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The Impact of the Medicaid Expansions of the Late 1990s on the Insurance Coverage of Poor Adolescents

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Abstract

This paper examines the effects of the Medicaid expansions of the late 1990s on the insurance coverage of poor adolescents. To control for the potentially confounding influences of competing trends, I use a difference-in-differences strategy that employs a comparison group of poor younger children who gained public insurance eligibility several years prior to their older peers. Results suggest that the introduction of Medicaid eligibility had a large impact on the insurance coverage of poor adolescents. The probability of being publicly insured at the time of survey rose substantially as a result of the expansions; both the probability of being privately insured and the probability of being uninsured decreased as a result of the expansions. The baseline specification implies that approximately 40% of the increase in public coverage came from those who were previously privately insured. The expansions also reduced the probability of spending any part of the past year without coverage and the probability of spending at least six months of the past year without coverage. Results from various robustness exercises, including a triple-difference specification, yield similar conclusions.

1. Introduction

Low-income adolescents have experienced massive increases in their public health insurance eligibility over the past decade. While the effects of earlier Medicaid expansions on the health insurance coverage of their younger peers have received careful study, much less is known about the effects of the more recent expansions on the coverage of older teens. This study builds upon existing research by identifying the impacts of the recent Medicaid expansions on the prevalence and composition of coverage among poor older adolescents. I use a natural experiment arising from the reduction of age discontinuities in Medicaid eligibility over the late 1990s to account for the confounding influence of competing secular trends.

Motivation

Understanding the effects that the Medicaid expansions had on adolescent insurance coverage is an important first step in identifying the potential of public health insurance programs to improve adolescent health. Insurance coverage is strongly associated with improved access to care and higher utilization of primary care services among adolescents. Research suggests that uninsured teenagers are five times as likely to lack a usual source of care and are twice as likely to have had no physician contact in the past year compared to insured adolescents (Newacheck et al. 1999). Uninsured adolescents are also more likely to forgo needed medical care and are less likely to have had a recent physical exam than insured adolescents (Ford, Bearman, and Moody 1999).

There are several dimensions along which primary care receipt influences adolescent health. Although the vast majority of adolescent mortality results from injury--which access primary care is unlikely to prevent—it is important to emphasize that adolescents suffer high rates of morbidity from conditions that are preventable and/or treatable by primary care. A recent study documents that 16% of adolescents ages 12-17 have a chronic condition that necessitates the intensive use of medical services (van Dyck et al. 2004). Respiratory conditions are one example of chronic conditions that are highly prevalent among teenagers. In a study of low-income children recently enrolled in a public health insurance plan, Keane et al. (1999) found that 14 percent of sample members ages 15-19 reported having had an earache or ear infection in the past 6 months; 13 percent of sample adolescents reported having had asthma symptoms in the past 6 months; 25 percent reported having had allergies; and 28 percent of them reported having had a nose or throat infection. While the younger children in the sample had appreciably higher rates of earaches and ear infections, adolescents had higher rates of asthma symptoms and allergies than the younger children, suggesting that respiratory conditions remain prevalent throughout adolescence.

Depression is an additional health concern among adolescents. The lifetime prevalence of ever having had a major depressive episode among 12-17 year olds is estimated at 14 percent, and approximately 9 percent of children in this age group experienced a major depressive episode in the past year (SAMHSA 2005). Moreover, research finds that poor children are more likely than middle- and higher-income children to suffer from depression (Goodman, Slap, and Huang 2003). Although Medicaid programs across states differ in their levels of coverage of mental health services for enrolled children, each state at a minimum includes mental health services covered by the federally-mandated Early and Periodic, Screening, Diagnostic, and Treatment Services (EPSDT) benefit.

EPSDT also covers access to family planning services; and while it does not specifically mandate the coverage of routine gynecological care and testing for sexual transmitted infections (STIs), all states have chosen to provide these benefits to adolescents enrolled in Medicaid (Gold and Sonfield 2001). Each year an estimated 3 million new cases of STIs occur among teenagers, which suggests that these services constitute an important component of adolescent health care provision (Eng and Butler 1997).

Background on the Medicaid expansions

Medicaid is the primary government funding source for the medical care of low-income children and adults. It is administered and financed jointly by the state and federal governments, with the federal financing rate ranging between 50-68% across states depending on state per-capita income (Federal Register 2005). The federal government sets rules regarding who and what must be covered as well as rules regarding which populations are eligible but not mandated to receive coverage; beyond this basic structure states are allowed considerable latitude in operating their Medicaid programs. Up through the late 1980s, the Medicaid eligibility of poor children was largely determined by their families' eligibility for cash welfare receipt. A series of legislative acts, beginning in 1989 and continuing through 1996, first expanded Medicaid eligibility to slightly higher-income children and eventually formally severed the link between eligibility for cash welfare and Medicaid.

The Omnibus Budget Reconciliation Act of 1989 (OBRA 1989) mandated states to extend eligibility to all children under the age of six living in families with incomes of less than 133% of the federal poverty level (FPL). One year later, the Omnibus Budget

Reconciliation Act of 1990 (OBRA 1990) was passed, requiring states to gradually expand Medicaid eligibility to children of all ages living in families with incomes at or below 100% FPL. Specifically, the law mandated that states extend eligibility to all children ages 6-18 years who were born after September 30, 1983; thus all 0-18 year olds in poor families would be eligible by October 1, 2002 if states did nothing but meet the federal minimums. While some states chose to exceed these minimums, the majority did not, which led to a gradual phase-in of eligibility for older children. Welfare reform, as enacted by the Personal Responsibility and Work Opportunity Act of 1996 (PRWORA), formally delinked cash welfare eligibility and Medicaid eligibility. The law set Medicaid eligibility levels for older children not yet covered under the OBRA 1990 expansions at the 1996 cash welfare thresholds of their respective states.

By 1997, the pre-period of this study, the OBRA 1990 mandates covered children through the age of 14. Only 18 states provided coverage to all poor adolescents ages 18 and under as of March 1997 and only 5 states had equal income thresholds for all children regardless of age (Morreale and English 2003). The 1996 cash welfare thresholds upon which subsequent Medicaid eligibility was based for older children ranged from 10%-85% FPL, with 29 states' maximums falling at or below 50% FPL (Morreale and English 2003). The thresholds for the states used in this study are displayed in Table 1. Alabama and Texas had the lowest thresholds among the study states (15% FPL and 17% FPL, respectively) and Wisconsin and New York had the highest thresholds (61% FPL and 62% FPL, respectively). It is worthwhile to reemphasize that as of 1997 younger children ages 1-5 were eligible at income levels up to 133% FPL (at minimum, depending on state) and children ages 6-14 were eligible at income levels up to 100% FPL (again, at minimum). I use the discontinuity in income eligibility across age groups resulting from OBRA 1990 as a natural experiment with which to identify the impact of Medicaid eligibility on the insurance coverage of older teenagers.

This sharp discontinuity in income eligibility across ages diminished in the aftermath of the Balanced Budget Act of 1997 (BBA 1997). The BBA 1997 allocated $40 billion dollars over 10 years to create the State Children's Health Insurance Program (SCHIP), a joint federal/state program targeting low-income children living in families with incomes slightly above Medicaid eligibility thresholds. By 2002, the post-period of this study and five years after the passing of BBA 1997, the modal threshold for income eligibility across states for children of all ages had risen to 200% FPL (National Governors Association 2002). Teenagers greatly benefited from the SCHIP-era efforts; by 2002 their eligibility levels had been equalized to those of younger school-age children.

Although this study focuses exclusively on a Medicaid-eligible population, it is important to recognize that the implementation of SCHIP had important consequences for Medicaid. Indeed, over 70% of the reduction in uninsurance among low-income children occurring between 1997-2005 is attributable to gains in Medicaid coverage (Dubay et al. 2007). An overriding goal of SCHIP was to reduce the rate of uninsurance among all low-income children, regardless of which program's income thresholds they satisfied. Efforts were made to streamline the enrollment process as well as facilitate coverage renewal in both Medicaid and SCHIP and states also focused considerable resources on outreach campaigns targeting eligible populations.

2. Conceptual framework

In the absence of public insurance eligibility, parents may choose to insure their children in the non-group market or, if offered, through dependent coverage on an employer- or group- sponsored plan. Collectively these plans are referred to as “private coverage” throughout this paper. Private plans typically require out-of-pocket premiums, co-pays for medical care, and deductibles. While the generosity of private plans along these three dimensions varies widely, employer-sponsored plans are on average less costly than policies purchased in the non-group market. Private insurance plans also differ in the variety of medical services that they cover. In deciding whether or not to insure their children, parents optimize over their families’ expected utility given their children’s expected medical needs, out-of-pocket costs, and the range of services provided under available private insurance options. In the absence of a publicly provided alternative, children will be uninsured if their parents decide that the expected benefit from having insurance for their children is less than the out-of-pocket premiums required to purchase such coverage.

