APPENDICES

APPENDIX A: MEDICAID AND STATE PROGRAM ELIGIBILITY LIMITS

Table 20: Eligibility Thresholds for Infants, 1996 and 2000

State	Medicaid 1996	SCHIP 1996	Medicaid 2000	SCHIP 2000
Alabama	133	-	133	200
Alaska	133	-	200	-
Arizona	140	-	140	200
Arkansas	133	-	200	-
California	200	300	200	300
Colorado	133	-	133	185
Connecticut	185	-	185	300
Delaware	185	-	185	200
Florida	185	-	185	200
Georgia	185	-	185	200
Hawaii	185	-	185	-
Idaho	133	-	150	-
Illinois	133	-	200	-
Indiana	150	-	150	-
Iowa	185	-	185	-
Kansas	150	-	150	200
Kentucky	185	-	185	200
Louisiana	133	-	150	-
Maine	185	-	185	-
Maryland	185	-	200	-
Massachusetts	185	-	200	-
Michigan	185	-	185	200
Minnesota	275	-	280	-
Mississippi	185	-	185	-
Missouri	185	-	300	-
Montana	133	-	133	150
Nebraska	150	-	185	-
Nevada	133	-	133	200
New Hampshire	185	-	300	-
New Jersey	185	-	185	350
New Mexico	185	-	235	-
New York	185	-	185	192
North Carolina	185	-	185	200
North Dakota	133	-	133	140
Ohio	133	-	150	-
Oklahoma	150	-	185	-
Oregon	133	-	133	170
Pennsylvania	185	235	185	235
Rhode Island	250	-	250	-
South Carolina	185	-	185	-
South Dakota	133	-	140	-
Tennessee	400	-	400	-
Texas	185	-	185	-
Utah	133	-	133	200
Vermont	225	-	225	300
Virginia	133	-	133	185
Washington	200	-	200	-
West Virginia	150	-	150	-
Wisconsin	185	-	185	-
Wyoming	133	-	133	-

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Table 21: Eligibility Thresholds for Children Ages 1-5, 1996 and 2000

State	Medicaid 1996	SCHIP 1996	Medicaid 2000	SCHIP 2000
Alabama	133	-	133	200
Alaska	133	-	200	-
Arizona	133	-	133	200
Arkansas	133	-	200	-
California	133	300 (age 2)	133	300/200
Colorado	133	-	133	185
Connecticut	185	-	185	300
Delaware	133	-	133	200
Florida	133	-	133	200
Georgia	133	-	133	200
Hawaii	133	-	133	-
Idaho	133	-	150	-
Illinois	133	-	133	185
Indiana	133	-	150	-
Iowa	133	-	133	185
Kansas	133	-	133	200
Kentucky	133	-	150	200
Louisiana	133	-	150	-
Maine	133	-	150	185
Maryland	185	-	200	-
Massachusetts	133	-	150	200
Michigan	150	-	150	200
Minnesota	275	-	280/275	-
Mississippi	133	-	133	-
Missouri	133	-	300	-
Montana	133	-	133	150
Nebraska	133	-	185	-
Nevada	133	-	133	200
New Hampshire	185	-	185	300
New Jersey	133	-	133	350
New Mexico	185	-	235	-
New York	133	185	133	192
North Carolina	133	-	133	200
North Dakota	133	-	133	140
Ohio	133	-	150	-
Oklahoma	133	-	185	-
Oregon	133	-	133	170
Pennsylvania	133	235	133	235
Rhode Island	250	-	250	-
South Carolina	133	-	150	-
South Dakota	133	-	140	-
Tennessee	400	-	400	-
Texas	133	-	133	-
Utah	133	-	133	200
Vermont	225	-	225	300
Virginia	133	-	133	185
Washington	200	-	200	-
West Virginia	133	-	150	-
Wisconsin	185	-	185	-
Wyoming	133	-	133	-

Table 22: Eligibility Thresholds for Children Age 6 and Older, Born After 10/1/1983, 1996 and 2000

