Summary

1. **Introduction**

The Medicaid public health insurance program emerged out of the Great Society initiatives to provide healthcare access and financial protection to low-income families. Since its inception in 1965, the Medicaid program has grown tremendously; in 1966, **X**% of government spending was dedicated to the program, where by 2005 this number has grown to **X%**. Two major trends are attributed to this rapid rise in the role of Medicaid. The first is the well-documented rise in the cost of healthcare and health insurance **(citation)**. The second is the increasing generosity of the program, particularly to low-income pregnant women, infants and children. Primarily initiated in the mid 1980s through the early 2000s, a series of federal mandates instituted eligibility expansions and drove states to increase coverage rates of these vulnerable populations (Gruber 1997; Shore-Sheppard X).

The first-order effects of these expansions are well documented by previous research. Medicaid expansions increased coverage, decreased child mortality, increased healthcare utilization, and reduced the likelihood of personal bankruptcies (Currie and Gruber 1996; Dafny and Gruber 2005; Gross and Notowodigdo 2008). Less well studied are the human capital spillover benefits of the eligibility expansions – that is, are there measurable spillover benefits resulting from the improved healthcare access and financial protection of subsidized health insurance? This study is dedicated to contributing toward this question by focusing on the education achievement outcomes of children. Since the education production function depends on health and family income inputs, it follows that public insurance programs are predicted to have a reduced-form effect on education.

Furthermore, the spillover benefits of subsidized health insurance are likely to have a greater impact on poorer households. Poorer households have higher rates of illness.[[1]](#footnote-1) They are also more likely to incur medical debts and suffer financial harm as a result of illness (Himmelstein et al. 2009; Dranove and Millenson 2006; Gross and Notowidigdo 2010; Finkelstein 2012).[[2]](#footnote-2) Medicaid is intended to address these problems among low-income families. As a result, Medicaid may have significant effects on the educational achievement of children in low-income families.

An emerging literature documents the long-run human capital benefits of the Medicaid program as the original cohorts exposed to the Medicaid expansions age into adulthood. Some of these findings suggest decreased hospitalization rates in adulthood, increased rates of high school and college graduation, and better labor market outcomes (Wherry et al. 2015; Cohodes et al. 2015; Brown et al. 2015). Given these findings, a natural extension question is where and when exactly do these benefits occur? I focus largely on the education gains resulting from contemporaneous expansions or cumulative life exposure to Medicaid eligibility to assess the short-term or more immediate benefits that result from public insurance coverage.

The following section provides the institutional background and previous literature focusing on this topic. Section 3 details the theoretical framework guiding the study. In Section 4, I use math and reading test scores available from the National Assessment of Education Progress to estimate the effect of Medicaid eligibility expansions on test scores. In section 5, I use administrative data from the universe of public schools in North Carolina to investigate slightly longer run effects on test scores and grade promotion resulting from increased Medicaid eligibility exposure. Results from these two sections suggest that Medicaid expansions are associated with small test score gains for low-income students, with little or lower corresponding effects occurring on high-income placebo samples.

Section 6 investigates potential mechanisms through which Medicaid expansions may impact education outcomes. Specifically, I use data from the Medical Expenditures Panel Survey to estimate the effect of Medicaid expansions on out-of-pocket healthcare expenditures. Since part of the causal pathway is predicted through increased income available to families, this section examines whether there are, in fact, any savings generated from Medicaid coverage. I find that while spending in some healthcare categories, such as emergency room and inpatient spending, are reduced, patients gaining health insurance tend to increase their utilization of other healthcare categories such as doctor visits and prescription drug use. On average, the small out-of-pocket costs spent on this increased utilization wash out the reduction in out-of-pocket savings in other categories, suggesting that increased non-health income is not a likely mechanism driving the education gains.

**2.** **Background**

**2.1 Institutional Background on Medicaid Expansions**

Established in 1965, the Medicaid program provides subsidized health insurance to eligible low-income individuals. Eligibility is determined by each state separately within federally established guidelines, and financed jointly by both federal and state funds. Although the program’s payment scheme and plan benefits are largely administered by each state, federal rules mandate the coverage of particular medical services for children such as physician services, inpatient and outpatient hospital services, and x-ray and laboratory services.

Eligibility for Medicaid was limited prior to the mid-1980s. Qualifying for Medicaid required eligibility for the Aid to Families with Dependent Children (AFDC) welfare program. Qualifying for the AFDC program was difficult – income eligibility thresholds were extremely low, and, unless the state participated in the AFDC Unemployed Parents program, eligibility was typically granted solely to single-parent households.[[3]](#footnote-3) Through the mid-1980s and early 1990s, a series of federal mandates gradually separated Medicaid eligibility from AFDC eligibility, and expanded Medicaid eligibility to provide coverage for children and pregnant women in low-income households that were unable to meet AFDC requirements due to earned income above the qualifying poverty threshold or failure to meet family structure requirements. Eligibility subsequently expanded to include older children. The most significant of these mandates and their descriptions are provided in Table 1.

One of the largest of the early federal expansions was the Omnibus Budget Reallocation Act of 1990 (OBRA90), requiring states to cover all children born after September 30, 1983 living in families with incomes below 100% of the federal poverty level. In section 5, I exploit this arbitrary cutoff date in a regression discontinuity framework by comparing those born before and after this date. The legislation went into effect in 1991, creating a jump in Medicaid eligibility for those born in October 1983 or later relative to those born earlier. This gap would not be closed until the passage of the CHIP/Medicaid expansions in 1997, giving the treated cohort about 5 extra years of Medicaid eligibility.

Despite these significant expansions in the Medicaid program, uninsured rates for children remained persistently high during the mid nineties. For example, in the baseline 1996 MEPS survey, nearly 14% of respondents under age 18 are uninsured. The Children’s Health Insurance Program (CHIP) was enacted to reduce the number of uninsured children by targeting the population of children left non-eligible after the initial federal mandates. This consisted largely of older-aged children as well as children in families that were unable to afford private health insurance yet earned more than the Medicaid eligibility threshold. The program was passed as part of the Balanced Budget Act and was provided more than $40 billion in federal matching funds to states dedicated to expanding Medicaid or entirely new state insurance programs and reducing the number of children uninsured.

The CHIP expansions typically increased eligibility for infants and young children from 133% of the FPL to 200% FPL. For older children, particularly those above age 15, the legislated change was much greater, since this group was not nearly as exposed to the earlier Medicaid expansion, and often had baseline eligibility cutoffs still set at the original AFDC thresholds.[[4]](#footnote-4) These baseline eligibility cutoffs were largely different from one another, so a substantial portion of the variation in Medicaid eligibility arises from the states with different starting points converging at similar end points (LoSasso and Buchmueller 2005).

Crucially for my research design, states varied in their initial AFDC/Medicaid income eligibility cutoff thresholds prior to the federal expansion mandates, and they introduced these expansions to provide at least the minimum federal eligibility mandates. States arrived at federal minimum eligibility requirements from different base levels, creating substantial variation in changes in eligibility across states, years, and ages. Figures 1 and 2 provide a visual representation of the variation induced by these expansions. Figure 1 shows the differences in eligibility across states in two separate points-of-time and at two separate ages. This figure highlights the dramatic increase federal Medicaid expansion mandates had on and across states in just the 10-year period between 1990 and 2000. Figure 2 illustrates the age and year variation in eligibility. Students born in different years experienced substantial differentials in exposure to Medicaid rates over the course of their lives. Also shown is the incremental national generosity of the expansions over time – eligibility was first extended to infants and pregnant women, then to young children, and finally to older children. By the time the CHIP expansions were fully implemented, children converged in eligibility rates across age.

**2.2 Related Literature**

*2.2.1. Medicaid and Education*

Although a growing literature reports evidence of substantial long-term gains from children’s Medicaid expansions in terms of health in adulthood (Wherry et. al 2015; Meyer and Wherry 2010), and income and employment outcomes (Brown et. al 2015), shorter-term childhood gains have proven difficult to demonstrate. Most pertinent to my research is the relatively small literature on whether Medicaid eligibility expansions affect earlier human capital measures such as education achievement for children.

Levine and Schanzenbach (2009) examined the impact of Medicaid expansions on 4th and 8th grade math and reading assessments using NAEP data. Results from this study showed significant effects of Medicaid eligibility during prenatal-timed expansions, but only for reading test scores. Cohodes et al. (2015) used the American Community Survey to investigate whether Medicaid affected teenage and early-adult education outcomes. They find that cohorts exposed to higher rates of Medicaid eligibility have higher rates of high school graduation, college attendance, and college graduation. They note that the effects are particularly pronounced for Medicaid eligibility in contemporaneous periods.

I extend this small literature by examining whether Medicaid eligibility expansions affected children’s test scores. An important feature of my research, for example, vis-à-vis Levine and Schanzenbach (2009), is that I make use of individual-level information in the NAEP, which allows me to focus on the more affected group of low-income children. I also examine a longer period of time and assess whether expansions in Medicaid eligibility at different points in a child’s life has different effects.

*2.2.2 Previous work using OBRA 90 Eligibility Data Cutoff*

In section 5 I make use of the OBRA90 Medicaid eligibility discontinuity for those born directly after September 31, 1983. While this section is novel in that it the first study to examine the education effect of this discontinuity, it is important to note that it is not the first study exploiting this discontinuity to measure the impact of increased eligibility. Card and Shore-Sheppared (2005) estimate the take-up effect of the policy and find that Medicaid coverage rates increase by approximately 8 percentage points for children born directly after the cutoff date. Meyer and Wherry (2012) use mortality data to show that affected cohorts experience lower later-aged mortality. Wherry et al. (2015) further use data from the National Health Interview Survey to show an association between birth after the eligibility cutoff date and a reduction in adulthood hospitalizations.

*2.2.3. Medicaid and Out-of-pocket Healthcare Spending*

There are two proposed mechanisms through which Medicaid may plausibly impact children’s education achievement. The first is improved health outcomes of the child. Medicaid expands healthcare access and lowers the price of care and increases utilization. If the increased use of health care improves child health, the improved health of the child may enhance the cognitive abilities of the child and as a result improve education achievement. The second potential mechanism is the income saved from the reduction in out of pocket health expenditures. The saved income can increase spending available to the family that can in turn be spent on child cognitive investments. Section 6 of this project is dedicated to estimating the magnitude of such reductions in out of pocket expenditures, an endeavor that has surprisingly not been largely addressed in the large Medicaid literature.

Prior literature generally finds that subsidized health care expansions tend to reduce out-of-pocket spending and family health insurance burdens. Finkelstein et al. (2012) indicate that the recent Oregon Medicaid lottery expansion resulted in approximately a 35% decline in the likelihood of having out-of-pocket expenses. The authors also estimate a 10% reduction in the likelihood of unpaid bills being sent to collections agencies. While this study contains an ideal research design, the Oregon study affected a limited sample of low-income childless adults. The effect on children, generally thought to be a healthier subgroup, may not be as dramatic as the findings in Oregon.

In an evaluation of the effects of the SCHIP program Davidoff et al. (2005) use survey data from the NHIS and find that the CHIP expansions are associated with reductions in out-of-pocket expenditures for chronic children (although estimates are not significant). The authors’ study focuses exclusively on the impact of the expansions on chronically ill children, which overlooks the impact of the expansion on low-income children who drive up high out-of-pocket expenditures due to severe acute health events.

Banthin and Selden (2003) estimate the change in total family out of pocket health expenditures occurring as a result of earlier Medicaid expansions occurring between 1987-1996. They combine a wave of the National Medical Expenditures Survey (NMES) and the Medical Expenditures Panel Survey and use a simple differences-in-difference design to evaluate the impact of the Medicaid expansions. The authors find that the early Medicaid expansions to children reduce total family out-of-pocket healthcare expenditures relative to families of children who would have been eligible for the later CHIP expansions. The authors find the magnitude of this spending reduction is about $580, of which about 60% is due to a reduction of spending on services and 40% is due to a reduction in spending on out-of-pocket premiums, which suggest potential for crowding-out of private insurance with these public insurance expansions.

Although there is general consensus that Medicaid expansions reduced out-of-pocket expenditures, except for Banthin and Selden (2003), these previous studies have not investigated whether the composition of health care spending is affected by public insurance expansions. While Banthin and Selden (2003) separate expenditures for services and health insurance premiums, they do not look at spending within services.

1. **Conceptual Framework**

3.1 Impact of Public Insurance on Education Production

Medicaid provides greater access to medical care by reducing the price of care and increasing family income available to spend on other consumption. As a result, children who gain insurance may be healthier and families will have more income to spend. Some of that extra spending may be on children. These consequences may increase children’s educational achievement.

To illustrate how Medicaid is predicted to affect achievement, consider a simple production function for education achievement, which assumes that present achievement depends on current health stock and cumulative cognitive investments, as well as individual ability. This relationship can be described as . That is, achievement, , for child at age depends on the child’s current health stock, the past history of the child’s cognitive investments (, ability , and an error term.