The choice set is expanded once children become eligible for public insurance. Public and private insurance differ in several important ways. First, a typical state’s Medicaid program covers a wider array of medical services relative to a typical private insurance plan. The combination of the federal Medicaid EPSDT mandates and the choice of a majority of states to cover optional services such as dental care has created a disparity between the scope of medical services covered by Medicaid and all but the most generous private insurance plans. Indeed, Gruber and Simon (2007) note that Medicaid is “the best insurance money can’t buy!”

The costs associated with public and private coverage also differ. Public insurance requires no out-of-pocket financial costs for children made eligible for Medicaid under OBRA 1989 or OBRA 1990 (Solomon 2007). While Medicaid coverage imposes no financial costs on families, it is associated with sizeable costs along other margins. An emerging body of research documents that administrative procedures such as asset tests, the requirement of an in-person interview at enrollment, and shorter re-certification intervals are associated with decrements in the likelihood of having public coverage (Wolfe and Scrivner 2005; Bansak and Raphael 2006; Summer and Mann 2006). Another important cost arising from Medicaid enrollment is the expenditure of what can be an appreciable amount of time and effort in finding a provider who accepts Medicaid patients. A recent survey found that 15% of pediatricians are not accepting new Medicaid patients, an unwillingness that likely stems from the relatively low reimbursement rates doctors receive from Medicaid compared with other insurers (Cunningham and May 2006).

To synthesize, parents deciding between enrolling their children in public or private coverage face trade-offs between the non-monetary costs associated with public coverage and the financial costs associated with private coverage. As a result, we would expect some parents to drop their children's private insurance plans once their children become eligible for Medicaid but we would not expect a complete substitution of public coverage for private coverage. The phenomenon of substituting newly-available public coverage for existing private coverage is frequently referred to as "crowd-out."

Another important implication of the non-monetary costs associated with public insurance is that some uninsured children who become eligible for Medicaid will remain uninsured if their parents decide that the expected benefit of gaining public coverage is less than the opportunity cost of the time required to enroll in and navigate care provision under Medicaid.

3. Previous research

Studies that seek to isolate the plausibly causal effects of both the earlier (late 1980s through the early 1990s) Medicaid expansions and the more recent public insurance eligibility expansions have employed either instrumental variables techniques (IV), difference-in-differences (DD) techniques, or a combination of both. Several review papers carefully describe and synthesize the results of the earlier crowd-out literature (Dubay 1999; Davidson, Blewett, and Call 2004); below I provide an overview of the major findings.

The seminal work on crowd-out is Cutler and Gruber's (1996) study in which they use a "simulated eligibility" measure as an instrument for Medicaid eligibility. This measure is the percentage of a national sample of children eligible for Medicaid for a given state in a given year. They find that Medicaid eligibility is associated with a sizeable increase in the probability of having public coverage and statistically significant decreases in the probability of being uninsured and being privately insured. Their results suggest that between 30-40% of the gains in public coverage came from children who were previously privately insured.

Ham and Shore-Sheppard (2005) and Shore-Sheppard (2005) also use a simulated eligibility instrument to identify the effects of the earlier Medicaid expansions on the insurance coverage of children. These papers find significantly lower effects of Medicaid eligibility on both the take-up of public insurance and the crowd-out of private coverage.

Shore-Sheppard (2005) first replicates the Cutler and Gruber (1996) results and subsequently augments their models with age by year interactions. The latter models yield results of much smaller magnitude relative to the specifications without the added interactions; results from the augmented specifications imply that there is no statistically significant effect on the likelihood of being privately insured.

Several of the papers on the earlier Medicaid expansions use a DD study design (Dubay and Kenney 1996; Blumberg, Dubay, and Norton 2000; Yazici and Kaestner 2000; Card and Shore-Sheppard 2004). The treatment groups across the various studies are comprised of pre-adolescent school-age children in poor and near-poor families who were affected by the earlier Medicaid expansions. The following populations serve as Medicaid-ineligible comparison groups across the different studies: older children with similar incomes; children of the same age with higher incomes; and adult men with incomes below the poverty line. In general, these papers find that the earlier Medicaid expansions had a statistically significant impact on the probability of having public coverage; however the magnitudes of the significant effects vary from sizeable (on the order of 20 percentage points) to qualitatively small (6 percentage points). Taken as a whole, the results from these studies indicate that the expansions had at most a modest effect on the probability of having private coverage. Their results regarding the effect of the expansions on the probability of being uninsured are mixed.

No clear conclusion emerges in synthesizing the results from the literature on the earlier Medicaid expansions. The Cutler and Gruber (1996) study finds large crowd-out effects while most subsequent studies find little to no crowd-out. There are several possible reasons why the estimates from the first generation crowd-out studies vary so widely. One potential explanation is precision limitations. None of these studies report standard errors for their estimated crowd-out ratios and it is highly likely that the confidence intervals around the crowd-out estimates are large, suggesting that the estimates are not so dissimilar once power considerations are taken into account. Another possible cause of the disparate results is that Medicaid eligibility may exert heterogeneous effects across children of different ages and income categories and that these effects may have differed across various years. Heterogeneity in the effects of Medicaid eligibility would lead to different crowd-out estimates for samples that vary along the dimensions of study period, age composition, and income composition. A final explanation lies in the identification strategies employed in crowd-out studies. Any confounding related to the violation of the exclusion restriction in the IV studies and/or an inappropriate choice of comparison group in the DD studies may bias the crowd-out results in an unknown way.

In contrast to the first generation of crowd-out studies, the three existing studies on the recent expansions of the SCHIP era (post 1997) generally find appreciable amounts of crowd-out. LoSasso and Buchmueller (2004) is the first study to examine the effects of the SCHIP-era expansions on the insurance coverage of children. Using a simulated eligibility instrument, they find that public insurance eligibility is associated with a 9.1 percentage point increase in the probability of being publicly insured and that crowd-out of private insurance accounts for about 46.6% of this increase.[1]

Hudson, Selden, and Banthin (2005) estimate the impacts of recent policy changes on children's health insurance coverage using both DD and IV models. Using a

[1] An interesting aside: in direct contrast to the estimates from Shore-Sheppard (2005), the inclusion of age by year interaction terms does not influence the results found in this paper.

comparison group of higher-income children, they find that SCHIP eligibility is associated with a 8.9 percentage point increase in probability of being publicly covered ($p<0.01$), a 5 percentage point decrease in the probability of being privately covered ($p<0.05$), and a 4 percentage point decrease in the probability of being uninsured ($p<0.05$). The associated crowd-out estimate is a statistically significant 56%.

Using a variant of the simulated eligibility instrument, they find that being made eligible for public coverage is associated with a 27 percentage point increase in the likelihood of having public coverage ($p<0.01$), a 14 percentage point decrease in the likelihood of having private coverage ($p<0.01$), and an 11 percentage point decrease in the likelihood of being uninsured ($p<0.01$). Crowd-out in this specification is a statistically significant 53%. Of note is their finding that the IV results are quite sensitive to the choice of sample; specifications that exclude very high- and very low- income children yield different results than the baseline specification. Furthermore, results from a non-linear IV technique yield estimates for public insurance that are one-half the magnitude of those from the linear specification and estimates for uninsurance are fully five times higher in the linear specification than the non-linear model.

Gruber and Simon (2007) also estimate both DD and IV models to identify the effects of the recent expansions. Results from their IV analysis using a simulated eligibility instrument suggest that eligibility is associated with a statistically significant 7.2 percentage point increase in the probability of having public coverage, a statistically insignificant 1.7 percentage point decrease in having private coverage, and a marginally significant 1.5 percentage point increase in having both types of coverage. The associated crowd-out estimates range from 24-37% depending on the treatment of sample children reporting both public and private coverage; while no statistical significance tests are performed on these ratios, the confidence intervals surrounding them are likely quite large given the insignificance of the result on private coverage. In models accounting for family spillovers associated with eligibility, they find much larger effects of eligibility on private coverage, with crowd-out increasing to 61-68%.