State	Medicaid 1996	SCHIP 1996	Medicaid 2000	SCHIP 2000
Alabama	100	-	100	200
Alaska	100	-	200	-
Arizona	100	-	100	200
Arkansas	100	-	200	-
California	100	-	100	200
Colorado	100	-	100	185
Connecticut	185	-	185	300
Delaware	100	-	100	200
Florida	100	-	100	200
Georgia	100	-	100	200
Hawaii	100	-	100	-
Idaho	100	-	150	-
Illinois	100	-	133	185
Indiana	100	-	150	-
Iowa	100	-	133	185
Kansas	100	-	100	200
Kentucky	100	-	150	200
Louisiana	100	-	150	-
Maine	125	-	150	185
Maryland	185	-	200	-
Massachusetts	114	-	150	200
Michigan	150	-	150	200
Minnesota	275	-	275	-
Mississippi	100	-	100	133
Missouri	100	-	300	-
Montana	100	-	100	150
Nebraska	100	-	185	-
Nevada	100	-	100	200
New Hampshire	185	-	185	300
New Jersey	100	-	133	350
New Mexico	185	-	235	-
New York	100	185	100	192
North Carolina	100	-	100	200
North Dakota	100	-	100	140
Ohio	100	-	150	-
Oklahoma	100	-	185	-
Oregon	100	-	100	170
Pennsylvania	100	235 (<age 10)	100	235
Rhode Island	100	-	250	-
South Carolina	100	-	150	-
South Dakota	100	-	140	-
Tennessee	400	-	400	-
Texas	100	-	100	-
Utah	100	-	100	200
Vermont	225	-	225	300
Virginia	100	-	100	185
Washington	200	-	200	-
West Virginia	100	-	100	150
Wisconsin	100	-	185	-
Wyoming	100	-	100	133

Table 23: Eligibility Thresholds for Children Born Before 10/1/1983, 1996 and 2000

State	Medicaid 1996	SCHIP 1996	Medicaid 2000	SCHIP 2000
Alabama	15	-	100	200
Alaska	71	-	200	-
Arizona	30	-	30	200
Arkansas	18	-	200	-
California	82	-	100	300
Colorado	37	-	37	185
Connecticut	100	-	185	300
Delaware	100	-	100	200
Florida	28	-	100	200
Georgia	100	-	100	200
Hawaii	100	-	100	-
Idaho	100	-	150	-
Illinois	46	-	133	185
Indiana	24	-	150	-
Iowa	37	-	133	185
Kansas	100	-	100	200
Kentucky	33	-	150	200
Louisiana	10	-	150	-
Maine	125	-	150	185
Maryland	40	-	200	-
Massachusetts	86	-	150	200
Michigan	100	-	150	200
Minnesota	275	-	275	-
Mississippi	34	-	100	133
Missouri	100	-	300	-
Montana	41	-	41	150
Nebraska	33	-	185	-
Nevada	31	-	31	200
New Hampshire	185	-	185	300
New Jersey	41	-	133	350
New Mexico	185	-	235	-
New York	51	185	100	192
North Carolina	100	-	100	200
North Dakota	40	-	100	140
Ohio	33	-	150	-
Oklahoma	48	-	185	-
Oregon	100	-	100	170
Pennsylvania	41	-	41	235
Rhode Island	100	-	250	-
South Carolina	48	-	150	-
South Dakota	100	-	140	-
Tennessee	400	-	400	-
Texas	17	-	100	-
Utah	100	-	100	200
Vermont	225	-	225	300
Virginia	100	-	100	185
Washington	200	-	200	-
West Virginia	100	-	100	150
Wisconsin	45	-	185	-
Wyoming	55	-	55	133

Sources for all tables in Appendix A: HCFA State Reports (www.hcfa.gov/init/chpa-map.htm); Mathematica Policy Research, Inc. (2001); and the State Children's Health Insurance (SCHIP) Database from the U.S. Department of Health and Human services, Office of the Assistant Secretary for Planning and Evaluation (aspe.hhs.gov/health/schip2/default.htm).