The health stock can be written as:

In equation (1), the health stock is specified as depending on all past use of medical care inputs (for example, doctor visits, ER visits, medications, lab tests) and other health investments (for example, nutrition, exercise, behaviors). A period-specific health shock term introduces a stochastic element in the determination of the health stock. The time subscripts on the input coefficients indicate that inputs have varying ability to improve contemporaneous health. For example, if medical care inputs depreciate in their ability to improve current health stock, we might find that , for . On the other hand, if medical care inputs operate on health with a lag, or if earlier health interventions are more effective than later ones, then it is predicted .

Taken together, the achievement production function depends on all past use of medical care, other health investments, and cognitive inputs for the child. That is:

To simplify for empirical use, the production function may be expressed as:

Empirical evidence suggests that Medicaid expansions increase inputs. Medicaid increases the use of physician services as well as other medical services (Currie and Gruber 1996; Dafny and Gruber 2005) and improves teen health behaviors (Cohodes et al. 2015). Thus, replacing these inputs in the above production function with indicators for Medicaid eligibility will yield an estimate of the reduced form effect of such eligibility on achievement. Specifically:

Estimates of equation (4) may be biased, however, because other determinants of production inputs such as child ability, time-invariant health characteristics, period-specific health shocks, and parental preferences may be correlated with Medicaid eligibility and are either not observed or not measured.

To address this selection problem, the research design in Section 4 and Section 6 exploits state-year-age level changes in Medicaid eligibility – these are assumed orthogonal to and . This creates a quasi-experimental design in which eligibility status is shifted independent of individual factors contributing toward eligibility, coverage, and achievement. The key identifying assumptions of the study is that education trends across individuals affected and unaffected by Medicaid expansions would have been the same in the absence of Medicaid expansions. In Chapter 5, I exploit a discontinuity in Medicaid eligibility occurring by date of birth due to legislation details. This approach assumes other covariates and unobserved characteristics are smooth functions across the eligibility threshold date. These econometric approaches are detailed in their respective chapters below.

As shown in equation (3), investments at each age may have different effects on children’s achievement (Todd and Wolpin 2003). This may be due to a variety of reasons. For investments in health and how health affects educational achievement, the timing of investments may matter because of biological and clinical reasons. The causal effect of health, and therefore investments in health, on cognitive achievement may work through early improvements in health or contemporaneous improvements in health. There is not clear evidence to suggest which of these possibilities is correct.

Medicaid provides access to medical care and income for other health investments. How this access and income translates into health and at what age is unknown. It is possible there are “critical” periods in child development and that investments in health may matter most during these periods (Cunha and Heckman 2007; Knudsen et al. 2006). A number of studies emphasize early intervention programs and Medicaid expansions during infant and toddler years may be most significant in terms of improving later life health and education (Levine and Schanzenbach 2009; Carneiro and Heckman 2003; Cunha et al. 2005). On the other hand, Cohodes et al. (2015) found that more recent Medicaid expansions are linked to improvements in teen health behaviors, and Dahl and Lochner (2012) find that EITC income benefits to families are most effective in improving education achievement during the contemporaneous years.

* 1. *The Theoretical Framework of the Effect of Medicaid on Out of Pocket Health Expenditures.*

One pathway through which Medicaid is predicted to affect education achievement is increased savings on out-of-pocket healthcare expenditures. However, the impact of subsidized healthcare spending is theoretically ambiguous. The financial importance of health insurance is to directly protect individuals from excessive out-of-pocket expenditures arising from unanticipated health complications. We would expect out-of-pocket spending at the highest end of the spending distribution to reduce due to this financial protection from catastrophic care. The savings at all points of the distribution depend on how much the uninsured spend on out-of-pocket healthcare at baseline and the incidence of illness in children. However, transitioning from uninsured status to insured status may provide healthcare access not easily available before to beneficiaries – this may increase spending on healthcare, particularly on preventive care.

To help illustrate this, consider a utility framework in which individuals gain utility from health and a composite good and seek to maximize health subject to a budget constraint. Define the health production as a function of two medical care inputs:

|  |  |
| --- | --- |
| (1) |  |

Suppose the two inputs vary in their marginal ability to improve health and their price. In times of illness, health can be repaired using medical care inputs. In a simple constrained optimization framework, we can describe the individual’s optimization problem as:

|  |  |
| --- | --- |
| (2) |  |

Where refers to the individual’s utility relation, is the health stock, is the composite consumption good. is family income and is the prices of the medical input . The price of the consumption good is normalized to 1. The standard first order condition for medical care input is:

|  |  |
| --- | --- |
| (3) |  |

This is a basic marginal benefits equal marginal costs relationship. Benefits are the marginal utility enjoyed from improved health scaled by the efficacy of the th input. However, increased medical care utilization comes at the cost of the marginal utility that could have been derived from consumption, scaled by the relative price of medical care. The marginal rate of substitution between medical care inputs is:

|  |  |
| --- | --- |
| (3) |  |

That is, individuals will use medical inputs in such that their relative marginal efficacy is equal to their relative price. Medicaid can be thought of as a price reduction of medical care. Generally, Medicaid reduces prices across all major inputs. If the price reduction across inputs is proportionally the same, then it is not clear beneficiaries will alter their relative consumption of medical care inputs. This framework, however, does not incorporate the idea of improved access to Medicaid beneficiaries relative to uninsured individuals. For example, if physicians are more willing to make appointments with insured patients than uninsured patients, but emergency rooms are relatively indifferent to patient coverage status, then we may anticipate the relative price of doctor visits to emergency room care to decline and induce beneficiaries to substitute toward doctor visits upon coverage. The effect on total health care expenditures depends on the general price elasticity of medical care relative to consumption. Section 6 is dedicated to parsing these theoretical ambiguities.

**4. Evidence from the National Assessment for Education Progress**

4.1. Introduction

Health insurance provides two major benefits. Insurance protects families from the financial risk arising from unforeseen, adverse health shocks. It also provides access to medical care to repair health after illness. Accordingly, health insurance may raise the education achievement of children by making them healthier and more productive in school and by increasing family income that can be used for educational investments.

In this section, I test this hypothesis by examining whether Medicaid eligibility expansions occurring in the mid-1980s through the early 2000s improved the cognitive achievement and absenteeism of children in low-income environments. Data for the analysis are from the National Assessment for Education Progress (NAEP), which provide math and reading test scores for 4th and 8th graders.

4.2. Data

Since 1969, the National Center of Education Statistics has assessed education achievement statistics for school-aged children. These assessments are collected in the National Assessment of Education Progress (NAEP). Starting in 1990, math and reading assessments were administered separately in states to 4th grade and 8th grade students (later assessments include 12th grade analysis as well), allowing for a state-level analysis of test scores and enabling state-by-state comparison of education achievements. The data from the test score analysis is from the restricted-use individual level National Assessment of Education Progress (NAEP). I use math and reading assessments for 4th and 8th graders.

Levine and Schanzenbach (2009) used NAEP data to similarly assess the impact of Medicaid expansions on test scores. I extend their analysis in a fundamental way. Levine and Schanzenbach (2009) make use of the NAEP’s item-response theory (IRT) scaled scores. The scaled score is comprised of many different “blocks” of questions, each block testing a unique math or reading skill. Students are randomly assigned question blocks. As a result of this design, the IRT test scores are meant to provide a distribution of education achievement at the district, state, or national level, but not the individual or school level.[[5]](#footnote-5) This prevents Levine and Schanzenbach (2009) from focusing on the affected group of low-income children.

Taking advantage of access to restricted-use individual level NAEP, I observe both the questions asked and the answers provided by each student. Because students are randomly assigned question booklets, assessment difficulty is orthogonal to each individual’s abilities and health. I calculate the percentage of questions answered correctly within each block of questions that each individual takes – this is the main unit of analysis. My empirical design uses block-questions fixed effects; that is, I adjust each student’s scores for differences in difficulty across blocks. This allows the econometric specification to retain an individual-level analysis and permits richer testing for heterogeneous effects in Medicaid eligibility expansions.

In particular, I use individual and school-level factors as covariates, as well as items by which to stratify the analysis. Although individual free lunch status is not provided consistently as a covariate in earlier assessments, all assessments contain a principal questionnaire asking what percentage of students in the school are eligible for free or reduced lunch. To investigate heterogeneous effects, I test models separately by individuals who attend schools in which 0-25% of their peers are eligible for free or reduced lunch and individuals who attend schools with 75-100% of their peers are eligible free or reduced lunch. Children in poorer schools are more likely to be eligible for Medicaid while students in wealthier environments serve as a placebo group not likely exposed to the eligibility expansions.

Table 2 provides descriptive statistics for the main outcomes and controls used for the NAEP analysis. I provide means for the full sample, the sample of students attending high-income schools in which 0-25% of their peers are eligible for free or reduced lunch, and the sample of students attending low-income school in which more than 75% of their peers are eligible for free or reduced lunch. In terms of student demographics, high-income schools have a larger percentage of white students, while students in low-income schools have larger percentages of black and Hispanic students.[[6]](#footnote-6) Income eligibility cutoffs are well balanced for the two groups.[[7]](#footnote-7) The composition of poverty changes across grades – in 4th grade, 20% of the sample attends the lowest-income schools whereas by 8th grade this drops to 15%.

I also use make use of attendance outcomes available in the NAEP. Each assessment asks children to indicate the number of school days missed in the previous month. Table 2 shows that these measures are similarly balanced across both grades. Students in the low-income school sample are more likely to miss slightly more school across all thresholds.

4.3 Econometric Approach

In order to estimate the effect of the Medicaid expansions on education, I implement a differences-in-differences design exploiting within-state variation in changes in eligibility. The main specification is:

indicates the maximum allowed income to remain eligible for Medicaid for each age, state, and year group. These models are stratified by grade. State fixed effects control for time-invariant differences in characteristics across states. Year fixed effects control for changes over time in national education trends. The parameters of interest are the coefficients on – they provide an estimate of the impact of changes in income eligibility cutoffs on achievement. Individual covariates (i.e. sex and race) as well as state-by-year variables (i.e. unemployment rate, demographic characteristics, average state income) are included to improve matching. Since Medicaid expansions might be correlated with changes in the generosity of welfare and food stamp programs, all specifications include state-year averages of household welfare and food stamp receipt.

To exploit the variation in likelihood of treatment across individuals, the main specifications are stratified by school income. I use two samples – students attending schools in which 75-100% of the student population is eligible for free or reduced lunch and students attending schools in which 0-25% of the students in the school are eligible. Generally, reduced lunch eligibility designates family income below 185% of the federal poverty level while eligibility for free lunch indicates family income below 130% of the of the federal poverty level. The sample of students attending high-income schools constitutes a falsification test and should have minimal response to the Medicaid expansions, while the sample of students attending low-income schools are predicted to have the largest response to Medicaid expansions.

As noted earlier, it is unclear when eligibility for Medicaid may matter most for children’s educational achievement. Accordingly, I use several specifications that allow for different possibilities. For example, I estimate several versions of equation (4), and I show these with reference to 4th grade students:

For ease of notation, equations 5a-5c omit fixed effects and covariate factors. As shown, the specifications above allow for Medicaid to have different effects depending on when they occur in the child’s life. To clarify, note that is the average Medicaid income cutoff between ages 8 through 10 of the child. The other variables are denoted analogously. Specifications for 8th graders include a fourth age-bin category for ages 11 through 14.

Equations (5a) through (5c) allow me to distinguish between the relevant ages during which Medicaid expansions are most effective in improving education outcomes. I begin by estimating the contemporaneous response of Medicaid expansions on test scores in equation (5a).

If Medicaid produces health, income, and subsequent education improvements evenly across the ages of children, I may find that and are not different in magnitude – then the timing of Medicaid expansions may not be as important as the cumulative exposure to them. I test for this in two ways. First, F-tests in equations (5b) and equations (5c) measure whether these Medicaid parameters are jointly significant in determining education and attendance improvements. Second, I use following specifications for a more complete analysis of Medicaid cumulative exposure effects:

These specifications simply expand the age range to analyze the importance of the cumulative effects of Medicaid expansions. Equations (5e) through (5f) incrementally expand the age range of analysis beginning with the most recent years. If additional years of Medicaid expansions further improve education outcomes, and will be larger than and from equation (5a). If Medicaid eligibility during early ages do not influence education outcomes today, and will be smaller in magnitude and lose precision since they add noise to the cutoffs estimate.

4.4. Results

4.4.1. Medicaid’s Impact on Cognitive Achievement

Results for the impact of Medicaid eligibility at various ages over the child’s life for the 4th grade sample are presented in Table 3. Estimates are interpreted as the effect of a $10,000 increase in the Medicaid income eligibility cutoff. Panel A contains the 4th grade mathematics and reading analysis for students attending schools comprised of mostly low-income students (75-100% of students in school are eligible for free/reduced lunch). Panel B shows results from the analysis for students in high-income schools (0-25% of students in school are eligible for free/reduced lunch). Each column represents a separate regression. Column (1) uses the specification from equation (5a) and uses the average income cutoff threshold the student is exposed to for the most recent years.[[8]](#footnote-8) Column (2) estimates equation (5b) by augmenting equation (5a) with the average Medicaid eligibility income cutoffs for ages 4-7, and column (3) estimates equation (5c) by including the average Medicaid eligibility cutoffs for ages 0-3.