Their DD estimates also suggest that recent expansions had a sizeable crowd-out effect. Using a treatment group of children ages 0-18 living in families with incomes between 100-200% FPL and various comparison groups of children with higher and lower incomes (<100% FPL, 200-300% FPL, 300-400% FPL), they find crowd-out ratios ranging between 58%-113%.[2]

That the results differ between studies focusing on the older versus the more recent public insurance expansions is not surprising given the changes in the policy environment that occurred between the two time periods. It is interesting that the emerging literature on the post-SCHIP era expansions (including my study) exhibits relatively consistent crowd-out results while estimates in the earlier literature vary quite widely. Although it is not possible to state with certainty why these two literatures differ, I hypothesize that the increases in the price of private insurance play a primary role. Average insurance premiums for private coverage rose 50% over the course of the 1990s, making private insurance in the post-SCHIP era a considerably less attractive option than it was a decade prior (Chernew, Cutler, and Keenan 2005).

[2] Many of the children in two of these comparison groups were actually "treated" during the study period. In some states, children living in families with incomes below 100% FPL were ineligible for public coverage in 1996; indeed all of the treatment group in my study would fall into this comparison group. Additionally, some states extended eligibility to children in families with incomes above 200% FPL.

Banthin and Selden (2003) is the one existing study that examines the effects of the Medicaid eligibility expansions on the probability of spending all or part of the past year uninsured. Their treatment group is all children ages 0-18 who became Medicaid-eligible during the earlier Medicaid expansions; as a comparison group they use higher-income children who became eligible for SCHIP. Using a regression-adjusted DD specification, they estimate that Medicaid eligibility is associated with a statistically significant 15.7 percentage point decrease in the probability of spending the entire year uninsured, a statistically significant 20.2 percentage point decrease in the probability of spending at least 4 months of the past year uninsured, and a statistically significant decrease of 17.2 percentage points in the probability of spending any part of past year uninsured.

My work builds upon the current literature in several ways. Neither the older studies nor the more recent studies can speak to the effects of Medicaid eligibility on the insurance coverage of poor teenagers. Both genres of studies use samples comprised of children of varying income groups and ages. Identification from IV studies in the older literature is largely driven off of poor younger children who were granted eligibility in the late 1980s and early 1990s, while identification in the studies from later years is largely driven off of higher-income children of all ages who were made eligible for SCHIP. Similarly, the treatment groups defined in the more recent DD studies are comprised of higher-income children made eligible for SCHIP. None of these results can speak to the effects of Medicaid eligibility on the many poor teenagers who were gradually phased into Medicaid under OBRA 1990.

Furthermore, in a departure from existing DD studies, I provide heuristic evidence that in the absence of the intervention of interest the chosen comparison and treatment groups would have experienced similar trends in their insurance coverage. I am also able to estimate a triple-difference model, relaxing the parallel trends assumption inherent in the DD study design.

4. Data, measures, and methods

Data

The data are from the National Survey of America's Families (NSAF). The NSAF is a nationally representative sample of the civilian, non-institutionalized population under the age of 65. It is comprised of three rounds of cross-sectional data collected in 1997, 1999, and 2002; the pooled cross-section includes information on over 100,000 children. This data effort was designed and executed by the Urban Institute with the goal of tracking the economic and social well-being of families and children in the wake of welfare reform (Abi-Habib, Safir, and Triplett 2002a; Abi-Habib, Safir, and Triplett 2002b). Low-income families with children constituted the population of primary interest; they were oversampled in all three rounds of the NSAF. Survey content includes information on the following: household composition and demographics; public program participation; employment, income, and earnings; measures of economic hardship and poverty status; child support receipt; child care; health care utilization, insurance coverage, and access to care; and various social dimensions of well-being.

The devolution of welfare programs from the federal to the state level was an integral part of welfare reform, consequently the NSAF was designed to provide estimates that are representative at the state level for 13 "focal states". These focal states are: Alabama, California, Colorado, Florida, Massachusetts, Michigan, Minnesota, Mississippi, New Jersey, New York, Texas, Washington, and Wisconsin. Over half of the U.S. population lives in the focal states; they were chosen for their variation with respect to geography, population size, and attitudes and traditions regarding welfare systems. The focal states account for 85% of the observations in the data and the sample used for this study includes only observations from these states.

Each household had a maximum of two children included in the survey: one under the age of 6, and the other between 6 and 17. If a household had two or more children under the age of 6, only one of them was (randomly) chosen for inclusion in the study; analogously, if a household had two or more children between the ages of 6 and 17, only one of them was chosen for the sample. The interviewer asked to speak to the adult who was most knowledgeable about the sample child's education and health care; this adult (called the "most knowledgeable adult" or MKA) responded to all questions regarding the sample child.

Measures

To place this work in context with the existing literature, I use point-in-time measures of insurance coverage as three of the outcome variables of interest. The specific point-in-time measures are whether a child has public insurance coverage, private insurance coverage, or no insurance coverage at the time of survey. Dual public/private enrollees constitute approximately 3% of the sample in both the pre- and post- periods. Following the coding scheme used in the NSAF, I characterize these children as having private coverage. Children with Medicare coverage account for less than 1% of the total number of child observations in the NSAF and are not included in the sample.

The length of the past year spent uninsured serves as an additional outcome of interest in this study. The NSAF asks respondents to report the number of months that they spent without coverage during the past 12 months. Using this measure, I create three outcome variables that capture the dynamics of insurance coverage over the past year: spending any part of the past 12 months without coverage, spending at least six months without coverage, and spending the entire year without coverage.

Focusing on the length of time spent uninsured is a particularly relevant exercise since research suggests that the duration of uninsured spells matters with respect to health care access (eg. Olson, Tang, and Newacheck 2005) and that even short spells without coverage are associated with decrements in the likelihood of utilizing care (Aiken, Freed, and Davis 2004). Most of the literature examining the effects of public insurance expansions focuses on the crowd-out of private coverage. While changes in the composition of private and public coverage certainly have important public finance implications, recent research has shown that what matters for health care utilization and access outcomes is having any kind of coverage (Selden and Hudson 2006). Thus shifting the focus from the effects of the expansions on static measures of public versus private coverage towards their effects on the presence and length of uninsured spells provides more relevant outcome measures for analysts concerned with the potential for public insurance expansions to impact health care utilization.

Methods

The equation of interest for the sample of older adolescents gaining eligibility is:

$$Coverage_{it} = \alpha + \beta_P * PostPeriod_t + \phi X + \varepsilon_{it} \qquad (1)$$

“Coverage” represents the vector of dependent variables discussed above. Time is indexed by the letter *t* and individuals by the letter *i*.

The coefficient on the post-period dummy, β_P, represents the main estimate of interest: the effect of the eligibility expansions on insurance coverage. The vector *X* represents a variety of control variables. The concern with equation (1) is that β_P may not be a clean estimate of the effect of the intervention but rather a combination of the effects of the intervention and other unobserved intervening mechanisms that occurred between the pre- and post- periods.

There are several potentially confounding trends that occurred during the study period. Research has found that the 1996 welfare reform legislation influenced the insurance coverage of poor children, likely due to confusion regarding eligibility in the aftermath of the decoupling of the Medicaid and cash welfare programs (eg. Kaestner and Kaushal 2003; Cawley, Schroeder, and Simon 2006). In addition, several states expanded public insurance eligibility to low-income parents during the time period of study. These expansions may have had important spillover effects on the coverage of poor children (Dubay and Kenney 2003; Sommers 2006). An additional concern is that the increase in the price of employer-sponsored coverage over the study period likely played a primary role in driving coverage trends (Chernew, Cutler, and Keenan 2005). The presence of these and other potential confounders—which may be unobservable and therefore impossible to model—necessitates the use of quasi-experimental methods in the attempt to isolate true program effects from the effects of competing trends.

To account for these potentially confounding unobserved influences, it is useful to compare the changes over time for the group of interest to the changes over time for a control group (also referred to in this paper as a “comparison group”). If the unobserved variables affect the control group in a parallel fashion to the treatment group, comparing the differences across the two leaves a plausibly unbiased estimate of the effect of the intervention. This “parallel trends” assumption is the key identifying assumption for this estimation strategy.

As mentioned above, I use discontinuities by age in the Medicaid eligibility rules to construct a comparison group for the analysis. By 1997—the pre-period for this study—each state was required by federal law to extend eligibility to all children through the age of 14 in families with incomes at or below 100% FPL. Motivated by the combination of federal Medicaid requirements and newly available SCHIP federal funds, all of the expansion states drastically increased the income thresholds for adolescents by 2002.