APPENDIX B: PROBIT REGRESSION RESULTS FROM CHAPTER 6

Table 24: Complete Probit Regression Results

	Coefficient	Standard Error	Mean Value	Marginal Effect at Mean Value
Policy Variables:				
MLim	-0.085601***	0.026623	1.34	-0.016568
SDif	-0.045405**	0.021085	0.35	-0.008788
Demographic Variables:				
Female	-0.006155	0.008943	0.49	-0.001191
White	-0.099542**	0.049429	0.79	-0.019934
Black	-0.124164**	0.052383	0.16	-0.022822
Asian	0.034132	0.060298	0.04	0.006731
Other Race	(omitted)			
Both Parents	-0.907113***	0.030535	0.69	-0.213352
Mother Only	-1.008200***	0.031347	0.23	-0.142731
Father Only	-0.689998***	0.037684	0.04	-0.089274
Neither Parent	(omitted)			
# in Family	-0.130560***	0.022114	4.27	-0.025270
# in Family Squared	0.012723***	0.002142	20.31	0.002463
Family Earnings	-0.072120***	0.006511	5.05	-0.013959
Family Earnings Squared	0.001507***	0.000148	54.92	0.000292
Other Income	-0.176135***	0.015570	0.47	-0.034091
Other Income Squared	0.008233***	0.001348	1.52	0.001594
0-50% FPL	0.290364***	0.047792	0.08	0.064710
50-100% FPL	0.401608***	0.042003	0.11	0.092904
100-150% FPL	0.480721***	0.037482	0.11	0.114598
150-200% FPL	0.377220***	0.034030	0.11	0.086366
200-250% FPL	0.257503***	0.031648	0.10	0.056157
250-300% FPL	0.077069**	0.029842	0.09	0.015489
300-350% FPL	0.048875	0.030016	0.08	0.009696
<350% FPL	(omitted)			
Age Indicators:				
Newborn	(omitted)			
Age=1	-0.199195***	0.029316	0.06	-0.034559
Age=2	-0.192641***	0.028460	0.05	-0.033539
Age=3	-0.167063***	0.028428	0.06	-0.029511
Age=4	-0.159103***	0.028299	0.06	-0.028235
Age=5	-0.145800***	0.028206	0.06	-0.026068
Age=6	-0.146512***	0.030756	0.06	-0.026186
Age=7	-0.141329***	0.030841	0.06	-0.025336
Age=8	-0.151296***	0.031475	0.06	-0.026963
Age=9	-0.160236***	0.031326	0.06	-0.028432
Age=10	-0.114640***	0.031215	0.06	-0.020857
Age=11	-0.101348***	0.031582	0.06	-0.018569
Age=12	-0.108353***	0.031713	0.06	-0.019774
Age=13	-0.058680*	0.031742	0.06	-0.011004
Age=14	-0.057978*	0.033086	0.06	-0.010876
Age=15	-0.048789	0.034571	0.05	-0.009197
Age=16	-0.048869	0.035722	0.05	-0.009212
Age=17	-0.021912	0.035905	0.05	-0.004191

Complete Probit Regression Results (Contiued)