Estimates in Panel A of Table 3 indicate that contemporaneous Medicaid eligibility expansions are associated with positive effects on math test scores for students in low-income schools. The estimate in column 1 indicates that expanding Medicaid income eligibility by $10,000 improves test scores by 0.8 percentage points. Including exposure to expansions from earlier years does not affect these estimates, and estimates of the effect of earlier life expansions are not significant. In column (3) the estimate for the effect of contemporaneous Medicaid expansions increases to 0.9 percentage points, which is 0.05 of a standard deviation.

Estimates for reading test scores indicate no significant relationship with contemporaneous Medicaid expansions for 4th graders in low-income schools. However, expansions during ages 4 through 7 do improve test scores for low-income 4th graders. Column (3), shows that when the complete Medicaid expansion history during the child’s life, expansing Medicaid income eligibility by $10,000 improves low-income 4th grade reading test scores by 0.9 percentage points, or 0.04 of a standard deviation.

In contrast, Panel B shows contemporaneous Medicaid eligibility expansions are associated with negative effects on math scores for students in high-income schools. These effects are indistinguishable from zero. There is no relationship between math test scores for high-income students and Medicaid eligibility at early years. The estimates indicate that there is no significant association between Medicaid expansions and reading scores for students in high-income schools.

Table 4 presents the 8th grade mathematics and reading results. Estimates in Panel A indicate a strong positive association between contemporaneous Medicaid eligibility and math tests scores for 8th graders attending low-income schools. Expanding Medicaid income eligibility cutoffs by $10,000 result in a 0.7 percentage-point increase, which is a 0.04 standard deviation improvement. Estimates pertaining to reading achievements indicate positive effects of Medicaid eligibility in contemporaneous periods – however these estimates are not statistically distinguishable from zero. Conversely, Panel B indicates that Medicaid expansions have no significant effect on test scores for 8th graders in high-income schools. This is true for all specifications and Medicaid expansions at all ages in the child’s life.

Since Medicaid expansions are often correlated across ages and years, there is correlation between Medicaid expansions occurring at different points of life for the same child. Further, the effects of public insurance expansions may not be driven by point-in-time exposure, but rather by the cumulative exposure experienced throughout a child’s life. Tables 5 and 6 test for this using the total average Medicaid eligibility exposure across age ranges to assess the impact on test scores. In these tables, each cell represents a separate regression. The age ranges tested start from the contemporaneous period, then are incrementally expanded to accommodate larger portions of the child’s life – the bottom row uses the total average Medicaid eligibility exposure throughout the child’s life.

Table 5 shows the effect of cumulative Medicaid exposure on math test scores. In the 4th grade, low-income sample, estimates are positive and large for the contemporaneous period, but then fall in magnitude and precision as the age range is extended to earlier periods of the student’s life. In 8th grade, Medicaid expansions during the contemporaneous period have large, positive and significant effects on math scores among students in low-income schools. In contrast to the 4th grade results, however, these estimates get larger as the age range is expanded – expanding the age range also dramatically increases the standard errors, indicating a decrease in the variation of cutoffs once averaging for full life cumulative effects. Estimates from 4th and 8th grade results for students in high-income schools show no association in Medicaid cumulative exposure and math test scores.

Table 6 repeats the cumulative exposure analysis for reading test scores. 4th grade low-income students improve in test scores as a result of expansions occurring throughout their lives. The estimates from more contemporaneous age ranges have no effect on test scores. For the 8th grade sample of students in low-income schools, estimates from semi-contemporaneous models indicate positive results on reading test (expansions occurring through the range of ages 5 through 14), however the estimates depreciate when considering larger cumulative ranges of the child’s life. The estimates again indicate that neither 4th grade nor 8th grade test scores for students in high-income schools are measured to have any effect resulting from Medicaid expansions.

To summarize, Medicaid eligibility expansions are associated with improvements in math test scores among low-income students. It is shown that reading test scores for 4th grade low-income students also improve, although these improvements are attributed to Medicaid eligibility expansions earlier in life. In contrast, estimates suggest little effect of Medicaid expansions on test scores of students attending high-income schools.

4.4.2. Effect of Medicaid on School Attendance

One plausible mechanism linking Medicaid to test scores is health. If Medicaid expansions improve health, they may reduce student absenteeism occurring due to illness. I use four measures of student attendance provided in NAEP assessments to test this hypothesis. The NAEP asks children how much school they have missed in the previous month. I use measures of increasing severity: whether the child missed any days, missed more than 3 days, missed more than 5 days, or missed more than 10 days of school in the previous month. While Medicaid coverage may cause an increase in school absences as children schedule appointments with doctors, higher absenteeism thresholds (for example, missing complete weeks of school), are likely to be sensitive to underlying health.

Tables 7 and 8 show attendance results for the 4th grade samples. Table 7 highlights the analysis on the two less severe measures of attendance. Among students in low-income schools (Panel A), Medicaid expansions are linked to improved attendance (negative signs indicates students are missing *less* school). Column (2) indicates that expanding Medicaid income cutoffs by $10,000 reduces the likelihood of missing any school by about 1.7 percent off a baseline of 52 percent. However, across both attendance measures, most estimates are indistinguishable from zero. In the analysis of missing more than 3 days in the past month, F-tests in column (2) and column (3) demonstrate that estimates of the effect of Medicaid expansions are jointly significant for low-income students. Panel B demonstrates little relationship between Medicaid expansions and school attendance along both attendance measures for high-income students.

For measures of larger number of absences, Table 8 provides the results for missing more than 5 days or more than 10 days in the past month. Panel A indicates Medicaid expansions decrease the likelihood of missing school on these thresholds. For example, in column (3) it is shown Medicaid expansions in infant years significantly reduce the likelihood of missing more than 5 days of school by 6%, while eligibility expansions in preschool through early elementary years contribute to a 7% decline from the mean of 0.09.

However, Panel B of Table 9 indicates that high-income students in the 4th grade sample exhibit similar improvements in attendance rates as a result of Medicaid expansions. This questions the experiment’s validity, as it would be expected the health of high-income students is unaffected by Medicaid expansions.

Table 9 and 10 investigate the 8th grade sample attendance analysis. Panel A of Table 9 shows negative effects of Medicaid expansions on missing school along the less severe thresholds of missing any school or missing more than 3 days in the past month. However, these effects are for the most part indistinguishable from zero. In Panel B, a positive effect is observed for contemporaneous Medicaid expansions impacting the missed school days of students in high-income schools. Contemporaneous Medicaid eligibility expansions are shown to make high-income students miss *more* school.

Table 10 provides the attendance analysis results for larger measures of absenteeism for the 8th grade sample. The impact of Medicaid expansions are not found to have any impact on attendance for 8th graders attending low-income schools when the measure is for missing more than 5 days. Across all specifications, a $10,000 Medicaid income eligibility expansion the effect is associated with 10% decrease in the likelihood of missing school for more than 10 days in the past month. Panel B, on the other hand, demonstrates that similar expansions have no effect on the placebo sample.

Table 11 tests the cumulative exposure analysis for the outcome of missing more than 10 days of school in the past month. For 4th graders, Medicaid expansions are shown to impact both low-income and high-income students. The magnitude of the effect for both samples increases as the age-range is augmented to include earlier years. For the 8th grade sample, more contemporaneous Medicaid expansions affect student attendance in low-income environments only – the comparison group of students in high-income schools exhibits no relationship between cumulative exposure to Medicaid eligibility expansions and attendance.

**5. Early Life Medicaid Eligibility and Education Achievement**

5.1 Introduction

Public health insurance provides children with increased healthcare access and financial protection from unanticipated adverse health events. Medicaid, the nation’s largest means-tested program, provides coverage to more than 30 million children and finances 40% of all births[[9]](#footnote-9). The immediate benefits of this program to children are increased healthcare utilization (Currie and Gruber 1996), decreased mortality, and decreased personal bankruptcy rates (Gross and Notowodigdo; Finkelstein et. al.).

A series of expansions starting in the mid-1980s through the early 2000s vastly increased the number of children eligible and covered by Medicaid. One of the largest of the federal expansions was the Omnibus Budget Reallocation Act of 1990 (OBRA90), which required states to cover all children born after September 30, 1983 living in families with incomes below 100% of the federal poverty level. We exploit this arbitrary cutoff date in a regression discontinuity framework by comparing those born before and after this date. The legislation went into effect in 1991, creating a jump in Medicaid eligibility for those born in October 1983 or later relative to those born earlier. This gap would not be closed until the passage of the CHIP/Medicaid expansions in 1997, giving the treated cohort about 5 extra years of Medicaid eligibility. Our analysis begins in 1997, after the eligibility gap has closed. Thus, we are not directly testing the contemporaneous effects of Medicaid coverage but rather the impact of past Medicaid use on later academic achievement.

  Using administrative student-level data in North Carolina, we look at whether the increased exposure to Medicaid eligibility created differences in test scores, grade promotion, and school absences.

One drawback of the administrative data from North Carolina is that we do not have information on student health insurance coverage. In order to frame an appropriate first stage estimate, I use survey data described below to estimate the effect of the policy by census region. I show that take-up rates vary based on geography, however the largest estimates of take-up are observed in the south census region (which contains North Carolina), indicating the legislation is particularly binding in this region of the country. The estimated range of take-up is between 12-15 percentage points. There is no corresponding discontinuity observed for those on either side of the cutoff with family income above 100% of the FPL during period of time.

**5.2 Data Description and Hurdles**

We use administrative data provided by the North Carolina Education Research Data Center. These data provide the full universe of test scores administered to students attending public school in the state. The evaluation of the dataset does create three major challenges to assess our research question. First, the dataset begins in 1995, notably four years after the implementation of OBRA90. Thus the earliest we see the treated September/October 1983 cohort is at age 11/12. To complicate matters, the NCERDC does not collect data on 5th grade and above in 1996. This gap in data is problematic because it prevents following students in the treated cohort longitudinally from 1995-on (student IDs are reset in 1997). Finally, the analysis of high school students in the NCERDC is not comprehensive until 1999. This means we are only able to observe snapshots of the affected cohort in 1995, 1997 (provided they are not in high school by this year), and 1999-on. Nonetheless, considering the large sample sizes and the considerable statistical power, the NCERDC dataset is still a rich choice for evaluating this research question.

A final challenge is presented in the official kindergarten entry date in North Carolina. The kindergarten entry date is October 15, thus those born just after this date are likely to be a full grade *lower* than those born prior to October 15. Our North Carolina dataset censors the day of birth for students, so we cannot observe whether born in October do or do not make the kindergarten entry date. Therefore while students born in September and October of 1983 are likely to differ in their Medicaid eligibility, they are also likely to differ in terms of the grade. To separate the effect of Medicaid eligibility from grade assignment, we will use a regression discontinuity differences-in-differences design detailed in the econometric section below.

In order to assess whether the OBRA90 legislation is binding for children in North Carolina, we also estimate the Medicaid take-up effect. The NCERDC administrative data unfortunately does not provide health insurance information for students. Large public data sets each have limitations in estimating this problem. I use the Integrated Health Information Series (IHIS) managed by the University of Minnesota, which crucially provides a large sample with birthdate information at the month and year level. The drawback with the IHIS dataset is the lack of state identifiers, so the first stage analysis is run at the census region level and focuses on the south census region, which contains North Carolina.

**5. 3 Econometric Approach**

*5.3.1. Estimating Take-up Rates for the South*

A first estimate to show is whether the OBRA90 legislation was binding for students in North Carolina. Unfortunately the NCERDC does not provide information on health insurance, so I cannot estimate take-up for the tested samples. In the IHIS sample, I run the following estimation:

Here I estimate the probability of Medicaid coverage for individual in birth month cohort in year . Cohorts are centered on October of 1983, the point at which Medicaid eligibility is discontinuous. The indicator represents whether or not the birth month cohort is on or after October of 1983 (i.e. ). The specification here presents a quadratic polynomial in the running variable; however, we will also implement linear and cubic polynomials for sensitivity testing. The polynomial is interacted with permitting the functional form to change on both sides of the cutoff date. The parameter of interest is , which represents the impact of Medicaid eligibility at the cutoff date. The specification includes year indicators and birth month indicators to adjust for national trends and seasonality effects.

*II. Differences-in-differences*

We opt for three econometric approaches in estimating the impact of the Medicaid eligibility expansions on education outcomes. Due to the kindergarten cutoff entry month occurring in October, we cannot simply implement a standard regression discontinuity estimation strategy – individuals on both sides of the October cutoff are likely to be in different grades and therefore are likely to have a discontinuous impact on academic outcomes solely driven by this effect.[[10]](#footnote-10) Thus to separate the observed-grade discontinuity effect from the Medicaid eligibility discontinuity effect, we are required to compare the effect for the treated cohort with another sample of students who are not effected by the Medicaid eligibility. Then the difference in estimates between this group and a treated sample would difference out the observed grade effect.