The treatment group is comprised of adolescents in families with incomes between 50-100% FPL living in expansion states. It is important to note that the definition of treatment status is imprecise and it is likely that there is some misclassification of eligible adolescents as ineligible in the pre-period. One potential source of misclassification is the categorical nature of the income variable in the NSAF. Income below the poverty level is categorized into two bins: having family income between 0-50% FPL and having family income between 50-100% FPL. The eligibility cut-offs for two of the expansion states in the study (New York and Wisconsin) are slightly above the 50% threshold, therefore some of the older adolescents in the treatment group may have been eligible in these two states in the pre-period. Misclassification of this nature would likely (conservatively) bias the results towards zero since some treatment group members were eligible prior to the study period.

The control group is children ages 6-13 in families with incomes between 50-100% FPL living in these same states. The younger children in the control group were eligible for Medicaid in both the pre- and post- periods while the older adolescents in the treatment group became eligible for Medicaid during the time period of study.

It is impossible to know whether or not the treatment and control groups would have experienced similar trends in coverage absent the intervention (i.e. the counterfactual); however, it is possible to provide some suggestive evidence regarding the similarity of the two groups. I have constructed a heuristic test of the parallel trends assumption with data available from the NSAF. Figures 1a-1c plot the trends in insurance coverage for children living in families with incomes between 50-100% of the FPL over the time period of study for the four NSAF focal states that had extended eligibility to poor older adolescents prior to 1997.[3] Children of all ages were eligible for public coverage by 1997 in these states; therefore the plots illustrate whether influences unrelated to public insurance expansions exerted similar effects on the insurance coverage of poor teenagers and younger school-age children over the time period of interest.

The trends in coverage among poor children ages 6-13 and poor teenagers ages 15-17 year olds in the states with prior expansions are similar, substantiating the assumption that younger school-age children are an appropriate population to use as a comparison group for older adolescents. I formally test the hypothesis that the children in the two age groups had similar insurance trends over the study period using a regression framework. The results are reported in Table A1. For no insurance measure do I reject the null that the two age groups exhibited similar trends; however it is important to mention that these tests may be underpowered.

[3] These four states are: Massachusetts, Michigan, Minnesota, and Washington.

The unadjusted “difference-in-differences” (DD) estimator is:

$$\left(Ins_{02} - Ins_{97}\right)_{Teen} - \left(Ins_{02} - Ins_{97}\right)_{YoungChild} \tag{2}$$

Ins represents the insurance coverage variable of interest. *Teen* represents being in the treatment group and *YoungChild* is an indicator reflecting membership in the comparison group. The regression-adjusted DD estimates are computed as follows:

$$Ins_{it} = \alpha + \beta_1 Treatment_i + \beta_2 PostPeriod_t + \beta_3 Treatment_i * PostPeriod_t + \phi X + \varepsilon_{it} \tag{3}$$

In this specification, β_3, the coefficient on the interaction term between being in the treatment group and the post-period dummy, is the estimate of the effect of the intervention on the treatment group. The vector *X* represents the following covariates: sex; age; race; health status; presence of a limiting condition; immigrant status; parental education; parental age; presence of a full-time worker in the household; family structure; average family contribution for health insurance in a child’s state during the year of survey; percent of all firms in the child’s state that offer health insurance during the year of survey; state unemployment rate during the year of survey; and state dummies.

There are several potential concerns with using a comparison group that is comprised of younger children than the treatment group. Ham and Shore-Sheppard (2005) explain that younger children may be less likely to be privately insured than older children because their parents are younger on average than those of older children and are therefore less likely to be working at a job that offers health insurance. Blumberg, Dubay, and Norton (2000) argue that parents may be more likely to enroll younger children in Medicaid than their older peers because younger children use health care services more frequently. Both of these arguments suggest that changes in public insurance eligibility may exert differential effects among children of different ages. As noted earlier, Shore-

Sheppard (2005) finds that her IV specifications are sensitive to the inclusion of age by year interactions, which suggests that the parallel trends assumption may not hold when making comparisons across children of different ages. While the graphs in figures 1a-1c suggest provide reassurance that any differences across the relevant age groups are not a serious concern over the time period of this study, it remains important to provide further evidence that the results are robust to this assumption.

To address this concern, I estimate a triple-difference (DDD) model that utilizes the differences in the timing of the eligibility expansions across states.[4] The NSAF focal states that had extended eligibility to poor children of all ages by 1997 include: Massachusetts, Michigan, Minnesota, and Washington. The DDD estimator compares the changes in insurance coverage between poor teenagers and their younger counterparts across states that expanded Medicaid eligibility before the study period and those that expanded eligibility during the study period. Adding this third dimension of comparison helps alleviate the concern that teenagers and young children may have had differential coverage trends absent the intervention.

The unadjusted DDD estimator is calculated in the following manner:

$$\left[\left(Ins_{02} - Ins_{97}\right)_{Teen} - \left(Ins_{02} - Ins_{97}\right)_{YoungChild}\right]^{Exp=1} -$$

$$\left[\left(Ins_{02} - Ins_{97}\right)_{Teen} - \left(Ins_{02} - Ins_{97}\right)_{YoungChild}\right]^{Exp=0} \qquad (4)$$

Exp is an indicator variable representing living in a state that expanded Medicaid eligibility to adolescents during the study period (as opposed to before the study period). Similar to the DD case, a covariate-adjusted triple-difference estimate (DDD) can be computed in a regression framework:

[4] For an eloquent description of triple-difference models see Sarin (2004).

$$Ins_{ist} = \alpha_{ist} + \beta_1 \left(Teen_i * Exp_s * Post_t \right) + \zeta D + \phi X + \varepsilon_{ist} \quad (5)$$

The coefficient on the three-way interaction, β_1, is the estimate of interest. It measures the change over time in the difference between the insurance coverage of teenagers and young children in the expansion states minus the same measure in the states that had expanded eligibility to adolescents prior the study period. The vector *D* represents all of the main effects (a *Teen* dummy, an *Exp* dummy, and a *Post* dummy*)* as well as the two-way interactions between the main effects.

Linear and non-linear models are estimated and the results from both are reported. The multinomial logit specification is used as the non-linear method of estimation for the point-in-time coverage measures while the probit specification is used for the dynamic measures. Incremental effects are computed in the non-linear models by keeping observations for poor adolescents in the pre-period and comparing the probability of the outcome measure when the interaction term ($Treatment_i * PostPeriod_t$) is one versus when it is zero. The same calculation was performed using the alternate subsample of poor adolescents in the post-period; the results are quite similar.

All models are run using probability weights. Standard errors in the non-linear models are computed using a normal theory approximation on a 1,000 replicate weighted bootstrap procedure in which the replicates are drawn by state-year clusters. No bias corrections were made since the differences in the bootstrap estimates and the estimates obtained on the original sample are minimal across the various models. Standard errors are clustered at the state-year level to account for the non-independence of observations at this unit of aggregation.[5] All analyses were performed in Stata 9.

5. Results

Descriptive statistics

As shown in Table 1, the subset of focal states that witnessed expansions in adolescent Medicaid eligibility during the study period are: Alabama, Colorado, Florida, Mississippi, New Jersey, New York, Texas, and Wisconsin.[6] In these states there are 384 treatment group members in 1997 and 205 treatment group members in 2002. The control group is comprised of 1,076 observations in 1997 and 636 observations in 2002. Not included in these counts are the 30 observations for which data on MKA education and/or family structure is missing (original sample=2,331 observations; analytic sample=2,301 observations). I have estimated the unadjusted DD estimator for each dependent variable with and without the observations with missing data and, reassuringly, the results are very similar across the two sets of models. Table 2 displays the demographic characteristics of the treatment and control groups. All descriptive statistics are weighted to correct for the complex survey design of the NSAF .

Treatment and control group children are similar along the measures of health status. Approximately 14% of treatment group children are reported to be in fair or poor health; the analogous figure for comparison group children is 13%. Seventeen percent of the treatment group has a condition that limits regular activities and 16% of the comparison group has such a condition. An examination of the family-level descriptive

[5] Given the concerns raised in Primo, Jacobsmeir, and Milyo (2006) regarding computing clustered standard errors when the number of cluster is less than 50, I also estimate the baseline specification with no cluster correction and with a cluster correction at the state-year-age level. The results are robust across these specifications.

[6] California is not included since its income cutoff in 1997 was 81% of the FPL.

statistics suggests that treatment group members are more disadvantaged than control group members along several dimensions, most notably parental education and the presence of a worker in the household.[7] Almost 38% of treatment group children live in a household without a full-time, full-year worker compared to 30% of control group children. Twenty-six percent of children in the control group and 39% of children in the treatment group live in households in which the reporting adult (MKA) lacks a high school degree. The treatment group is also comprised of more immigrants and children of other (non-white, non-black) races and they are more likely to live with a single parent. All of these family-level variables are potential predictors of insurance coverage and raise a concern about the appropriateness of the comparison group.