		Coefficient	Standard Error	Mean Value	Marginal Effect at Mean Value
State Indicators:					
	1	(omitted)			
	2	-0.044448	0.090873	0.00	-0.008376
	3	-0.144333	0.102890	0.00	-0.025571
	4	-0.078047	0.077211	0.02	-0.014434
	5	-0.229304**	0.104011	0.00	-0.038510
	6	0.026600	0.089129	0.01	0.005229
	7	0.109541	0.067429	0.07	0.022425
	8	0.240090***	0.071973	0.03	0.052933
	9	-0.195229***	0.074663	0.04	-0.033831
	10	-0.067572	0.071951	0.04	-0.012597
	11	0.056408	0.081996	0.02	0.011275
	12	0.064787	0.068673	0.05	0.012984
	13	-0.088796	0.071926	0.04	-0.016349
	14	-0.224681***	0.085523	0.02	-0.038035
	15	0.031959	0.092170	0.02	0.006301
	16	-0.126222	0.084294	0.01	-0.022648
	17	-0.036020	0.086737	0.02	-0.006827
	18	0.069610	0.086591	0.00	0.014039
	19	-0.050444	0.083310	0.00	-0.009471
	20	-0.152005*	0.082235	0.01	-0.026823
	21	-0.010104	0.082790	0.01	-0.001944
	22	-0.030309	0.088456	0.00	-0.005761
	23	0.117819	0.089574	0.02	0.024383
	24	0.128793	0.080859	0.02	0.026799
	25	-0.164579*	0.086489	0.00	-0.028807
	26	0.097976	0.073203	0.03	0.020036
	27	0.186077**	0.084489	0.01	0.040012
	28	0.109032	0.078674	0.03	0.022424
	29	0.281670***	0.067743	0.05	0.063093
	30	0.076916	0.081280	0.01	0.015563
	31	0.166692	0.111101	0.02	0.035421
	32	0.027774	0.083149	0.02	0.005463
	33	0.158312*	0.081265	0.01	0.033541
	34	0.246361***	0.078410	0.01	0.054759
	35	0.292102***	0.078491	0.02	0.066391
	36	0.318726***	0.077868	0.01	0.073531
	37	0.449864***	0.065390	0.08	0.107362
	38	0.160366**	0.078585	0.00	0.034061
	39	0.243325***	0.075103	0.01	0.054065
	40	0.053444	0.080436	0.00	0.010678
	41	0.276018***	0.079338	0.01	0.062237
	42	0.316486***	0.077185	0.01	0.073012
	43	0.401485***	0.072211	0.02	0.096233
	44	0.033824	0.079971	0.01	0.006678
	45	0.370491***	0.076606	0.01	0.087859
	46	0.028265	0.084769	0.02	0.005560
	47	0.055439	0.081861	0.01	0.011082

Complete Probit Regression Results (Contiued)

		Coefficient	Standard Error	Mean Value	Marginal Effect at Mean Value
State Indicators (Continued):					
	48	0.288714***	0.065061	0.13	0.063203
	49	0.343385***	0.081689	0.00	0.080462
	50	-0.265045***	0.101185	0.00	-0.043517
Year Indicators:					
1996		(omitted)			
1997		0.029982	0.019735	0.199128	0.005866
1998		0.077320***	0.020898	0.200076	0.015386
1999		0.045789*	0.023881	0.201189	0.009009
2000		0.016302	0.026002	0.201708	0.003174
Constant		0.326163***	0.114073		

Pseudo R-Squared	0.1082
Log-Likelihood	-63059.641
Actual Percent Uninsured	14.08%
Predicted % Uninsured, at Mean Values	11.45%

*Statistically significant at the 90% confidence level
**Statistically significant at the 95% confidence level
***Statistically significant at the 99% confidence level

Standard errors are robust to heteroskedasticity and correlation between observations within the same household.

Sample Size is 174,004 observations. Estimation performed using sample weights.

APPENDIX C: LINEAR PROBABILITY MODEL REGRESSION RESULTS FROM CHAPTER 6

Table 25: Complete Linear Probability Model Results

	Coefficient	Standard Error
Policy Variables:		
MLim	-0.0167019	0.005021
SDif	-0.0106752	0.004283
Demographic Variables:		
Female	-0.0010992	0.001797
White	-0.0221602	0.013307
Black	-0.0312365	0.013885
Asian	0.0002280	0.015340
Other Race	(omitted)	
Both Parents	-0.2736983	0.010520
Mother Only	-0.3056751	0.010609
Father Only	-0.2234299	0.012193
Neither Parent	(omitted)	
# in Family	-0.0370099	0.005959
# in Family Squared	0.0033298	0.000610
Family Earnings	-0.0087488	0.000912
Family Earnings Squared	0.0001983	0.000023
Other Income	-0.0267623	0.002432
Other Income Squared	0.0015741	0.000267
0-50% FPL	0.0984876	0.008918
50-100% FPL	0.1193256	0.007750
100-150% FPL	0.1299610	0.007063
150-200% FPL	0.0913267	0.006316
200-250% FPL	0.0545841	0.005581
250-300% FPL	0.0147453	0.004701
300-350% FPL	0.0066755	0.004484
<350% FPL	(omitted)	
Age Indicators:		
Newborn	(omitted)	
Age=1	-0.0374241	0.006080
Age=2	-0.0358064	0.005912
Age=3	-0.0305431	0.005959
Age=4	-0.0283814	0.005981
Age=5	-0.0259083	0.005971
Age=6	-0.0258870	0.006312
Age=7	-0.0240946	0.006335
Age=8	-0.0267768	0.006411
Age=9	-0.0276422	0.006354
Age=10	-0.0191462	0.006416
Age=11	-0.0167642	0.006468
Age=12	-0.0176135	0.006455
Age=13	-0.0085044	0.006549
Age=14	-0.0074483	0.006752
Age=15	-0.0050199	0.006944
Age=16	-0.0044232	0.007121
Age=17	0.0003899	0.007168