A first approach at estimating the effect of the October 1983 cutoff date is to implement a standard differences-in-differences design. Consider two samples – a treated sample of students born between 1982-1984 and a control sample of students born between 1980-1982 (i.e. there should be a Medicaid eligibility discontinuity during the middle October of the 1982-1984 cohort, but not for the control sample). We use the middle October of both samples (i.e. Oct of 1983 and Oct of 1981) as a designation of a post period. In practice, we pool instances in which the two samples would be the same ages. For example, in the 1995 observations of the 80-82 sample to the 1997 observations of the 82-84 sample. Then a simple differences-in-differences specification is:

Above, is an indicator for whether the individuals is in the 82-84 sample, is an indicator for whether the individuals are born after the “middle” October. The parameter of interest is , the coefficient on the interaction term. The identifying assumption of this estimation is that the treated sample would have trended similarly (across birth months) as the control sample in the absences of the Medicaid eligibility expansion.

A second approach we estimate will is the differences-in-differences regression discontinuity approach. This approach combines specification (5) and specification (4):

Estimations (6) implements two flexible polynomials ( and parameters) on for each sample, and allows the functional form to change on both sides of the “middle” October. We would only expect a discontinuity in eligibility for the sample. The parameter of interest here is which estimates the impact of being born *after* October 1983 independent of the general effect of being born after October (which is picked up by the parameter).

**5.4 Results**

*5.4.1 – Take-up Rates*

**Chapter 6: The Impact of Medicaid Eligibility Expansions on Out-of-pocket Healthcare Expenditures: Estimating the Income Channel.**

6.1 Introduction

While health insurance mechanically reduces the out-of-pocket cost of care resulting from emergency room visits or hospitalizations, health insurance also increases access to health care, which may lead to increased utilization (Dafny and Gruber 2005). Therefore, the net impact of coverage has a theoretically ambiguous effect on total out of pocket health expenditures. This section uses restricted data from the Medical Expenditures Panel Survey (MEPS) to examine the impact of Medicaid expansions to children on their out of pocket healthcare expenditures. Since the MEPS breaks expenditures by category, this study tests for changes in the composition of healthcare spending – these categories are office-based doctor spending, outpatient spending, emergency room spending, inpatient hospital spending, spending on dental care, and spending on prescribed medications.

In 2015 when the CHIP program was up for reauthorization, Hillary Clinton and Bill Frist, supporters of the original legislation, wrote an opinion piece advocating for the continuation of the program. The authors write:

“If CHIP is not reauthorized … millions will have fewer health care benefits and much higher out-of-pocket costs, threatening access to needed health services. And because families without adequate insurance often miss out on preventive care and instead receive more expensive treatment in hospital emergency rooms, all of us will be likely to end up paying part of the bill.”[[11]](#footnote-11)

Policy proponents of Medicaid often make similar claims that subsidized insurance helps reduce excessive resource waste by transferring low-income patients from hospital and emergency room settings to office or practice-based ones where delivery of care is thought to be more effective and efficient. Increased preventive care use among low-income households is predicted to prevent or control the development of chronic diseases and reduce use of higher intensity care down the road. I test these claims by estimating whether Medicaid expansions had an impact on the composition of medical care utilization and spending. Specifically I test for whether individuals increase their use of healthcare utilization in office based or outpatient environments and whether they reduce their use of care that is described as inefficient in the emergency room and inpatient setting.

6.2 Data

Starting in 1996, the Agency for Healthcare Research Quality began a surveying individuals and family about health service utilization and expenditure information in the Medical Expenditure Panel Survey (MEPS). The MEPS analysis used in the study is from the Household Component of the data, which collects a subsample or participants National Health Insurance Survey. In order to investigate the impact of CHIP/Medicaid expansions on out-of-pocket expenditures, for this study I was granted access to the restricted-use MEPS which permits use of state-identifiers and subsequently allows merging of Medicaid eligibility exposure to individuals.

To estimate the impact of CHIP/Medicaid expansions on out of pocket spending, I use a number of “self-pay” variables by category as outcomes. Total out-of-pocket expenditures are available in the MEPS, but these do not include spending on health insurance premiums. Subcategories of utilization include office-based visits, outpatient visits, inpatient visits (which include zero-night hospital stays), emergency room visits, dental care visits, and the number of prescribed medicines. I use each of these categories’ utilization as well as out-of-pocket payments as outcomes in the analysis.

Table 1 provides descriptive statistics of the baseline 1996 sample by insurance status. Relative to the Medicaid sample, the uninsured pay *more* in out-of-pocket expenditures across all categories and, remarkably, use *less* across all categories as well. However, it should be noted these differences are quite small. On average the uninsured are paying about $90 more in out-of-pocket expenditures, and the bulk of this difference is driven by payments made for doctor visits and dental visits. The privately insured tend to both spend more and use more services than both groups, except for, notably, their use of ER and inpatient services which are used at roughly the same rate or less than the baseline Medicaid rate.

In terms of demographic characteristics, the baseline uninsured sample is less likely to be white, more likely to have live in families where the head of household is unmarried and has a high school degree or less education. The uninsured family income is larger than the incomes of those on Medicaid yet lower than those with private insurance, which is consistent with the notion that these are children living in families with incomes too high to qualify for Medicaid but too low to afford private health insurance. These are the individuals we should think of as the treatment group in the analysis.

6.3 Econometric and Empirical Approach

To test the model described, the study exploits the massive increase in the Medicaid expansions occurring during the Balanced Budget Act of 1997. The CHIP/Medicaid expansions generate substantial variation in eligibility across states and years. I employ a standard differences-in-differences design exploiting within-state, within-year variation. Specifically, my main approach estimates the following:

|  |  |
| --- | --- |
| (5) |  |

refers to the CHIP or Medicaid income eligibility cutoff for children age in state and in year . Equation (5) includes state fixed-effects and year fixed effects to control for within-state variation, age of child dummy variables, in addition to individual demographic variables available in the MEPS. These estimations are run on the full sample of children in the MEPS for the years 1996-2006. To augment this analysis, I run extensive sub-group testing on individuals thought to substantially differ in terms of exposure to Medicaid expansions. The identifying assumption of this approach is that trends in out-of-pocket spending would be unaltered in the absence of Medicaid expansions.

Results

*Changes in Out-of-Pocket Spending*

The primary channel through which CHIP/Medicaid eligibility expansions are thought to impact individuals is through increased Medicaid coverage and reduced uninsured rates. There is also potential for “crowd-out” in which an increased role of publicly provided insurance reduces the number of individuals on private insurance. Previous studies have found a wide range of take-up and crowd-out rates with various samples. Appendix Table 1 provides analogous estimates in take-up and crowd-out in the MEPS sample. The table shows that the CHIP/Medicaid expansions reduced uninsured rates and increased Medicaid rates yet had little affect on private insurance, indicating that crowd-out is not thought to be a mechanism driving changes in out-of-pocket spending.

Table 2 presents the main results on out of pocket expenditures and changes healthcare utilization. Coefficients are interpreted as the effect of a $10,000 increase in the state-age-year Medicaid eligibility cutoff. Total out-of-pocket spending *increases* by approximately 7 dollars following Medicaid eligibility expansions. The Poisson model identifies the proportional change as about a 3.1 percent increase in out-of-pocket expenditures. The categories driving this finding are increased spending on outpatient services, prescription medicines, and dental services. Meanwhile, there is a reduction in spending among inpatient and ER categories. A Poisson analysis of the estimates indicate that there are large relative changes in outpatient, inpatient, and ER spending categories, while the observed increase in spending for drugs and dental care are proportionally small.

Table 2 further highlights whether changes in utilization are associated with any of these spending changes. The bottom half of Table 2 highlight the changes in healthcare utilization across categories. Along the intensive margin, there are no large changes observed. The number of doctor visits increase while inpatient and ER use are associated with small decreases, albeit none of these estimates are distinguishable from zero. This suggests the decrease in out-of-pocket spending for inpatient and emergency room care appears to be largely driven by the reduced price of care to beneficiaries rather than underlying changes in patient behaviors.

Prescription drug use, on the other hand, increases following Medicaid eligibility expansions. Following a $10,000 expansion, use of pharmaceuticals increases by approximately 6 percent. The effect along the intensive margin is similar to the estimate for whether there are any drugs prescribed, suggesting that all of the observed effect arises from those moving from the margin of 0 to 1 prescription medicines. This increase also attributes to the rise in out-of-pocket payments for prescription medicines as well.

Finally, personal expenditures on dental care, the largest source of out-of-pocket payments for children, also increases following Medicaid expansions. A $10,000 dollar expansion is associated with about a $10 increase in out-of-pocket payments for dental services. However, there are no observed changes in dental care utilization, indicating potential for a spurious correlation. (Footnote how the increase in spending on dental care is driven by increases among samples that are predicted to have low exposure to eligibility expansions).

*Heterogeneity Analysis*

There are four samples used in addition to the full sample of children. These samples are created using the baseline demographic means in Table 1 as a predictor for those likely and unlikely to be affected by Medicaid. These samples are divided along race (white vs. non-white) and head of household education (high school or less vs. greater than high school educated).

The full sample demonstrates a clear increased use in doctor visits and an increased in the likelihood of having prescription medicines. The heterogeneity analyses show larger magnitudes of these effects on both less educated head of household samples and non-white samples. The placebo samples demonstrate no corresponding effect. Notably, however, while there is an observed reduction in inpatient hospital stays, there is no significant decrease in ER visits. This suggests that while the newly eligible do indeed seek care thought to be more “efficient” in the office-based setting, it does not appear to be a substitute for emergency room care but rather *in addition* to emergency room care. This is consistent with previous estimates in the literature (Dafny and Gruber 2005; Finkelstein et al. 2012).

While Medicaid is observed here to have little effect on ER visits and a small negative impact on hospitalization rates, the insurance aspect of the program should drop the amount paid out of pocket for these services. However, as shown in Table 1 and as mentioned in Finkelstein et al. (2015), the low-income uninsured appear to pay very little for these services to begin with. Furthermore, the newly Medicaid eligible individuals have increased doctor visits and pharmaceutical utilization and so even a small co-pay for such services could *increase* the total out-of-pocket expenditures paid.[[12]](#footnote-12)

This is in fact the observed impact of eligibility expansions on out-of-pocket spending, presented in Table 3. The full sample and both of the treatment samples have small positive increases in total out-of-pocket spending on healthcare. There is statistically significant effect on the probability of having any out-of-pocket expenditures. The full sample experiences a 1.3 percentage point gain in the likelihood of out-of-pocket payments – again these magnitudes are larger in the samples predicted to be more exposed to the Medicaid expansions while small and insignificant in samples predicted to be less exposed to the expansions. However, the intensive margin – the number of dollars spent – estimates are small, on the order of magnitude of 5-20 dollars across samples. There is some movement along the intensive margin for the placebo samples, suggesting that, for example, whites paid about $18 more for healthcare services when states expand income eligibility cutoffs by $10,000.