It is worthwhile to re-state the underlying assumption of the identification strategy: absent the intervention the *trends* in insurance coverage of the treatment and control group members would have been equal. The identification strategy does not rely upon the equivalence of the *levels* of the two groups. The research design is valid as long as the underlying differences between the treatment and control group members affect only the levels of coverage, not the trends. As previously mentioned, this is an assumption that cannot be definitively tested and is an important caveat of all DD studies.

Table 3 displays the distribution of insurance coverage among treatment and control group members in the pre- and post- periods. Approximately 22% of treatment group members had public coverage at the time of survey in 1997.[8] By 2002, fully 60%

[7] Note that these findings suggest that the concern raised in Ham and Shore-Sheppard (2005) about the use of older children as a comparison group is not applicable for this sample. Treatment group parents are not more likely than their control group counterparts to be employed (and therefore more likely to be offered employer-sponsored coverage); in fact the opposite is true.

[8] A sizeable percentage of children had public coverage in the pre-period even though their reported family income was above the income threshold. This is a common result in the literature: Currie and Gruber

of treatment group members were publicly covered at the time of survey. The comparison group also experienced an increase in public coverage over the study period, although the increase from 47% to 57% is much smaller than the increase seen for treatment group members. Private insurance decreased for both groups; in keeping with the trend for public coverage the decrease in private insurance was more marked for treatment group members (32% in 1997 vs. 16% in 2002) than their younger peers (26% in 1997 vs. 22% in 2002). Approximately 45% of the treatment group was uninsured at the time of survey in 1997; this number had fallen to 23% by 2002. A smaller decline in the percent uninsured is seen for the comparison group, with the 1997 figure of 27% dropping to 21% in 2002.

A majority of treatment group adolescents (55%) had spent at least one month in the previous year uninsured in the pre-period, a figure that is more than twenty percentage points higher than the corresponding proportion of control group children (33%). In 2002, the percent of treatment group members who spent at least one month in the previous year uninsured dropped to 26%; this result is especially notable when compared with the one percentage drop experienced by the control group. The treatment group experienced similarly large drops in spending at least six months of the previous year without coverage (50% in 1997 to 24% in 2002) and spending the entire year without coverage (40% in 1997 and 21% in 2002), while the control group had much more modest decreases for both coverage outcomes (a drop of four percentage points for spending at least six months without coverage and a drop of one percentage point for spending the entire year without coverage). It is striking that the large gap in the

(1996); Cutler and Gruber (1996); Yacizi (1997); and Hudson, Selden, and Banthin (2005) all find that approximately 20% of seemingly ineligible sample members report having Medicaid coverage.

uninsured rates between the treatment and control groups in 1997 was completely closed--and for some coverage measures reversed--by 2002.

Regression results—baseline specification

The results of interest for the point-in-time outcomes are displayed in Table 4. Multivariate regression estimates for all of the covariates in the model are listed in Table A2. In the linear specification, becoming eligible for public insurance is associated with a 24 percentage point increase in the probability of being publicly insured at survey; this result is significant at the p<0.01 level. The estimated effect on private coverage is a statistically significant decrease of 10 percentage points. The likelihood of being uninsured at survey also decreased as a result of the expansions; the results suggest that there was a 14 percentage point decrease in the probability of being uninsured (p<0.05). Results from non-linear specifications, which are displayed in column 2 of the table, are similar to those derived from linear probability models.

Table 5 contains the results from the models estimating the effects of the eligibility expansions on dynamic measures of insurance coverage. Both the linear probability and the probit specifications yield negative and statistically significant impacts on the likelihood of spending any part of the past year without coverage. In the linear framework, becoming eligible for public insurance is associated with a 26 percentage point decrease in the likelihood of spending any part of the year without coverage. The estimated impact of the expansions on spending at least six months of the past year without coverage is also negative and statistically significant; becoming eligible for public insurance is associated with a 19 percentage point decrease in the likelihood of being uninsured for six or more months (p<0.01) in the linear model. Regression results suggest that the eligibility expansions exerted a negative effect on the probability of being uninsured for the entirety of the past year, however these results are not significant at conventional levels of statistical significance for either the linear or the non-linear specifications.

Sensitivity tests

The estimates from specifications with limited control variables are displayed in Table A3. The results are relatively robust to the set of control variables employed in the regressions. The estimates are also robust to the exclusion of the 3% of sample children who reported having both public and private coverage at the time of survey (results not shown).

The DDD results (Tables 4 & Table 5) are comparable to the results from the baseline specification, however there are some differences in the magnitudes of the point estimates from the two models. The estimates for being publicly covered are higher in magnitude for the DDD model, with a difference of five percentage points in the linear case and two percentage points in the non-linear case. In both the linear and non-linear specifications, the estimates for the DDD models suggest a larger effect on private insurance. For example, the estimated impact from the linear baseline specification is a 10 percentage point decrease in the likelihood of being privately covered at survey, while the linear DDD model yields an estimate of a 16 percentage point decrease. The non-linear results from these two specifications are similar to their linear counterparts. The non-linear results for the probability of being uninsured at survey differ across the DDD and the baseline specifications, with the DDD model yielding an estimated impact of a decrease of 11 percentage points (not significant) and the baseline specification yielding an estimated impact of a decrease of 15 percentage points ($p<0.05$).

As can be seen in Table 5, the estimated impact of the expansions on the probability of being uninsured at any point in the past year is quite robust. Linear and non-linear results from the baseline specification look similar to those from the DDD specification. Triple-difference estimates of the probability of spending at least six months of the past year without coverage are smaller than those from the baseline specification; the estimate from the linear DDD model is negative 12 percentage points ($p<0.05$) compared to negative 19 percentage points ($p<0.01$) in the baseline specification and the estimate from the non-linear DDD model is negative 16 percentage points (not significant) compared to negative 20 percentage points ($p<0.01$). Estimates from the alternate specifications are similar to those from the baseline specification for the probability of being uninsured for the entire year; and while all of these point estimates suggest a negative association between eligibility and this outcome it is important to view these results as merely suggestive since only one point estimate is statistically significant.

Limitations

Several limitations warrant careful consideration. A potential problem with using younger children in the NSAF as a control group for older children is the possibility that some of the control group children have older siblings who were affected by the expansions. The NSAF samples one child age 0-5 and one child age 6-17 within each sample household. It is impossible to determine from the NSAF data whether the 6-10 year olds in the comparison group have siblings ages 15-17 due to the nature of the survey design. Ham and Shore-Sheppard (2005) find that sibling eligibility exerts a qualitatively small but statistically significant impact on the likelihood of public insurance take-up (but not private insurance coverage) for a child.[9] Therefore it is possible that some of the comparison group members may have experienced spillover effects in coverage from the expansions targeted at their older siblings. The presence of such spillovers would decrease the differences in impacts between the comparison and treatment groups, biasing the results towards zero. Similarly, Cutler and Gruber (1996) and Gruber and Simon (2007) argue that eligibility expansions aimed at children may induce parents to drop their own private coverage once all of their children become eligible for public coverage. If this is the case, then crowd-out measured at the family level will be higher than crowd-out measured at the child-level.

Another limitation of my study design is that family income may be endogenous with respect to Medicaid eligibility. Both Gruber and Simon (2007) and Ham and Shore-Sheppard (2005) argue that this compromises DD study designs in the context of the Medicaid expansions. While neither paper provides detail regarding the nature of the potential bias, I posit that their concern regards the following. It is possible that prior to the expansions, parents of older teenagers with high expected medical expenditures chose to keep their income levels below the Medicaid income eligibility thresholds. After eligibility was extended to higher income levels, parents of such children were able to increase their incomes without compromising their children's public health insurance eligibility. This behavior would likely have the following effect: older adolescents in families with incomes between 50-100% FPL would look healthier in the pre-period and would likely have lower levels of insurance coverage than adolescents in families with the same income level in the post-period. Increases in insurance coverage over the time

[9] However, Blumberg, Dubay, and Norton (2000) find no evidence of sibling spillovers in their analysis.

period may be the result of a shift in the composition of adolescents in the 50-100% FPL income range from a population with weaker insurance preferences in the pre-period to one with stronger preferences in the post-period. If this is indeed the case, then any impacts attributed to eligibility alone will be overstated.