Complete Linear Probability Model Results (Continued)

		Coefficient	Standard Error
State Indicators:			
	1	(omitted)	
	2	-0.0017236	0.014167
	3	-0.0243822	0.015290
	4	-0.0126052	0.012528
	5	-0.0230665	0.014609
	6	0.0122255	0.014179
	7	0.0192293	0.011845
	8	0.0467249	0.012930
	9	-0.0270888	0.012217
	10	-0.0116911	0.012144
	11	0.0106215	0.014772
	12	0.0125226	0.011906
	13	-0.0122641	0.011837
	14	-0.0307112	0.012837
	15	0.0112690	0.015024
	16	-0.0200313	0.013604
	17	-0.0048881	0.014424
	18	0.0114341	0.016265
	19	-0.0087950	0.014223
	20	-0.0259355	0.013183
	21	-0.0019607	0.014180
	22	-0.0048356	0.015352
	23	0.0227522	0.014933
	24	0.0253266	0.014560
	25	-0.0392704	0.014873
	26	0.0196270	0.013333
	27	0.0365903	0.016808
	28	0.0205227	0.014725
	29	0.0575270	0.012466
	30	0.0129915	0.015067
	31	0.0278386	0.019909
	32	0.0017689	0.015067
	33	0.0311611	0.016063
	34	0.0495396	0.016026
	35	0.0650814	0.016474
	36	0.0661888	0.016128
	37	0.1060213	0.012106
	38	0.0295091	0.015563
	39	0.0469745	0.014760
	40	0.0043548	0.014945
	41	0.0532540	0.015039
	42	0.0655505	0.015994
	43	0.0948609	0.014852
	44	0.0043484	0.014005
	45	0.0809481	0.015630
	46	0.0045774	0.014158
	47	0.0095031	0.014879

Complete Linear Probability Model Results (Continued)

		Coefficient	Standard Error
State Indicators (Continued):			
	48	0.0593762	0.011634
	49	0.0658866	0.016001
	50	-0.0462345	0.016550
Year Indicators:			
	1996	(omitted)	
	1997	0.0052877	0.004115
	1998	0.0153618	0.004353
	1999	0.0086280	0.004812
	2000	0.0025277	0.005052
Constant		0.5265899	0.025453
R-Squared		0.0895	

*Statistically significant at the 90% confidence level
**Statistically significant at the 95% confidence level
***Statistically significant at the 99% confidence level

Standard errors are robust to heteroskedasticity and correlation between observations within the same household.
Sample Size is 174,004 observations. Estimation performed using sample weights.

APPENDIX D: PROBIT REGRESSION RESULTS FROM CHAPTER 7

Table 26: Complete Probit Regression Results with Policy/Poverty-Level Interactions