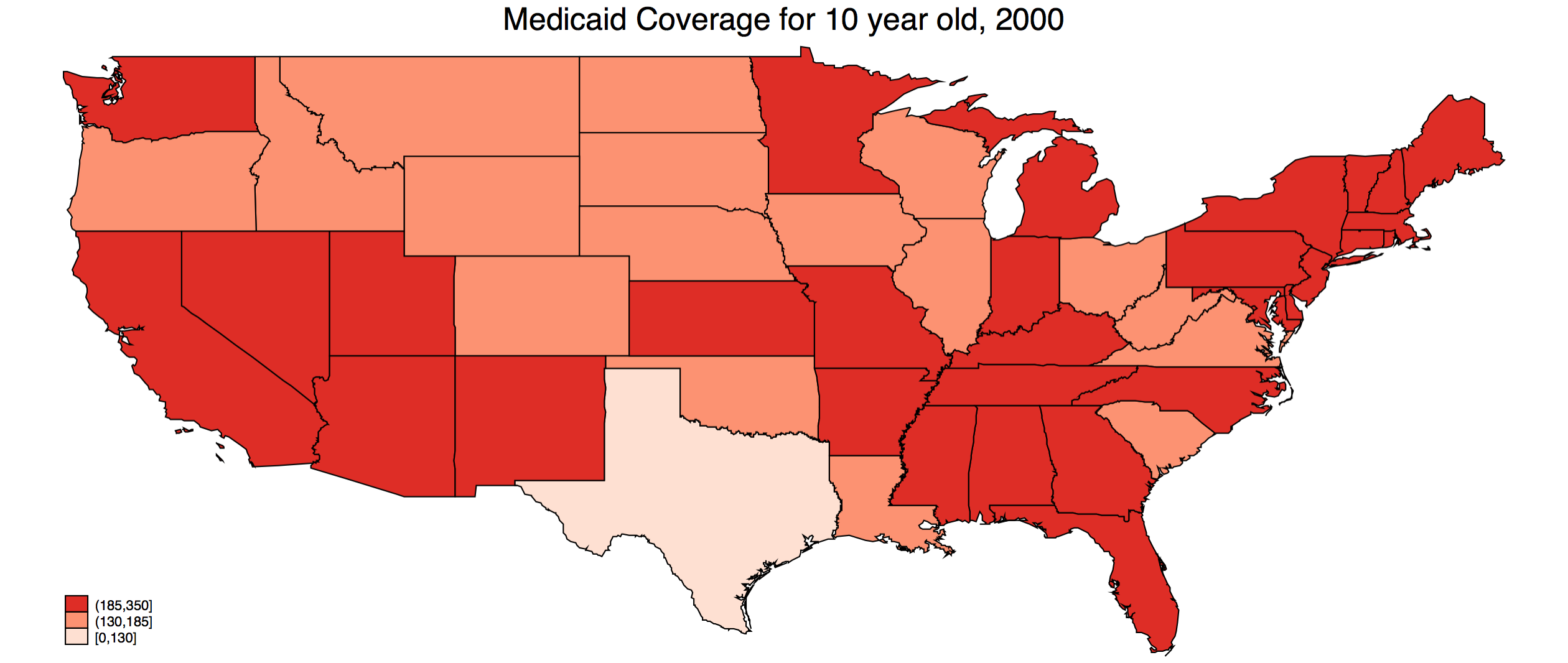
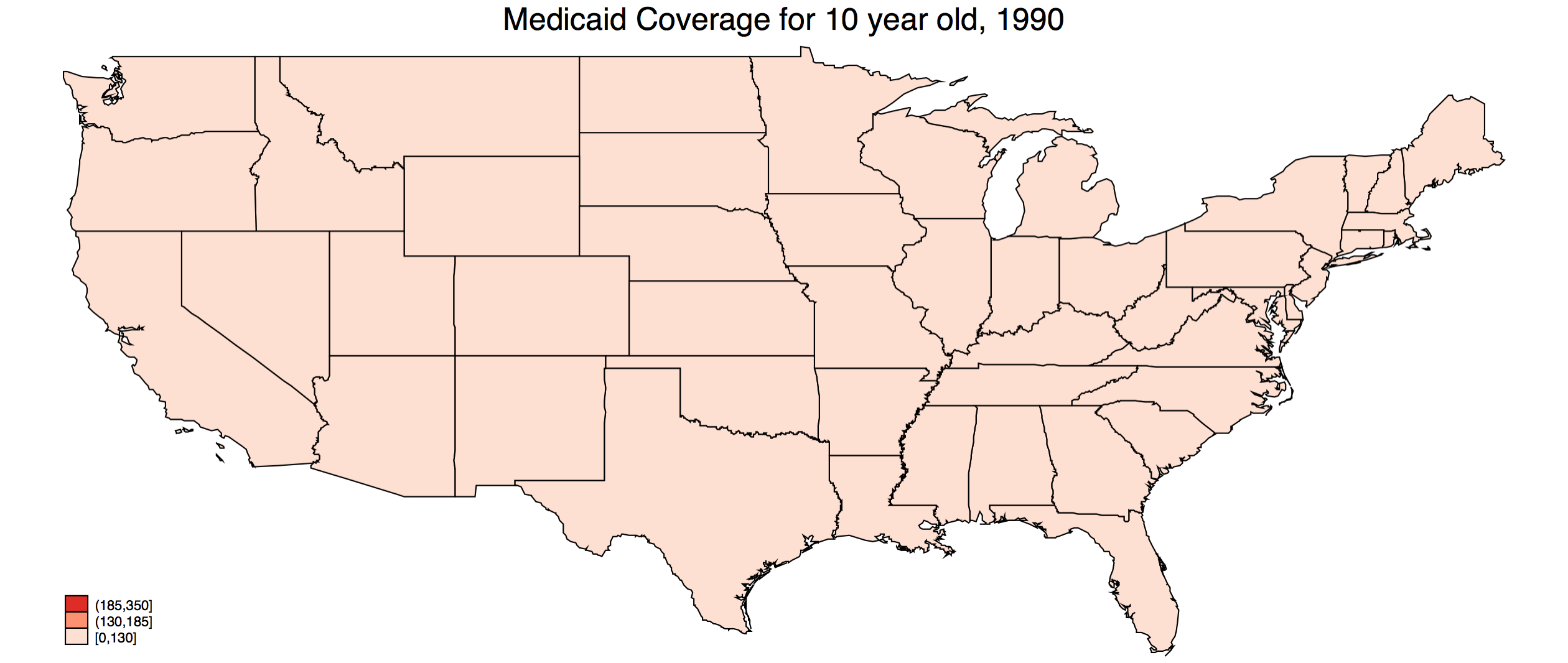
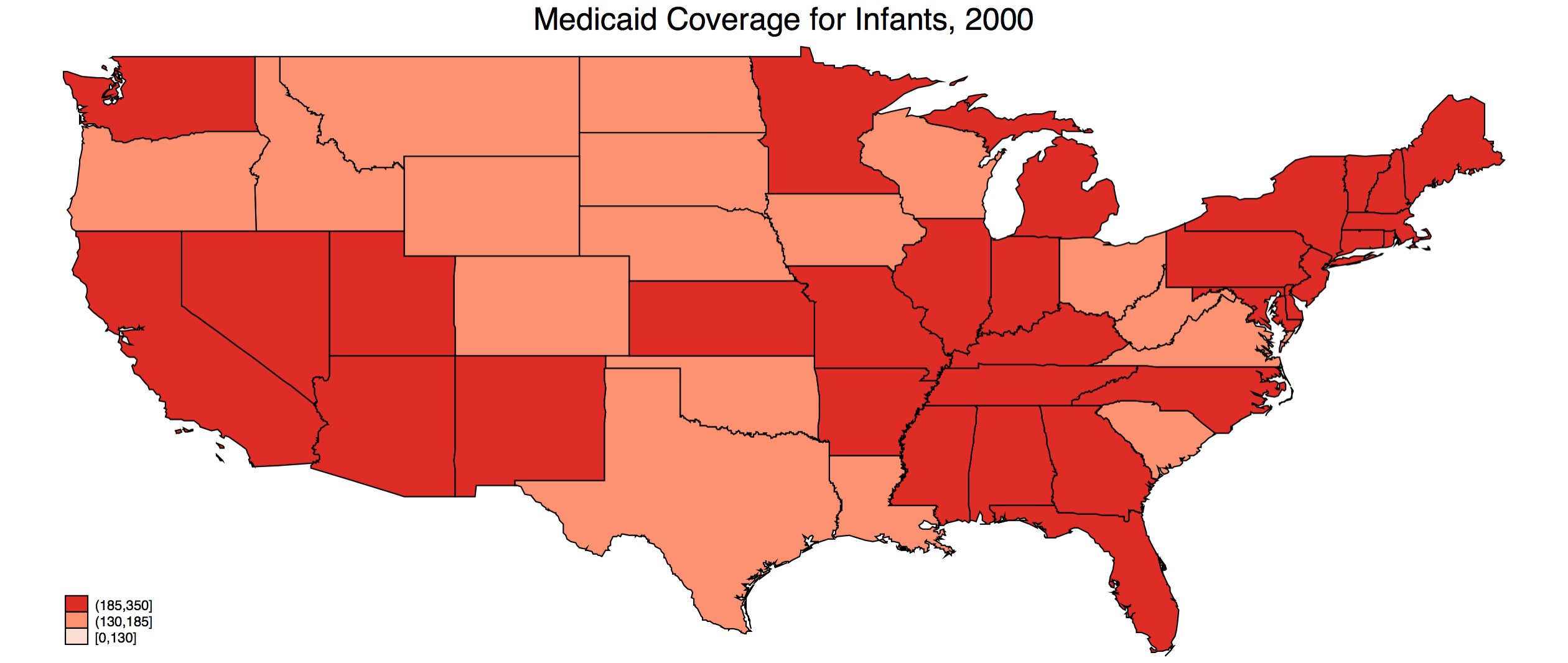
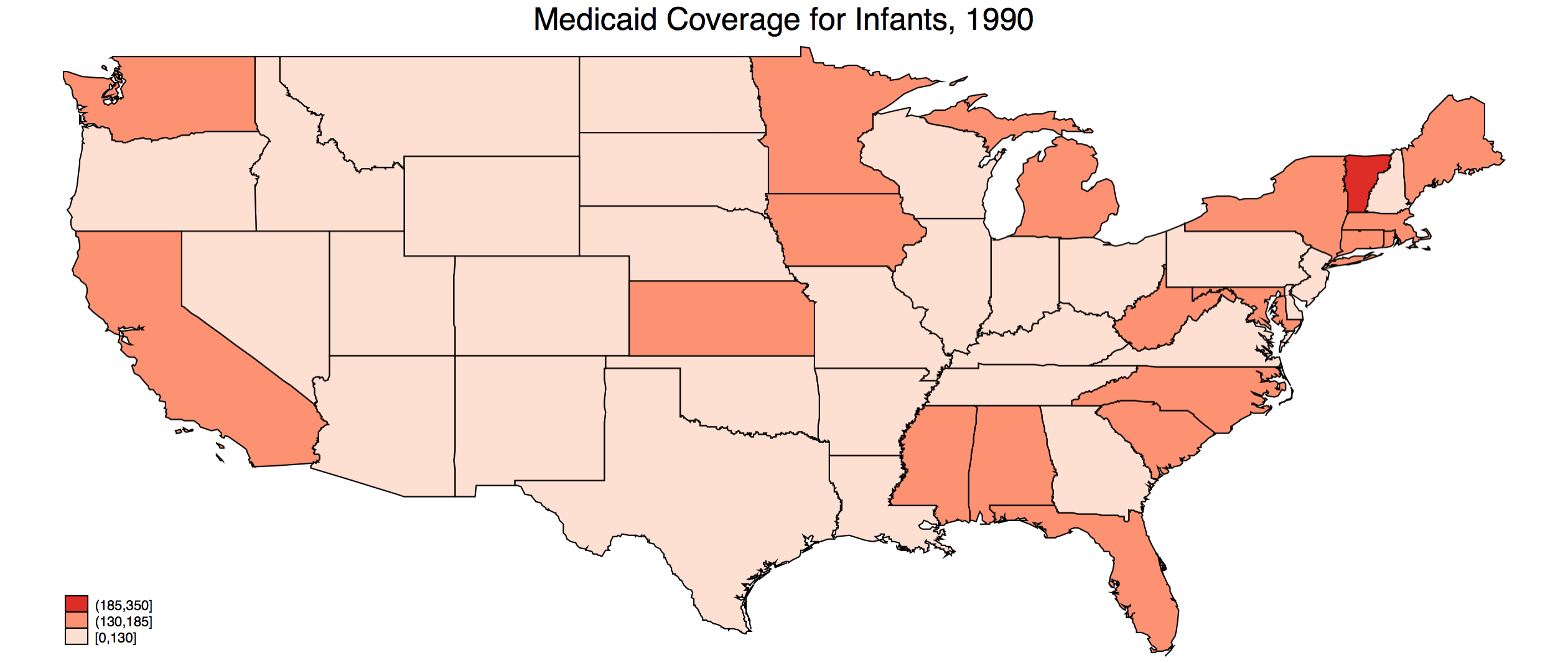
For the treatment samples, the increase in out-of-pocket costs is driven by an increase in payments in for prescription medicines and outpatient visits. The prescription medicines out-of-pocket increase coincides with the increase in medicine use observed in Table 3 – the magnitudes of the likelihood in any out-of-pocket prescription medicine payments is nearly equal to the magnitudes of the likelihood in any *use* of prescription medicines, which suggests that there beneficiaries were required to pay some co-payment for drugs. However, the very small magnitude changes along the intensive margin indicate that these co-payments are likely to be small.

The Medicaid expansions have a consistent negative relation with out-of-pocket spending in medical care delivery settings thought to wasteful, such as ER and inpatient out-of-pocket spending. However, as observed in the baseline statistics in Table 1, the low-income uninsured sample do not appear to spending a lot of their income on these categories to begin with. As a result, the magnitudes of the savings estimated in Table 4 are small and insignificant.