Even though I cannot rule out the possibility that income endogeneity is biasing my results, I do perform several tests that suggest that the severity of the problem is minimal. First, I test whether older adolescents in the targeted income range in 1997 report similar levels of poor health and limiting conditions as older adolescents in the same income range in 2002. If income endogeneity were pervasive, I would expect the former group to be in better reported health and/or to have fewer limiting conditions. This is not the case: poor older adolescents in the two cross-sections have statistically identical rates of poor health and limiting conditions. I also estimate the baseline DD specification and the DDD specification on the subsample of children who are in good health and have no limiting conditions. While precision losses due to this sample filter preclude definitive statements, the results from these models are qualitatively similar to those from the baseline specifications including all children (results not shown).

Finally, the most important limitation of this study is shared with all DD studies: the parallel trends assumption cannot be definitively tested. While it is reasonable to hypothesize that the control and treatment groups are appropriately similar, it remains possible that secular trends for the treatment and control groups would have diverged even absent the intervention.

6. Discussion

This study provides new evidence regarding the effects of recent public insurance expansions on the insurance coverage of poor adolescents. I find that the Medicaid eligibility expansions of the late 1990s had a profound impact on insurance coverage among the population of interest, increasing the likelihood of poor teenagers having any insurance coverage as well as changing the composition of coverage held by this group. Results from the baseline specification suggest that the expansions were responsible for a 14 percentage point decrease in the probability of being uninsured at survey, which represents a 30% decrease from the pre-period uninsured rate of 45%. Extending Medicaid eligibility to poor teenagers also greatly reduced both the likelihood of them spending any part of the year uninsured and the likelihood of them spending more than six months of the year without coverage.

I also find that the take-up of public coverage was split between those who were privately insured and those who were uninsured prior to the eligibility expansions. The expansions induced an appreciable amount of substitution away from private coverage towards public coverage, implying that a sizeable component of the overall cost of the expansions was the funding of coverage for adolescents who would have otherwise been privately covered. The baseline specification yields a crowd-out ratio of 42%. An important caveat of this finding is that the crowd-out ratio—a ratio of two separate regression coefficients--is an imprecise measure. Using a 1,000 replicate bootstrap, I have created a 95% confidence interval for the crowd-out ratio yielded from the baseline specification; the lower bound is 87% and the higher bound is negative 4% (implying an *increase* in private insurance relative to public coverage).

While crowd-out certainly has implications for the cost of public insurance programs, it is important to balance cost concerns with the recognition that public and private coverage are not equivalent in their benefit coverage. This is an especially important consideration for policymakers and researchers studying teenagers, a group that has a relatively high prevalence of mental health issues. As mentioned above, mental health services covered by Medicaid vary across states; however, in general they are considerably more comprehensive than those of private insurers (Glied and Cuellar 2003). This is confirmed by a recent study that finds that 45% of publicly covered poor 6-17 year olds with emotional or behavioral problems receive mental health services, while only 18% of their privately covered peers receive such services (Howell 2004).

Medicaid is also more generous than private insurers in its coverage of preventive services (Lewit, Bennett, and Behrman 2003). A recent study on low-income adolescents participating in Florida's SCHIP program found that the receipt of a preventive care visit is associated with a higher likelihood of receiving counseling regarding risk behaviors, including counseling regarding sexual activity, suggesting that preventive care has a potentially important role in adolescent health (Shenkman, Youngblade, and Nackashi 2003).

A further consideration in the crowd-out debate is the financial benefit that low-income families receive when they substitute public coverage for private coverage. The provision of publicly funded coverage reduces the share of disposable income that families must spend to obtain health insurance. Given that the average premium for privately provided family coverage is nearing $3,000 (Kaiser Family Foundation and

Health Research and Educational Trust 2006), the magnitude of this transfer has serious financial consequences for low-income families.

As a final note, it is important to highlight that eligibility alone did not lead to near-universal coverage of the population of interest. Over 20 percent of sample children—all of whom were eligible for public coverage--were uninsured at the time of the 2002 survey. Interestingly, roughly one-quarter of these children had public coverage at some point over the prior year. Achieving future gains in insurance coverage will require strategies aimed at retaining these children who drop out of Medicaid as well as increasing outreach to those who have never enrolled.

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Tables and figures

Table 1. Public insurance eligibility cutoffs as a percentage of the FPL

Adolescents ages 15-17 in expansion states

State	1997 cutoff	2002 cutoff
Alabama	15%	200%
Colorado	39%	185%
Florida	28%	200%
Mississippi	34%	200%
New Jersey	41%	350%
New York	61%	250%
Texas	17%	200%
Wisconsin	62%	200%

Table 2. Individual- and family- level descriptive statistics

Variable	Treatment	Control	Pooled
Female	0.471	0.485	0.482
	(0.034)	(0.021)	(0.018)
Age ***	15.965	9.285	10.846
	(0.054)	(0.092)	(0.125)
Black	0.277	0.305	0.298
	(0.026)	(0.021)	(0.017)
Hispanic	0.358	0.359	0.359
	(0.032)	(0.019)	(0.017)
White	0.296	0.303	0.301
	(0.029)	(0.018)	(0.016)
Other race ***	0.067	0.033	0.041
	(0.023)	(0.008)	(0.008)
Fair or poor health	0.138	0.132	0.133
	(0.024)	(0.017)	(0.014)
Limiting condition	0.171	0.155	0.159
	(0.025)	(0.014)	(0.012)
Immigrant ***	0.189	0.120	0.136
	(0.030)	(0.014)	(0.013)
MKA < HS degree ***	0.394	0.261	0.292
	(0.035)	(0.017)	(0.016)
MKA has HS degree ***	0.533	0.667	0.635
	(0.035)	(0.019)	(0.018)
MKA has college +	0.073	0.073	0.073
	(0.021)	(0.009)	(0.008)
MKA age ***	41.800	36.435	37.689
	(0.533)	(0.311)	(0.281)
Does not live w/2 parents ***	0.653	0.577	0.595
	(0.035)	(0.022)	(0.018)
At least 1 worker in HH ***	0.613	0.692	0.674
	(0.034)	(0.021)	(0.018)
No worker in HH ***	0.377	0.298	0.317
	(0.034)	(0.021)	(0.018)
Missing worker in HH info	0.010	0.010	0.010
	(0.006)	(0.003)	(0.002)
N	*589*	*1,712*	*2,301*

Notes:

* denotes treatment and control differ at 10% level

** denotes treatment and control differ at the 5% level

***denotes treatment and control differ at the 1% level

Standard errors in parentheses.

Figures adjusted to account for the complex survey design of the NSAF.

Table 3. Insurance coverage pre- and post- expansions: Unadjusted proportions

	Treatment (n=589)	Control (n=1,712)
% publicly insured at time of survey, 1997	0.224	0.469
	(0.036)	(0.022)
% publicly insured at time of survey, 2002	0.604	0.568
	(0.056)	(0.029)
% privately insured at time of survey, 1997	0.324	0.260
	(0.041)	(0.020)
% privately insured at time of survey, 2002	0.162	0.220
	(0.034)	(0.026)
% uninsured at time of survey, 1997	0.453	0.271
	(0.045)	(0.020)
% uninsured at time of survey, 2002	0.234	0.212
	(0.048)	(0.025)
% spent any part of past yr unins., 1997	0.545	0.328
	(0.043)	(0.021)
% spent any part of past yr unins., 2002	0.255	0.318
	(0.049)	(0.029)
% spent > 6 mths. of past yr unins., 1997	0.496	0.282
	(0.044)	(0.020)
% spent > 6 mths. of past yr unins., 2002	0.242	0.242
	(0.049)	(0.028)
% spent entire year uninsured, 1997	0.395	0.196
	(0.046)	(0.018)
% spent entire year uninsured, 2002	0.205	0.193
	(0.046)	(0.022)

Notes:
Standard errors in parentheses.
All reported statistics are adjusted to account for the complex survey design of the NSAF.

Table 4. Regression results, point-in-time measures

Treatment group: 15-17 year olds living in families w/incomes b/t 50-100% FPL in expansion states
Control group: 6-13 year olds living in families w/incomes b/t 50-100% FPL in expansion states

Dependent variable	DD LPM		DD MN logit		DDD LPM		DDD MN logit	
Publicly insured at survey	0.239	***	0.251	***	0.289	***	0.270	**
	(.156, .322)		(.163, .339)		(.099, .479)		(.026, .514)	
Privately insured at survey	-0.100	**	-0.106	*	-0.155	*	-0.164	**
	(-.195, -.005)		(-.223, .011)		(-.325, .016)		(-.321, -.007)	
Uninsured at survey	-0.139	**	-0.145	**	-0.134	*	-0.105	
	(-.261, -.017)		(-.273, -.017)		(-.287, .019)		(-.396, .186)	
N	*2,301*		*2,301*		*3,203*		*3,203*	

*** p < 0.01 ** p < 0.05 * p< 0.10

Notes:
95% confidence intervals in parentheses. All models account for clustering at the state-year level and were estimated using probability weights. Confidence intervals for the multinomial logit results were calculated from bootstrap estimates using the normal approximation.