	Coefficient	Std. Error	mean value	Marginal Effect at mean value
Policy Variables:				
MLim: 0-100% FPL	-0.1939005	0.035728	0.241106	-0.037525
SDif: 0-100% FPL	-0.1309103	0.033286	0.062194	-0.025334
Limit: 100-150% FPL	-0.1642206	0.030784	0.183872	-0.031781
MLim: 150-200% FPL	-0.1430038	0.049810	0.142979	-0.027675
SDif: 150-200% FPL	-0.0797135	0.038106	0.035656	-0.015427
Mlim: 200-250% FPL	-0.0644463	0.038016	0.132546	-0.012472
SDif: 200-250% FPL	0.0413475	0.044168	0.033748	0.008002
Limit: >250% FPL	0.0333071	0.020904	0.856480	0.006446
Demographic Variables:				
Female	-0.0064609	0.008845	0.486639	-0.0012502
White	-0.1034218	0.033653	0.78663	-0.0207356
Black	-0.1300948	0.035714	0.157817	-0.0238497
Asian	0.0275784	0.041299	0.042962	0.0054183
Other Race	(omitted)			
Both Parents	-0.9061552	0.023516	0.691256	-0.2130654
Mother Only	-1.007975	0.024077	0.234226	-0.1426861
Father Only	-0.6877146	0.029257	0.044271	-0.0890846
Neither Parent	(omitted)			
# in Family	-0.1300754	0.012611	4.27253	-0.0251729
# in Family Squared	0.0010871	11.647	20.3136	0.0024503
Family Earnings	-0.0728045	0.006480	5.04585	-0.0140895
Family Earnings Squared	0.0015170	0.000147	54.9178	0.0002936
Other Income	-0.1770445	0.015564	0.467868	-0.0342626
Other Income Squared	0.008316	0.001347	1.52082	0.0016094
0-50% FPL	0.6409523	0.049099	0.076263	0.1653409
50-100% FPL	0.7530474	0.045697	0.106171	0.1983624
100-150% FPL	0.8072689	0.044723	0.109576	0.2158829
150-200% FPL	0.6472169	0.047476	0.106947	0.1643618
200-250% FPL	0.3842641	0.046363	0.099377	0.0884903
250-300% FPL	0.0788431	0.022536	0.089268	0.0158574
300-350% FPL	0.0530599	0.022504	0.077176	0.0105471
<350% FPL	(omitted)			
Age Indicators:				
Newborn	(omitted)			
Age=1	-0.1979107	0.027703	-0.042749	0.004282
Age=2	-0.1948569	0.027665	-0.042289	0.0042919
Age=3	-0.168561	0.027627	-0.038417	0.0044239
Age=4	-0.1607988	0.027574	-0.037241	0.0044576
Age=5	-0.1474929	0.027272	-0.035124	0.0044806
Age=6	-0.1504215	0.027867	-0.035767	0.0045631
Age=7	-0.1456364	0.027646	-0.034969	0.0045548
Age=8	-0.1547557	0.028081	-0.036486	0.004573

Complete Probit Regression Results with Policy/Poverty-Level Interactions (continued)

		Coefficient	Std. Error	mean value	Marginal Effect at mean value
Age Indicators (continued):					
Age=9		-0.1637089	0.028113	-0.037879	0.0045362
Age=10		-0.1177267	0.027957	-0.0307	0.0047556
Age=11		-0.1046006	0.028347	-0.028715	0.0048913
Age=12		-0.1105604	0.028268	-0.029644	0.0048443
Age=13		-0.0607852	0.028378	-0.021462	0.0051422
Age=14		-0.0609988	0.028931	-0.021695	0.0052409
Age=15		-0.0489723	0.029366	-0.019793	0.0053897
Age=16		-0.0500655	0.029824	-0.020146	0.0054674
Age=17		-0.0205017	0.029803	-0.014983	0.0056423
State Indicators:					
	1	(omitted)			
	2	-0.0598443	0.066902	-0.03476	0.012035
	3	-0.1462676	0.071732	-0.048479	0.0115311
	4	-0.0760787	0.056095	-0.033516	0.0099146
	5	-0.2330182	0.073872	-0.059689	0.0105378
	6	0.0163609	0.064837	-0.021871	0.0127898
	7	0.1157748	0.048597	0.003068	0.0105638
	8	0.2363849	0.052034	0.026819	0.012853
	9	-0.190756	0.052611	-0.049046	0.0081173
	10	-0.0661584	0.051693	-0.030532	0.0092812
	11	0.0627704	0.058045	-0.011045	0.012059
	12	0.068371	0.049914	-0.006636	0.0103893
	13	-0.0880166	0.0522	-0.034099	0.0091268
	14	-0.2185234	0.06219	-0.05503	0.0091343
	15	0.0052353	0.061737	-0.022543	0.0120204
	16	-0.1241495	0.060705	-0.042044	0.0100734
	17	-0.0462611	0.061923	-0.030951	0.0113451
	18	0.0675772	0.060065	-0.011035	0.0125746
	19	-0.0482603	0.060064	-0.030552	0.0109597
	20	-0.1467683	0.060945	-0.045196	0.0098047
	21	-0.0050379	0.059899	-0.023557	0.011523
	22	-0.0277409	0.063022	-0.028396	0.0117942
	23	0.1102191	0.064233	-0.004845	0.0140593
	24	0.133252	0.058431	0.002146	0.0130844
	25	-0.1715225	0.063446	-0.049226	0.0098667
	26	0.1001885	0.053014	-0.001944	0.0114565
	27	0.1862747	0.060104	0.012138	0.0142429
	28	0.1083933	0.056757	-0.001959	0.012368
	29	0.2839017	0.049285	0.038966	0.0125962
	30	0.0754371	0.059199	-0.00922	0.0124844
	31	0.173709	0.07053	0.00477	0.0164683
	32	0.0307093	0.059727	-0.017424	0.0119765