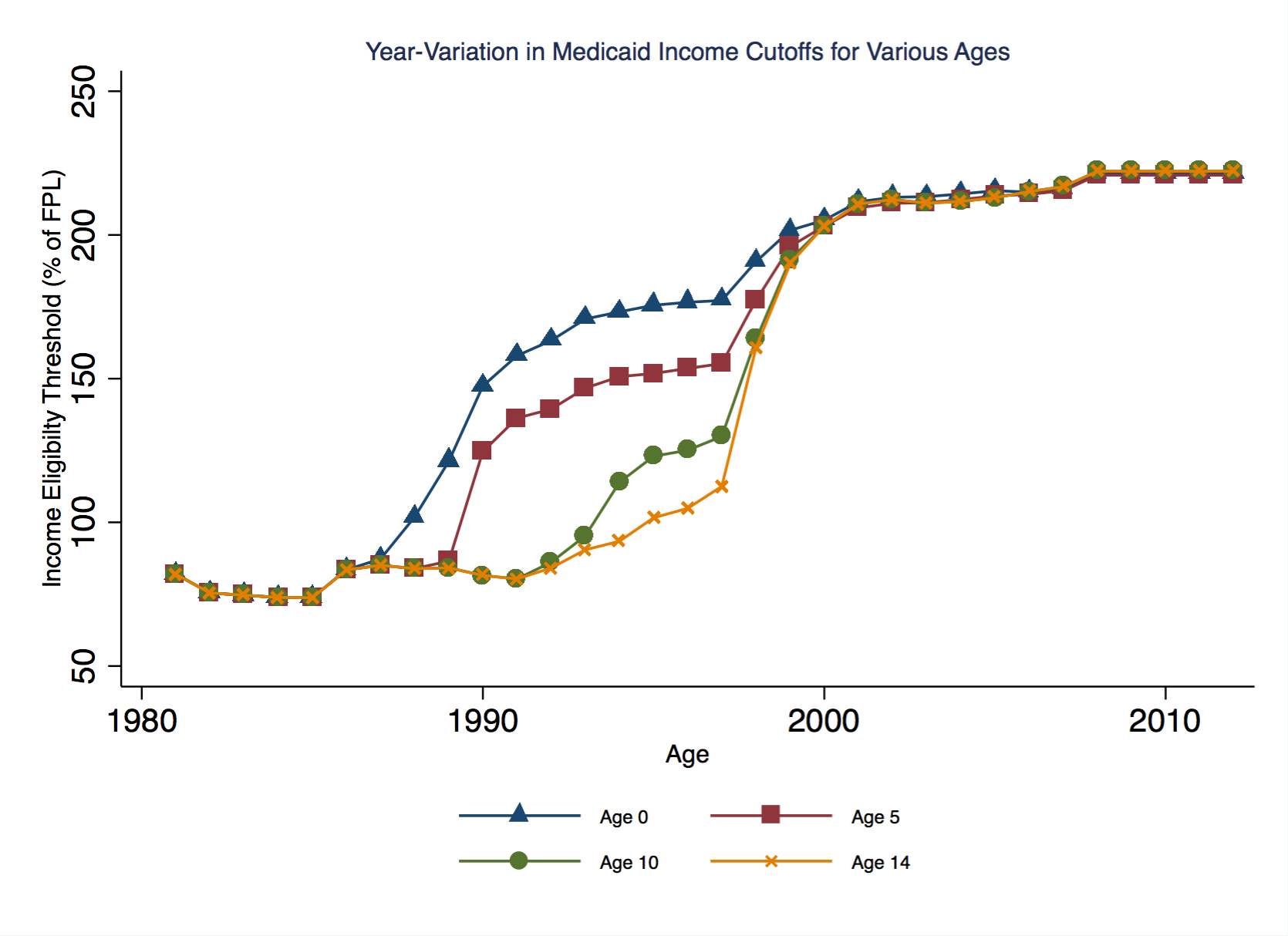
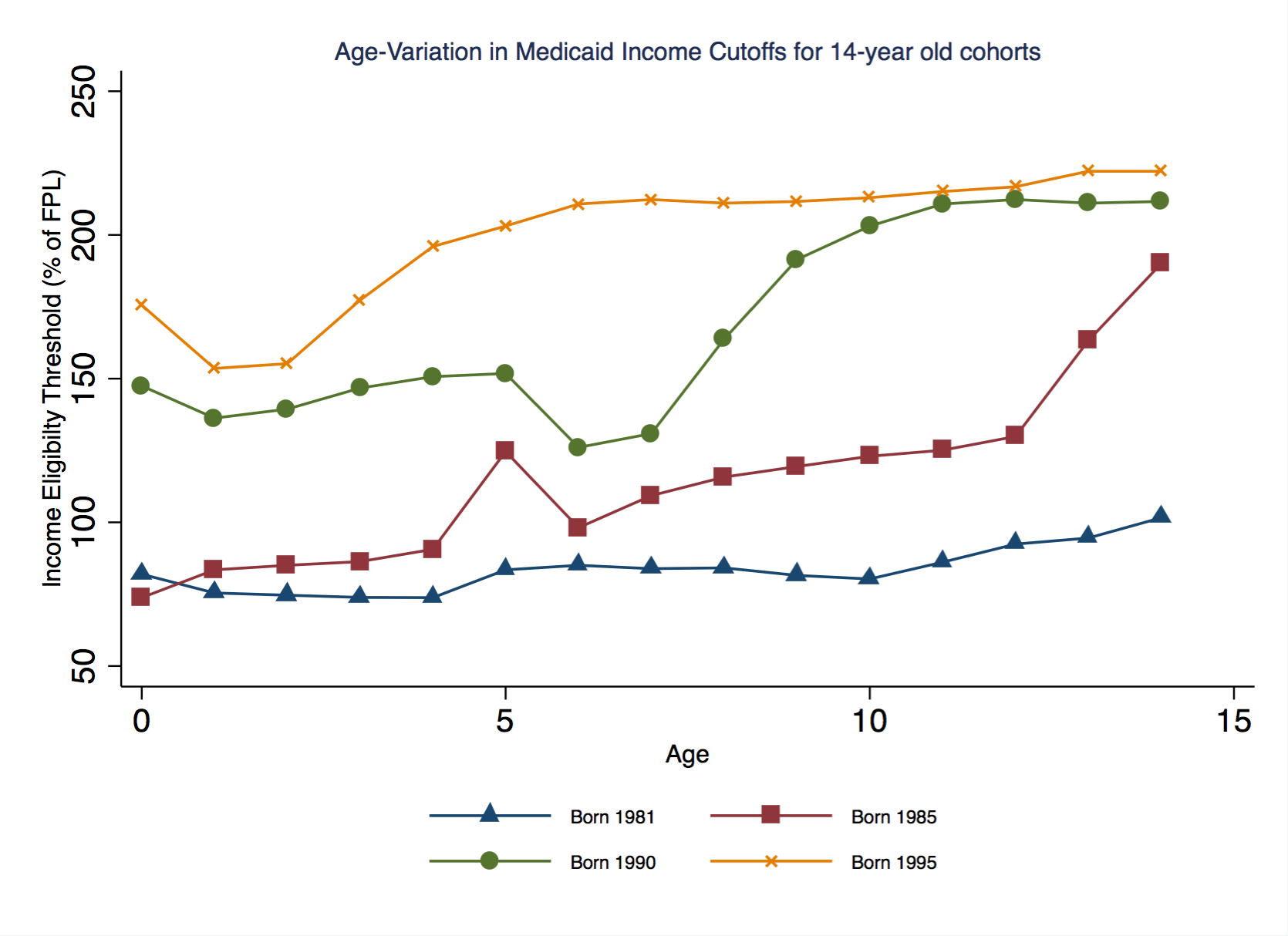
7. Conclusion

8. References

**Figure 1. State Variation in Medicaid Eligibility Cutoffs in 1990 and 2000**



**Figure 2. Age and Year Level Variation in Medicaid Eligibility**



**Figure 3 – Take-up Rates by Birth Month among Poor and Non-Poor Samples in the South (1992-1996)**

|  |  |
| --- | --- |
| **Panel A: Family Income <100%, South Sample** | |
|  |  |
| **Panel B: Family Income >100%, South Sample** | |
|  |  |

|  |  |
| --- | --- |
| **Table 1. Major Medicaid Expansion Mandates (1984-1997)** | |
|  | |
| **Mandate** | **Description** |
|  |  |
| *Deficit Reduction Act of 1984* | Children born after Sept. 30, 1983 and pregnant women eligible if income-eligible for AFDC |
| *OBRA 1987* | All children born after Sept. 30, 1983 are eligible if they meet state AFDC requirements |
| *Medicaid Catastrophic Coverage Act (1988)* | Pregnant Women and Children age 8 and under must be covered if below 75% of FPL |
| *Family Support Act (1988)* | Two-parent families with principal earner unemployed and meet income requirements are eligible |
| *OBRA 1989* | Pregnant women and children up to age 6 with incomes below 133% of FPL are made eligible |
| *OBRA 1990* | Children born after Sept. 30, 1983 and under 18 with incomes below 100% of FPL are made eligible |
| *Balanced Budget Act of 1997* | Establishes CHIP/Medicaid expansion funding - $40 billion made available to states specifically to address income and age expansions |

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| **Table 2.** | | | | | | |
| **Descriptive Statistics: NAEP Sample** | | | | | | |
|  | | | | | | |
|  | **4th Grade Sample** | | | **8th Grade Sample** | | |
|  | Full Sample | 0-25 % F/R Lunch | 75-100 % F/R Lunch | Full Sample | 0-25 % F/R Lunch | 75-100 % F/R Lunch |
|  |  |  |  |  |  |  |
| NAEP Percent of Block Questions Correct: Mathematics | 0.471  (0.213) | 0.539  (0.209) | 0.379  (0.194) | 0.482  (0.216) | 0.545  (0.213) | 0.379  (0.197) |
|  |  |  |  |  |  |  |
| NAEP Percent of Block Questions Correct: Reading | 0.472  (0.252) | 0.543  (0.241) | 0.361  (0.238) | 0.539  (0.243) | 0.590  (0.233) | 0.436  (0.239) |
|  |  |  |  |  |  |  |
| Missed Any School Past Month? | 0.50 | 0.46 | 0.52 | 0.56 | 0.54 | 0.59 |
| Missed 3 or more School Days Past Month? | 0.19 | 0.17 | 0.22 | 0.21 | 0.18 | 0.25 |
| Missed 5 or more School Days Past Month? | 0.07 | 0.06 | 0.09 | 0.07 | 0.06 | 0.09 |
| Missed 10 or more School Days Past Month? | 0.02 | 0.02 | 0.04 | 0.02 | 0.02 | 0.03 |
|  |  |  |  |  |  |  |
| Medicaid Income Cutoff at Birth | $23,703  (9,601) | $23,407  (10,526) | $24,394  (9,055) | $17,960  (8,274) | $17,252  (8,525) | $18,969  (8,113) |
| Medicaid Income Cutoff at Age 5 | $30,217  (10,985) | $30,423  (12,200) | $30,758  (10,286) | $23,578  (9,107) | $23,543  (9,792) | $23,793  ($8,535) |
| Medicaid Income Cutoff at Age 10 | $36,271  (12,368) | $36,173  (14,027) | $37,184  (11,632) | $31,901  (12,164) | $31,898  (13,543) | $32,788  (11,410) |
| Medicaid Income Cutoff at Age 14 |  |  |  | $37,261  (12,044) | $37,087  (13,574) | $38,435  (11,488) |
|  |  |  |  |  |  |  |
| Male | 0.50 | 0.51 | 0.49 | 0.50 | 0.50 | 0.48 |
| White | 0.54 | 0.73 | 0.18 | 0.58 | 0.76 | 0.15 |
| Black | 0.14 | 0.04 | 0.32 | 0.14 | 0.05 | 0.37 |
| Hispanic | 0.21 | 0.13 | 0.39 | 0.18 | 0.10 | 0.37 |
| Other Race | 0.11 | 0.10 | 0.11 | 0.10 | 0.09 | 0.11 |
| Age | 9.38  (0.48) | 9.35  (0.48) | 9.38  (0.49) | 13.39  (0.49) | 13.36  (0.48) | 13.41  (0.49) |
|  |  |  |  |  |  |  |
| School Poverty: Percentage of Students eligible for Free/Reduced Lunch |  |  |  |  |  |  |
| <25% | 0.32 | 1.00 | 0 | 0.33 | 1.00 | 0 |
| 25-50% | 0.28 | 0 | 0 | 0.34 | 0 | 0 |
| 50-75% | 0.20 | 0 | 0 | 0.19 | 0 | 0 |
| >75% | 0.20 | 0 | 1.00 | 0.15 | 0 | 1.00 |
|  |  |  |  |  |  |  |
| Observations | 1,728,609 | 524,262 | 360,592 | 1,368,545 | 450,672 | 199,492 |
|  |  |  |  |  |  |  |