Control variables included in all models are: bad health dummy, presence of a limiting condition dummy, immigrant status, dummies reflecting education of the parental respondent of the child, age of the parental respondent, race dummies, presence of a worker in the hh dummy, missing information on number of workers in the hh dummy, sex, age, a dummy reflecting living in a non-two parent family hh, the avg. family contribution for health insurance in the child's state during the year of survey, the % of all firms in the child's state that offer health insurance during the year of survey, the state unemp. rate in the child's state during the year of survey, and state dummies.

Table 5. Regression results, dynamic measures

Treatment group: 15-17 year olds living in families w/incomes b/t 50-100% FPL in expansion states
Control group: 6-13 year olds living in families w/incomes b/t 50-100% FPL in expansion states

Dependent variable	DD LPM	DD Probit	DDD LPM	DDD Probit
Ever uninsured in past year	-0.259 **	-0.268 ***	-0.204 ***	-0.225 ***
	(-.349, -.168)	(-.359, -.177)	(-.338, -.070)	(-.389, -.061)
Uninsured > 6 months in past year	-0.190 ***	-0.204 ***	-0.122 **	-0.159
	(-.285, -.094)	(-.304, -.103)	(-.240, -.004)	(-.585, .267)
Uninsured all of past year	-0.112	-0.118	-0.120	-0.108
	(-.253, .029)	(-.267, .031)	(-.302, .063)	(-.577, .360)
N	*2,301*	*2,301*	*3,203*	*3,203*

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Notes:
95% confidence intervals in parentheses. All models account for clustering at the state-year level and were estimated using probability weights. Confidence intervals for the probit results were calculated from bootstrap estimates using the normal approximation.

Control variables included in all models are: bad health dummy, presence of a limiting condition dummy, immigrant status, dummies reflecting education of the parental respondent of the child, age of the parental respondent, race dummies, presence of a worker in the hh dummy, missing information on number of workers in the hh dummy, sex, age, a dummy reflecting living in a non-two parent family hh , the average family contribution for health insurance in the child's state during the year of survey, the percent of all firms in the child's state that offer health insurance during the year of survey, the state unemployment rate in the child's state during the year of survey, and state dummies.

Figure 1a. Trends in public coverage in states with prior Medicaid expansions

Figure 1b. Trends in private coverage in states with prior Medicaid expansions

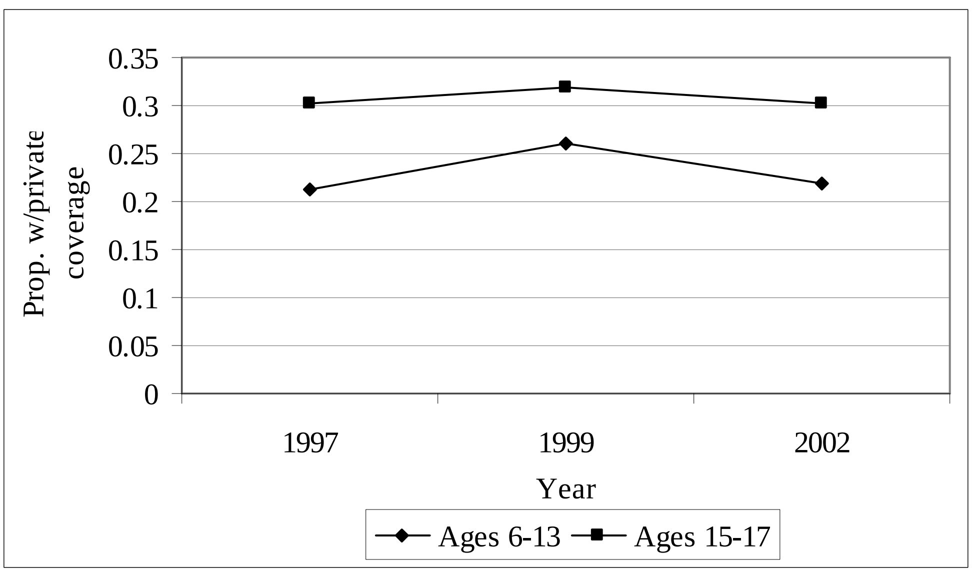
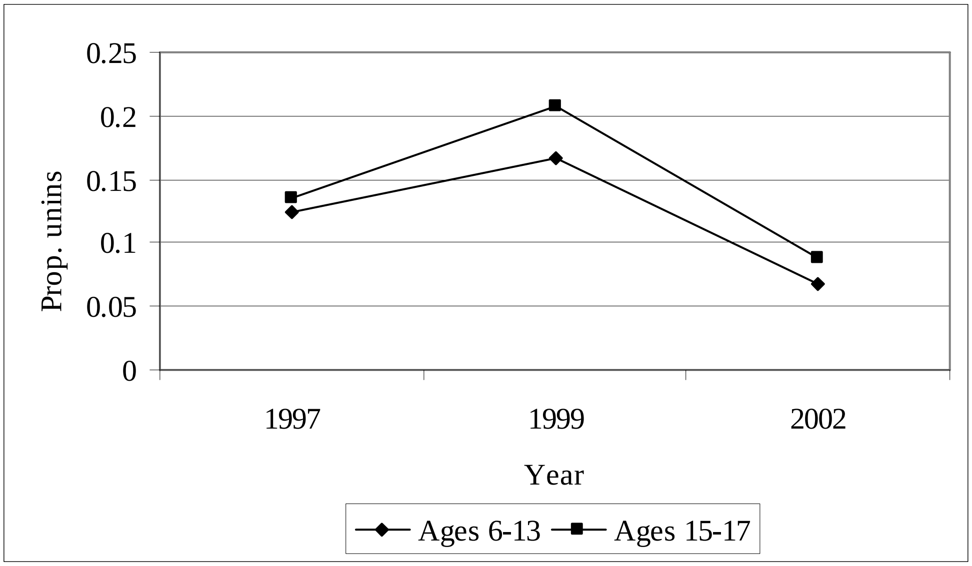


Figure 1c. Trends in uninsurance in states with prior Medicaid expansions



Appendix tables

Table A1. Regression tests of parallel trends assumption

P-value: test of joint significance of age* time interactions

Testing the trends in coverage over the study period (1997-2002) for teens (15-17) and younger children (6-13) in states with earlier (pre-1997) Medicaid expansions for teenagers

Dependent variable	P-value
Publicly insured at survey	**$p < 0.999$**
Privately insured at survey	**$p < 0.930$**
Uninsured at survey	**$p < 0.952$**
Ever uninsured in past year	**$p < 0.524$**
Uninsured > 6 months in past year	**$p < 0.492$**
Uninsured all of past year	**$p < 0.345$**

Notes:

N=1,844 for all regressions States: MA, MI, MN, WA

Each regression includes a measure of coverage as the dependent variable, a dummy for being a 15-17 year-old (the "age" dummy), a dummy for 1999 (the "time 2" dummy), a dummy for 2002 (the "time 3" dummy), an interaction of the age dummy and the time 2 dummy, an interaction with the age dummy and the time 3 dummy, and a constant term. The reference categories were 6-13 year olds (younger age group) and the year 1997 ("time 1"). Results are from linear probability models.