Complete Probit Regression Results with Policy/Poverty-Level Interactions (continued)

		Coefficient	Std. Error	mean value	Marginal Effect at mean value
State Indicators (Continued):					
	33	0.1509127	0.058749	0.005502	0.0134373
	34	0.2470678	0.055895	0.027365	0.0140639
	35	0.2870454	0.05679	0.036079	0.0147887
	36	0.3194649	0.055727	0.044392	0.0149641
	37	0.4481735	0.0478	0.080277	0.013568
	38	0.1553973	0.056299	0.007504	0.012962
	39	0.2418975	0.053752	0.027275	0.0134827
	40	0.0546468	0.057556	-0.012348	0.0118739
	41	0.2845769	0.056091	0.035868	0.0145795
	42	0.3275972	0.053713	0.047482	0.0145486
	43	0.3970744	0.051704	0.066069	0.0147425
	44	0.036899	0.054854	-0.014421	0.0110808
	45	0.3776306	0.055141	0.059319	0.0155825
	46	0.0252914	0.060374	-0.018609	0.0120282
	47	0.0559715	0.059064	-0.012703	0.0121908
	48	0.2941536	0.047032	0.041857	0.0115669
	49	0.3485161	0.05746	0.050746	0.0158785
	50	-0.2634079	0.0728	-0.062784	0.0099476
Year Indicators:					
	1996	(omitted)			
	1997	0.0299316	0.013822	0.000499	0.0027329
	1998	0.0780217	0.014587	0.009681	0.002983
	1999	0.0470699	0.017091	0.00256	0.0034206
	2000	0.0177758	0.018852	-0.003781	0.0036953
Constant		0.1470662	0.079		
Pseudo R-Squared			0.11		
Log-Likelihood			-62937.8		
Actual Percent Uninsured			14.08%		
Predicted % Uninsured, at Mean Values			11.45%		

*Statistically significant at the 90% confidence level
**Statistically significant at the 95% confidence level
***Statistically significant at the 99% confidence level

Standard errors are robust to heteroskedasticity and correlation between observations within the same household.
Sample Size is 174,004 observations. Estimation performed using sample weights.

APPENDIX E: LINEAR PROBABILITY MODEL RESULTS FROM CHAPTER 7

Table 27: Complete Linear Probability Model Results with Policy/Poverty-Level Interactions