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| **Table 3.**  **4th Grade Test Results by School Population Poverty** | | | | | | |
| ***Panel A****:* Student attends school with more than 75% of students eligible for free/reduced lunch | | | | | | |
|  | **Mathematics** | | | **Reading** | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | (3) | (1) | (2) | (3) |
| Ages (8-10) | 0.008\*\*  (0.003) | 0.009\*\*  (0.004) | 0.009\*\*  (0.004) | 0.003  (0.003) | -0.001  (0.004) | -0.001  (0.004) |
|  |  |  |  |  |  |  |
| Ages (4-7) |  | -0.002  (0.004) | -0.002  (0.004) |  | 0.010\*\*  (0.004) | 0.009\*\*  (0.004) |
|  |  |  |  |  |  |  |
| Ages (0-3) |  |  | -0.001  (0.005) |  |  | 0.005  (0.006) |
|  |  |  |  |  |  |  |
| F-Test: Joint Significance of Cutoffs |  | 2.82  (0.07) | 2.09  (0.11) |  | 4.24  (0.02) | 3.76  (0.02) |
|  |  |  |  |  |  |  |
| Obs. | 424,801 | 424,801 | 424,801 | 356,679 | 356,679 | 356,679 |
| Mean | 0.379 | 0.379 | 0.379 | 0.361 | 0.361 | 0.361 |
|  |  |  |  |  |  |  |
| ***Panel B****:* Student Attends school with less than 25% of students eligible for free/reduced lunch | | | | | | |
|  | **Mathematics** | | | **Reading** | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | (3) | (1) | (2) | (3) |
| Ages (8-10) | -0.002  (0.002) | -0.003  (0.002) | -0.004  (0.002) | 0.000  (0.003) | -0.000  (0.003) | -0.001  (0.003) |
|  |  |  |  |  |  |  |
| Ages (4-7) |  | 0.002  (0.003) | 0.004  (0.003) |  | 0.001  (0.003) | 0.003  (0.003) |
|  |  |  |  |  |  |  |
| Ages (0-3) |  |  | -0.004  (0.003) |  |  | -0.005  (0.003) |
|  |  |  |  |  |  |  |
| F-Test: Joint Significance of Cutoffs |  | 1.42  (0.25) | 1.79  (0.16) |  | 0.02  (0.98) | 1.08  (0.37) |
|  |  |  |  |  |  |  |
| Obs. | 635,888 | 635,888 | 635,888 | 646,077 | 646,077 | 646,077 |
| Mean | 0.539 | 0.539 | 0.539 | 0.541 | 0.541 | 0.541 |
| *Notes:* Data from NAEP. Outcome is percentage of correctly answered questions within question block (detailed in data section). Estimates report the coefficient on Medicaid income eligibility cutoff level and are interpreted as a $10,000 change. Regressions use state, year, age, and block fixed effects. Eligibility in each age bin is the average income eligibility cutoff for a family of four across indicated ages. Further control using race and sex indicators. State-level variables (calculated from Current Population Surveys): percent of population aged 0-18, percent of population aged 18-64, percent of population aged 65 or older, average income, percent of population married, percent of population never married, percent of population separated/divorced/widowed, percent of population white, black, or Hispanic, percent of population on welfare and percent of population with household members on food stamps. Standard errors are clustered at the state level and reported in parentheses. P-values from F-tests presented in parentheses. 4th Grade math analysis uses 1992, 1996, 2000, 2003, 2005, 2007 and 2009 assessments. 4th Grade reading analysis uses 1992, 1994, 1998, 2002, 2003, 2005, 2007, and 2009 assessments. \* represents significance at the 5% level. | | | | | | |

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| **Table 4.**  **8th Grade Test Results by School Population Poverty** | | | | | | | | | | | | | | | |
| ***Panel A****:* Student attends school with more than 75% of students eligible for free/reduced lunch | | | | | | | | | | | | | | | |
|  | **Mathematics** | | | | | | | **Reading** | | | | | | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | | (3) | | (4) | | (1) | | (2) | | (3) | | (4) | |
| Ages (11-14) | 0.009\*\*  (0.003) | 0.008\*\*  (0.003) | | 0.007\*\*  (0.003) | | 0.007\*\*  (0.003) | | 0.005  (0.003) | | 0.004  (0.003) | | 0.004  (0.003) | | 0.004  (0.003) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (8-10) |  | 0.005  (0.004) | | 0.006  (0.004) | | 0.006  (0.004) | |  | | 0.005  (0.003) | | 0.005  (0.003) | | 0.002  (0.003) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (4-7) |  |  | | -0.002  (0.004) | | -0.003  (0.004) | |  | |  | | -0.002  (0.004) | | -0.001  (0.004) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (0-3) |  |  | |  | | 0.004  (0.006) | |  | |  | |  | | -0.016\*\*  (0.008) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| F-Test: Joint Significance of Cutoffs |  | 3.81  (0.03) | | 3.31  (0.03) | | 2.66  (0.04) | |  | | 2.60  (0.08) | | 2.31  (0.09) | | 4.51  (0.003) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Obs. | 226,626 | 226,626 | | 226,626 | | 226,626 | | 169,311 | | 169,311 | | 169,311 | | 169,311 | |
| Mean | 0.376 | 0.376 | | 0.376 | | 0.376 | | 0.428 | | 0.428 | | 0.428 | | 0.428 | |
|  |  | |  | |  | |  | |  | |  | |  | |  |
| ***Panel B****:* Student Attends school with less than 25% of students eligible for free/reduced lunch | | | | | | | | | | | | | | | |
|  | **Mathematics** | | | | | | | **Reading** | | | | | | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | | (3) | | (4) | | (1) | | (2) | | (3) | | (4) | |
| Ages (11-14) | -0.003  (0.002) | -0.003  (0.002) | | -0.003  (0.002) | | -0.003  (0.002) | | 0.001  (0.003) | | 0.001  (0.003) | | 0.001  (0.003) | | 0.001  (0.003) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (8-10) |  | -0.001  (0.003) | | -0.001  (0.003) | | 0.002  (0.001) | |  | | -0.002  (0.003) | | -0.001  (0.003) | | -0.002  (0.003) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (4-7) |  |  | | -0.001  (0.003) | | -0.004  (0.004) | |  | |  | | -0.002  (0.002) | | -0.002  (0.003) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (0-3) |  |  | |  | | 0.008  (0.006) | |  | |  | |  | | -0.001  (0.004) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| F-Test: Joint Significance of Cutoffs |  | 1.45  (0.24) | | 0.98  (0.41) | | 1.08  (0.38) | |  | | 0.38  (0.69) | | 0.42  (0.74) | | 0.31  (0.87) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Obs. | 522,667 | 522,667 | | 522,667 | | 522,667 | | 407,496 | | 407,496 | | 407,496 | | 407,496 | |
| Mean | 0.541 | 0.541 | | 0.541 | | 0.541 | | 0.583 | | 0.583 | | 0.583 | | 0.583 | |
| *Notes:* Data from NAEP. Outcome is percentage of correctly answered questions within question block (detailed in data section). Estimates report the coefficient on Medicaid income eligibility cutoff level and are interpreted as a $10,000 change. Regressions use identical specifications to Table 3 Standard errors are clustered at the state level and reported in parentheses. P-values from F-tests presented in parentheses. 8th Grade math analysis uses 1996, 2000, 2003, 2005, 2007 and 2009 assessments. 8th Grade reading analysis uses 1998, 2002, 2003, 2005, 2007, and 2009 assessments. | | | | | | | | | | | | | | | |

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| **Table 5.**  **Cumulative Medicaid Exposure and NAEP Mathematics Assessments** | | | | | |
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| **4th Grade** | | | **8th Grade** | | |
| Eligibility at Age Range | 75-100% F.L. | 0-25% F.L. | Eligibility at Age Range | 75-100% F.L. | 0-25% F.L. |
| Ages 8 -10 | 0.008\*\*  (0.003) | -0.002  (0.002) | Ages 12-14 | 0.009\*\*  (0.003) | -0.003  (0.002) |
| Ages 6 - 10 | 0.009\*\*  (0.004) | -0.002  (0.002) | Ages 8 - 14 | 0.013\*\*  (0.005) | -0.004  (0.003) |
| Ages 3 - 10 | 0.007  (0.004) | -0.002  (0.002) | Ages 5 - 14 | 0.014\*\*  (0.006) | -0.005  (0.003) |
| Ages 0 - 10 | 0.007  (0.005) | -0.003  (0.003) | Ages 3 - 14 | 0.015  (0.008) | -0.005  (0.004) |
|  |  |  | Ages 0 - 14 | 0.018  (0.009) | -0.004  (0.005) |
| Observations | 424,801 | 635,888 | Observations | 226,626 | 522,667 |
| Mean | 0.379 | 0.539 | Mean | 0.375 | 0.540 |
| *Notes:* Data from NAEP. Outcome is percentage of questions student answered correctly within question block (detailed in data section). Estimates report the coefficient on Medicaid income eligibility cutoff level and are interpreted as a $10,000 change. Each cell represents one regression result. Regression specifications are identical to those indicated in Table 3. 0-25% F.L. identifies students that attend a school in which 0-25% of their peers are on free or reduced lunch. Standard errors are clustered at the state level and reported in parentheses. 4th Grade analysis uses 1992, 1996, 2000, 2003, 2005, 2007, and 2009 assessments. 8th Grade analysis uses 1996, 2000, 2003, 2005, 2007, 2009 assessments. \* represents significance at the 5% level. | | | | | |

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| **Table 6.**  **Cumulative Medicaid Exposure and NAEP Reading Assessments** | | | | | |
|  | | |  | | |
| **4th Grade** | | | **8th Grade** | | |
| Eligibility at Age Range | 75-100% F.L. | 0-25% F.L. | Eligibility at Age Range | 75-100% F.L. | 0-25% F.L. |
| Ages 8 -10 | 0.003  (0.003) | 0.000  (0.003) | Ages 12-14 | 0.005  (0.003) | 0.001  (0.003) |
| Ages 6 - 10 | 0.005  (0.004) | 0.001  (0.003) | Ages 8 - 14 | 0.010\*\*  (0.003) | -0.001  (0.004) |
| Ages 3 - 10 | 0.010\*\*  (0.004) | 0.000  (0.004) | Ages 5 - 14 | 0.011\*\*  (0.005) | -0.002  (0.004) |
| Ages 0 - 10 | 0.013\*\*  (0.005) | -0.001  (0.005) | Ages 3 - 14 | 0.008  (0.006) | -0.003  (0.004) |
|  |  |  | Ages 0 - 14 | 0.003  (0.008) | -0.004  (0.005) |
| Observations | 356,679 | 646,077 | Observations | 169,311 | 407,496 |
| Mean | 0.361 | 0.541 | Mean | 0.428 | 0.583 |
| *Notes:* Data from NAEP. Outcome is percentage of correctly answered questions within question block (detailed in data section). Estimates report the coefficient on Medicaid income eligibility cutoff level and are interpreted as a $10,000 change. Each cell represents one regression result. Regression specifications are identical to those indicated in Table 3. 0-25% F.L. identifies students that attend a school in which 0-25% of their peers are on free or reduced lunch. Standard errors are clustered at the state level and reported in parentheses. 4th Grade Analysis uses 1992, 1994, 1998, 2002, 2003, 2005, 2007 and 2009 assessments. 8th Grade analysis uses 1998, 2000, 2003, 2005, 2007, and 2009 assessments. \* represents significance at the 5% level. | | | | | |

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| **Table 7.**  **4th Attendance Results by School Population Poverty** | | | | | | |
| ***Panel A****:* Student attends school with more than 75% of students eligible for free/reduced lunch | | | | | | |
|  | **Miss Any School in Past Month?** | | | **Miss more than 3 days in Past Month?** | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | (3) | (1) | (2) | (3) |
| Ages (8-10) | -0.004  (0.003) | -0.001  (0.003) | -0.002  (0.004) | -0.005  (0.003) | -0.002  (0.003) | -0.003  (0.003) |
|  |  |  |  |  |  |  |
| Ages (4-7) |  | -0.009\*\*  (0.004) | -0.008  (0.005) |  | -0.011\*\*  (0.004) | -0.009  (0.004) |
|  |  |  |  |  |  |  |
| Ages (0-3) |  |  | -0.005  (0.005) |  |  | -0.007  (0.005) |
|  |  |  |  |  |  |  |
| F-Test: Joint Significance of Cutoffs |  | 2.48  (0.09) | 1.93  (0.14) |  | 4.71  (0.01) | 3.34  (0.03) |
|  |  |  |  |  |  |  |
| Obs. | 360,592 | 360,592 | 360,592 | 360,592 | 360,592 | 360,592 |
| Mean | 0.52 | 0.52 | 0.52 | 0.23 | 0.23 | 0.23 |
|  |  |  |  |  |  |  |
| ***Panel B****:* Student Attends school with less than 25% of students eligible for free/reduced lunch | | | | | | |
|  | **Miss Any School in Past Month?** | | | **Miss more than 3 days in Past Month?** | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | (3) | (1) | (2) | (3) |
| Ages (8-10) | 0.003  (0.002) | 0.005  (0.003) | 0.004  (0.003) | -0.001  (0.002) | 0.000  (0.002) | -0.000  (0.004) |
|  |  |  |  |  |  |  |
| Ages (4-7) |  | -0.004  (0.004) | -0.002  (0.005) |  | -0.004  (0.003) | -0.003  (0.004) |
|  |  |  |  |  |  |  |
| Ages (0-3) |  |  | -0.005  (0.004) |  |  | -0.003  (0.003) |
|  |  |  |  |  |  |  |
| F-Test: Joint Significance of Cutoffs |  | 1.40  (0.26) | 1.63  (0.19) |  | 1.67  (0.20) | 1.62  (0.20) |
|  |  |  |  |  |  |  |
| Obs. | 524,262 | 524,262 | 524,262 | 524,262 | 524,262 | 524,262 |
| Mean | 0.46 | 0.46 | 0.46 | 0.17 | 0.17 | 0.17 |
| *Notes:* Data from NAEP. Unit of analysis is at the individual level. Regressions use identical specification used in Table 3. Estimates report the coefficient on Medicaid income eligibility cutoff level and are interpreted as a $10,000 change. Standard errors are clustered at the state level and reported in parentheses. P-values from F-tests presented in parentheses. 4th Grade math analysis uses 1992, 1996, 2000, 2003, 2005, 2007 and 2009 assessments. 4th Grade reading analysis uses 1992, 1994, 1998, 2002, 2003, 2005, 2007, and 2009 assessments. \* represents significance at the 5% level. | | | | | | |