Table A2. Regression results for all variables, linear probability models

Treatment group: 15-17 year olds living in families w/incomes b/t 50-100% FPL
Control group: 6-13 year olds living in families w/incomes b/t 50-100% FPL

Panel 1: Point-in-time measures of coverage

Ind. variable	Dep. var. = Public cov.		Dep. var.= Private cov.		Dep. var.= Uninsured	
Treatment dummy	-0.056		0.084	**	-0.028	
	(-.168, .056)		(.004, .165)		(-.171, .115)	
Post dummy	0.194	*	-0.046		-0.148	
	(-.039, .427)		(.338, .245)		(-.501, .206)	
Treatment*post interaction	***0.239***	*******	***-0.100***	******	***-0.139***	******
	(.156, .322)		(-.195, -.005)		(-.261, -.017)	
In fair or poor health	0.044		-0.041		-0.003	
	(-.079, .168)		(-.180, .098)		(-.115, .110)	
Has limiting condition	0.108	***	-0.087	**	-0.021	
	(.042, .173)		(-.160, -.013)		(-.117, .075)	
Immigrant	-0.114		-0.122	**	0.236	***
	(-.336, .108)		(-.237, -.007)		(.069, .403)	
MKA has < HS degree	0.069		-0.065	**	-0.004	
	(-.016, .154)		(-.121, -.009)		(-.058, .050)	
MKA has >= college	-0.102	**	0.222	**	-0.119	
	(-.184, -.021)		(.059, .384)		(-.287, .049)	
MKA age	-0.002		0.004	*	-0.002	*
	(-.006, .002)		(-.001, .008)		(-.003, .001)	
Black	0.109		-0.093	**	-0.160	
	(.054, .165)		(-.185, -.002)		(-.080, .048)	
Hispanic	0.071	**	-0.117	***	0.046	
	(.006, .137)		(-.197, -.037)		(-.021, .113)	
Other race	0.153	**	-0.065		-0.088	**
	(.049, .257)		(-.184, .054)		(-.167, -.010)	
At least 1 worker in HH	-0.216	***	0.185	***	0.031	
	(-.294, -.138)		(.126, .245)		(-.037, .098)	
Worker in HH missing	-0.050		0.202	*	-0.152	**
	(-.314, .214)		(-.022, .427)		(-.289, -.016)	
Female	0.037	**	-0.029		-0.008	
	(.007, .068)		(-.090, .031)		(-.061, .046)	
Age	-0.026	***	-0.003		0.030	***
	(-.040, -.013)		(-.014, .007)		(.019, .041)	
Not a two-parent HH	0.049		0.035		-0.084	***
	(-.054, .152)		(-.039, .110)		(-.141, -.027)	
R^2	0.198		0.140		0.147	

*** p < 0.01 ** p < 0.05 * p < 0.10

Notes:
N=2,301 for all models. 95% confidence intervals in parenthesis. All models account for clustering at the state-year level and were estimated using probability weights. Coefficients for state dummies and state-level economic conditions not shown.

Table A2. *cont'd*

Panel 2: Dynamic measures of coverage

Ind. variable	Dep. var.: Ever unins.		Dep. var.: Unins>=6 mths		Dep. var.: Unins. all yr.	
Treatment dummy	0.065		0.030		-0.024	
	(-.108, .238)		(-.119, .179)		(-.200, .152)	
Post dummy	0.066		-0.032		-0.026	
	(-.163, .296)		(-.287, .223)		(-.346, .293)	
Treatment*post interaction	***-0.259***	******	***-0.190***	******	***-0.111***	
	(-.349, -.168)		(-.285, -.094)		(-.253, .029)	
In fair or poor health	0.006		-0.032		0.019	
	(-.104, .117)		(-.156, .091)		(-.088, .126)	
Has limiting condition	-0.093	**	-0.084	*	-0.054	
	(-.162, -.025)		(-.178, .010)		(-.151, .043)	
Immigrant	0.178	**	0.244	***	0.284	***
	(.010, .345)		(.068, .420)		(.109, .459)	
MKA has < HS degree	-0.004		-0.001		0.031	
	(-.060, .052)		(-.054, .052)		(-.023, .086)	
MKA has >= college	-0.089		-0.064		-0.116	**
	(-.251, .074)		(-.225, .096)		(-.211, -.020)	
MKA age	-0.003	**	-2E-4		0.001	
	(-.005, -5E-5)		(-.002, .001)		(-4E-4, .002)	
Black	-0.043		-0.020		-0.016	
	(-.124, .038)		(-.078, .038)		(-.069, .036)	
Hispanic	0.038		0.035		0.048	
	(-.043, .118)		(-.020, .091)		(.022, .074)	
Other race	-0.027		-0.023		-0.086	
	(-.324, .271)		(-.322, .275)		(-.208, .036)	
At least 1 worker in HH	0.042		0.065		0.056	
	(-.043, .127)		(-.018, .148)		(-.015, .126)	
Worker in HH missing	-0.210	***	-0.160	**	-0.087	
	(-.362, -.059)		(-.299, -.022)		(-.214, .041)	
Female	-0.006		-0.007		0.004	
	(-.069, .056)		(-.073, .059)		(-.040, .048)	
Age	0.022	**	0.024		0.029	***
	(.001, .044)		(.008, .041)		(.016, .042)	
Not a two-parent HH	-0.047	**	-0.040		-0.065	
	(-.094, -2E-4)		(-.094, .014)		(-.107, -.022)	
R^2	0.130		0.138		0.190	

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Notes:
N=2,301 for all models. 95% confidence intervals in parenthesis. All models account for clustering at the state-year level and were estimated using probability weights. Coefficients for state dummies and state-level economic conditions not shown.

Table A3. Sensitivity of estimates to inclusion of control variables

Panel 1: Point-in-time measures of coverage

Dependent variable	(1)	(2)	(3)
Publicly insured at survey			
Linear probability model	0.281 ***	0.245 ***	0.239 ***
	(.185, .378)	(.154, .335)	(.156, .322)
Multinomial logit	0.281 ***	0.247 ***	0.251 ***
	(.184, .377)	(.153, .341)	(.163, .339)
Privately insured at survey			
Linear probability model	-0.122 **	-0.100 **	-0.100 **
	(-.222, -.022)	(-.196, -.001)	(-.195, -.005)
Multinomial logit	-0.130 **	-0.103 *	-0.106 *
	(-.241, -.020)	(-.206, .001)	(-.223, .011)
Uninsured at survey			
Linear probability model	-0.160 **	-0.147 **	-0.139 **
	(-.291, -.028)	(-.263, -.031)	(-.261, -.017)
Multinomial logit	-0.150 **	-0.144 **	-0.145 **
	(-.293, -.008)	(-.267, -.021)	(-.273, -.017)
N	*2,301*	*2,301*	*2,301*

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

(1) No controls
(2) Some controls
(3) All controls

Notes:

95% confidence intervals in parentheses. All models account for clustering at the state-year level and were estimated using probability weights. Confidence intervals for the multinomial logit results were calculated from bootstrap estimates using the normal approximation.

Controls in "some controls" specifications include: bad health dummy, presence of a limiting condition dummy, immigrant status, dummies reflecting education of the parental respondent of the child, age of the parental respondent, race dummies, presence of a worker in the hh dummy, missing information on number of workers in the hh dummy, sex, age, and a dummy reflecting living in a non-two parent family hh.

The specifications in the "all controls" column include all variables listed under "some controls" as well as the following: the average family contribution for health insurance in the child's state during the year of survey, the percent of all firms in the child's state that offer health insurance during the year of survey, the state unemp. rate in the child's state during the year of survey, and state dummies.

Table A3. *cont'd*

Panel 2: Dynamic measures of coverage

Dependent variable	(1)		(2)		(3)	
Ever uninsured in past year						
Linear probability model	-0.280	***	-0.268	***	-0.259	**
	(-.373, -.186)		(-.361, -.174)		(-.349, -.168)	
Probit	-0.281	***	-0.273	***	-0.268	***
	(-.376, -.185)		(-.367, -.180)		(-.359, -.177)	
Uninsured > 6 months in past year						
Linear probability model	-0.214	***	-0.195	***	-0.190	***
	(-.332, -.096)		(-.292, -.098)		(-.285, -.094)	
Probit	-0.214	***	-0.203	***	-0.204	***
	(-.330, -.097)		(-.305, -.101)		(-.304, -.103)	
Uninsured all of past year						
Linear probability model	-0.133	*	-0.116	*	-0.112	
	(-.288, .022)		(-.253, .021)		(-.253, .029)	
Probit	-0.120		-0.118		-0.118	
	(-.281, .041)		(-.268, .032)		(-.267, .031)	
N	*2,301*		*2,301*		*2,301*	

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

(1) No controls
(2) Some controls
(3) All controls

Notes:

95% confidence intervals in parentheses. All models account for clustering at the state-year level and were estimated using probability weights. Confidence intervals for the probit results were calculated from bootstrap estimates using the normal approximation.

Controls in "some controls" specifications include: bad health dummy, presence of a limiting condition dummy, immigrant status, dummies reflecting education of the parental respondent of the child, age of the parental respondent, race dummies, presence of a worker in the hh dummy, missing information on number of workers in the hh dummy, sex, age and a dummy reflecting living in a non-two parent hh.

The specifications in the "all controls" column include all variables listed under "some controls" as well as the following: the average family contribution for health insurance in the child's state during the year of survey, the percent of all firms in the child's state that offer health insurance during the year of survey, the state unemployment rate in the child's state during the year of survey, and state dummies.