	Coefficient	Std. Error
Policy Variables:		
MLim: 0-100% FPL	-0.0582113	0.008012
SDif: 0-100% FPL	-0.0392377	0.008960
Limit: 100-150% FPL	-0.0523845	0.008049
MLim: 150-200% FPL	-0.0401507	0.010980
SDif: 150-200% FPL	-0.0207082	0.009760
Mlim: 200-250% FPL	-0.0170300	0.007688
SDif: 200-250% FPL	0.0090481	0.010241
Limit: >250% FPL	0.0063278	0.003557
Demographic Variables:		
Female	-0.0011798	0.001795
White	-0.0238537	0.013321
Black	-0.0333741	0.013892
Asian	-0.0020915	0.015348
Other Race	(omitted)	
Both Parents	-0.2727235	0.010522
Mother Only	-0.3044337	0.010616
Father Only	-0.2216963	0.012184
Neither Parent	(omitted)	
# in Family	-0.0367088	0.006002
# in Family Squared	0.0032967	0.000616
Family Earnings	-0.0089203	0.000911
Family Earnings Squared	0.0002009	0.000023
Other Income	-0.0269446	0.002426
Other Income Squared	0.0015894	0.000266
0-50% FPL	0.1990002	0.014506
50-100% FPL	0.2202502	0.013579
100-150% FPL	0.2283779	0.014934
150-200% FPL	0.1624776	0.016164
200-250% FPL	0.0848829	0.012310
250-300% FPL	0.0148945	0.004694
300-350% FPL	0.0072035	0.004479
<350% FPL	(omitted)	
Age Indicators:		
Newborn	(omitted)	
Age=1	-0.0389521	0.005868
Age=2	-0.0382840	0.005783
Age=3	-0.0327585	0.005809
Age=4	-0.0308067	0.005851
Age=5	-0.0281219	0.005835
Age=6	-0.0291812	0.005901
Age=7	-0.0276191	0.005898
Age=8	-0.0299422	0.005966

Complete Linear Probability Model Results with Policy/Poverty-Level Interactions (continued)

Age Indicators (continued):	Coefficient	Std. Error
Age=9	-0.0307873	0.005913
Age=10	-0.0223496	0.005997
Age=11	-0.0199160	0.006091
Age=12	-0.0202846	0.006023
Age=13	-0.0113790	0.006166
Age=14	-0.0107138	0.006279
Age=15	-0.0076195	0.006356
Age=16	-0.0072674	0.006433
Age=17	-0.0018172	0.006417
State Indicators:		
1	(omitted)	
2	-0.0051101	0.014079
3	-0.0226085	0.015120
4	-0.0117733	0.012508
5	-0.0225176	0.014142
6	0.0081933	0.014183
7	0.0195287	0.011586
8	0.0449176	0.012677
9	-0.0271056	0.011799
10	-0.0105865	0.012142
11	0.0124754	0.014764
12	0.0133002	0.011875
13	-0.0119315	0.011818
14	-0.0284423	0.012835
15	0.0068183	0.014051
16	-0.0194466	0.013573
17	-0.0060379	0.014084
18	0.0108083	0.016238
19	-0.0079729	0.014239
20	-0.0242042	0.013199
21	-0.0010412	0.014133
22	-0.0040934	0.015254
23	0.0216637	0.014813
24	0.0261166	0.014490
25	-0.0418689	0.014881
26	0.0194397	0.013235
27	0.0369106	0.016771
28	0.0197367	0.014616
29	0.0570292	0.012332
30	0.0125135	0.015007
31	0.0415749	0.017114
32	0.0015218	0.014950

Complete Linear Probability Model Results with Policy/Poverty-Level Interactions (continued)

State Indicators (Continued):		Coefficient	Std. Error
	33	0.0281580	0.015977
	34	0.0505039	0.015929
	35	0.0625144	0.016448
	36	0.0665520	0.016094
	37	0.1041319	0.012045
	38	0.0269185	0.015517
	39	0.0467067	0.014731
	40	0.0041536	0.014919
	41	0.0546434	0.014902
	42	0.0710475	0.015867
	43	0.0923193	0.014689
	44	0.0044729	0.013913
	45	0.0815760	0.015496
	46	0.0051446	0.013946
	47	0.0093132	0.014809
	48	0.0600146	0.011417
	49	0.0678448	0.015952
	50	-0.0451711	0.016580
Year Indicators:			
	1996	(omitted)	
	1997	0.0055738	0.004127
	1998	0.0154861	0.004362
	1999	0.0086523	0.004810
	2000	0.0024220	0.005035
Constant		0.4931417	0.024677
R-Squared		0.0921	

*Statistically significant at the 90% confidence level
**Statistically significant at the 95% confidence level
***Statistically significant at the 99% confidence level

Standard errors are robust to heteroskedasticity and correlation between observations within the same household.
Sample Size is 174,004 observations. Estimation performed using sample weights.

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