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| **Table 8.**  **4th Attendance Results by School Population Poverty** | | | | | | |
| ***Panel A****:* Student attends school with more than 75% of students eligible for free/reduced lunch | | | | | | |
|  | **Miss more than 5 days in Past Month?** | | | **Miss more than 10 days in Past Month?** | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | (3) | (1) | (2) | (3) |
| Ages (8-10) | -0.001  (0.002) | 0.001  (0.002) | 0.000  (0.002) | -0.002\*\*  (0.001) | -0.001  (0.002) | -0.001  (0.001) |
|  |  |  |  |  |  |  |
| Ages (4-7) |  | -0.007\*\*  (0.002) | -0.006\*\*  (0.002) |  | -0.003\*\*  (0.001) | -0.003\*\*  (0.001) |
|  |  |  |  |  |  |  |
| Ages (0-3) |  |  | -0.005\*\*  (0.003) |  |  | -0.001  (0.002) |
|  |  |  |  |  |  |  |
| F-Test: Joint Significance of Cutoffs |  | 6.53  (0.003) | 6.65  (0.001) |  | 4.60  (0.01) | 2.87  (0.05) |
|  |  |  |  |  |  |  |
| Obs. | 360,592 | 360,592 | 360,592 | 360,592 | 360,592 | 360,592 |
| Mean | 0.09 | 0.09 | 0.09 | 0.04 | 0.04 | 0.04 |
|  |  |  |  |  |  |  |
| ***Panel B****:* Student Attends school with less than 25% of students eligible for free/reduced lunch | | | | | | |
|  | **Miss more than 5 days in Past Month?** | | | **Miss more than 10 days in Past Month?** | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | (3) | (1) | (2) | (3) |
| Ages (8-10) | -0.003\*\*  (0.001) | -0.002  (0.001) | -0.002  (0.001) | -0.001\*\*  (0.000) | -0.000  (0.001) | -0.001  (0.000) |
|  |  |  |  |  |  |  |
| Ages (4-7) |  | -0.003\*\*  (0.001) | -0.002  (0.001) |  | -0.001\*\*  (0.000) | -0.001  (0.001) |
|  |  |  |  |  |  |  |
| Ages (0-3) |  |  | -0.002  (0.002) |  |  | -0.001  (0.001) |
|  |  |  |  |  |  |  |
| F-Test: Joint Significance of Cutoffs |  | 8.73  (0.001) | 6.61  (0.001) |  | 5.56  (0.01) | 3.32  (0.03) |
|  |  |  |  |  |  |  |
| Obs. | 524,262 | 524,262 | 524,262 | 524,262 | 524,262 | 524,262 |
| Mean | 0.24 | 0.24 | 0.24 | 0.02 | 0.02 | 0.02 |
| *Notes:* Data from NAEP. Unit of analysis is at the individual level. Regressions use identical specification used in Table 3. Estimates report the coefficient on Medicaid income eligibility cutoff level and are interpreted as a $10,000 change. Standard errors are clustered at the state level and reported in parentheses. P-values from F-tests presented in parentheses. 4th Grade math analysis uses 1992, 1996, 2000, 2003, 2005, 2007 and 2009 assessments. 4th Grade reading analysis uses 1992, 1994, 1998, 2002, 2003, 2005, 2007, and 2009 assessments. \* represents significance at the 5% level. | | | | | | |

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| **Table 9.**  **8th Grade Attendance Results by School Population Poverty** | | | | | | | | | | | | | | | |
| ***Panel A****:* Student attends school with more than 75% of students eligible for free/reduced lunch | | | | | | | | | | | | | | | |
|  | **Miss Any School in Past Month?** | | | | | | | **Miss more than 3 days in Past Month?** | | | | | | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | | (3) | | (4) | | (1) | | (2) | | (3) | | (4) | |
| Ages (11-14) | -0.002  (0.004) | -0.001  (0.004) | | -0.001  (0.004) | | -0.001  (0.004) | | -0.002  (0.003) | | -0.000  (0.003) | | -0.001  (0.003) | | -0.001  (0.003) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (8-10) |  | -0.008  (0.006) | | -0.007  (0.006) | | -0.006  (0.006) | |  | | -0.010  (0.005) | | -0.009  (0.005) | | -0.009  (0.006) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (4-7) |  |  | | -0.002  (0.008) | | -0.003  (0.008) | |  | |  | | -0.002  (0.007) | | -0.003  (0.007) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (0-3) |  |  | |  | | 0.006  (0.012) | |  | |  | |  | | 0.005  (0.008) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| F-Test: Joint Significance of Cutoffs |  | 0.93  (0.40) | | 0.62  (0.61) | | 0.46  (0.77) | |  | | 1.89  (0.16) | | 1.25  (0.30) | | 1.11  (0.36) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Obs. | 199,492 | 199,492 | | 199,492 | | 199,492 | | 199,492 | | 199,492 | | 199,492 | | 199,492 | |
| Mean | 0.59 | 0.59 | | 0.59 | | 0.59 | | 0.25 | | 0.25 | | 0.25 | | 0.25 | |
|  |  | |  | |  | |  | |  | |  | |  | |  |
| ***Panel B****:* Student Attends school with less than 25% of students eligible for free/reduced lunch | | | | | | | | | | | | | | | |
|  | **Miss Any School in Past Month?** | | | | | | | **Miss more than 3 days in Past Month?** | | | | | | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | | (3) | | (4) | | (1) | | (2) | | (3) | | (4) | |
| Ages (11-14) | 0.007\*\*  (0.003) | 0.010\*\*  (0.003) | | 0.010\*\*  (0.003) | | 0.010\*\*  (0.003) | | 0.006\*\*  (0.002) | | 0.007\*\*  (0.002) | | 0.006\*\*  (0.002) | | 0.006\*\*  (0.002) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (8-10) |  | -0.008\*\*  (0.003) | | -0.008\*\*  (0.003) | | -0.009\*\*  (0.003) | |  | | -0.004  (0.003) | | -0.002  (0.003) | | -0.003  (0.003) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (4-7) |  |  | | 0.001  (0.004) | | 0.002  (0.004) | |  | |  | | -0.003  (0.004) | | -0.003  (0.003) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (0-3) |  |  | |  | | -0.003  (0.005) | |  | |  | |  | | -0.001  (0.004) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| F-Test: Joint Significance of Cutoffs |  | 7.15  (0.002) | | 5.34  (0.002) | | 4.16  (0.01) | |  | | 4.67  (0.01) | | 3.25  (0.03) | | 2.44  (0.06) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Obs. | 450,672 | 450,672 | | 450,672 | | 450,672 | | 450,672 | | 450,672 | | 450,672 | | 450,672 | |
| Mean | 0.54 | 0.54 | | 0.54 | | 0.54 | | 0.18 | | 0.18 | | 0.18 | | 0.18 | |
| *Notes:* Data from NAEP. Unit of analysis is at the individual level. Regressions use identical specification used in Table 3. Estimates report the coefficient on Medicaid income eligibility cutoff level and are interpreted as a $10,000 change. Standard errors are clustered at the state level and reported in parentheses. P-values from F-tests presented in parentheses. 8th Grade math analysis uses 1996, 2000, 2003, 2005, 2007 and 2009 assessments. 8th Grade reading analysis uses 1998, 2002, 2003, 2005, 2007, and 2009 assessments. \* represents significance at the 5% level. | | | | | | | | | | | | | | | |

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| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Table 10.**  **8th Grade Attendance Results by School Population Poverty** | | | | | | | | | | | | | | | |
| ***Panel A****:* Student attends school with more than 75% of students eligible for free/reduced lunch | | | | | | | | | | | | | | | |
|  | **Miss more than 5 days in Past Month?** | | | | | | | **Miss more than 10 days in Past Month?** | | | | | | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | | (3) | | (4) | | (1) | | (2) | | (3) | | (4) | |
| Ages (11-14) | -0.003  (0.003) | -0.003  (0.003) | | -0.002  (0.003) | | -0.002  (0.003) | | -0.003\*\*  (0.001) | | -0.003\*\*  (0.001) | | -0.002\*\*  (0.001) | | -0.002\*\*  (0.001) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (8-10) |  | -0.001  (0.003) | | -0.002  (0.003) | | -0.002  (0.004) | |  | | -0.000  (0.002) | | -0.001  (0.002) | | -0.001  (0.002) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (4-7) |  |  | | 0.002  (0.004) | | 0.003  (0.004) | |  | |  | | 0.001  (0.001) | | 0.001  (0.001) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (0-3) |  |  | |  | | -0.003  (0.005) | |  | |  | |  | | -0.003  (0.002) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| F-Test: Joint Significance of Cutoffs |  | 0.69  (0.50) | | 0.63  (0.60) | | 0.54  (0.71) | |  | | 3.73  (0.03) | | 2.61  (0.06) | | 3.35  (0.02) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Obs. | 199,492 | 199,492 | | 199,492 | | 199,492 | | 199,492 | | 199,492 | | 199,492 | | 199,492 | |
| Mean | 0.09 | 0.09 | | 0.09 | | 0.09 | | 0.03 | | 0.03 | | 0.03 | | 0.03 | |
|  |  | |  | |  | |  | |  | |  | |  | |  |
| ***Panel B****:* Student Attends school with less than 25% of students eligible for free/reduced lunch | | | | | | | | | | | | | | | |
|  | **Miss more than 5 days in Past Month?** | | | | | | | **Miss more than 10 days in Past Month?** | | | | | | | |
| Avg. Medicaid Income Cutoff for Age Range | (1) | (2) | | (3) | | (4) | | (1) | | (2) | | (3) | | (4) | |
| Ages (11-14) | 0.002  (0.001) | 0.003\*\*  (0.001) | | 0.002  (0.001) | | 0.002  (0.001) | | 0.000  (0.0004) | | 0.000  (0.0004) | | -0.000  (0.0004) | | -0.000  (0.0004) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (8-10) |  | -0.002  (0.002) | | 0.001  (0.002) | | 0.001  (0.002) | |  | | 0.000  (0.001) | | 0.000  (0.001) | | 0.000  (0.001) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (4-7) |  |  | | -0.005\*\*  (0.002) | | -0.005\*\*  (0.002) | |  | |  | | -0.001  (0.001) | | -0.001  (0.001) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Ages (0-3) |  |  | |  | | 0.002  (0.003) | |  | |  | |  | | -0.001  (0.001) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| F-Test: Joint Significance of Cutoffs |  | 2.46  (0.10) | | 5.25  (0.003) | | 4.78  (0.002) | |  | | 0.01  (0.99) | | 0.37  (0.77) | | 0.31  (0.87) | |
|  |  |  | |  | |  | |  | |  | |  | |  | |
| Obs. | 450,672 | 450,672 | | 450,672 | | 450,672 | | 450,672 | | 450,672 | | 450,672 | | 450,672 | |
| Mean | 0.06 | 0.06 | | 0.06 | | 0.06 | | 0.02 | | 0.02 | | 0.02 | | 0.02 | |
| *Notes:* Data from NAEP. Unit of analysis is at the individual level. Regressions use identical specification used in Table 3. Estimates report the coefficient on Medicaid income eligibility cutoff level and are interpreted as a $10,000 change. Standard errors are clustered at the state level and reported in parentheses. P-values from F-tests presented in parentheses. 8th Grade math analysis uses 1996, 2000, 2003, 2005, 2007 and 2009 assessments. 8th Grade reading analysis uses 1998, 2002, 2003, 2005, 2007, and 2009 assessments. \* represents significance at the 5% level. | | | | | | | | | | | | | | | |

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| **Table 11.**  **Cumulative Medicaid Exposure and School Attendance: Missed more than 10 days of school in past month?** | | | | | |
|  | | |  | | |
| **4th Grade** | | | **8th Grade** | | |
| Eligibility at Age Range | 75-100% F.L. | 0-25% F.L. | Eligibility at Age Range | 75-100% F.L. | 0-25% F.L. |
| Ages 8 -10 | -0.002\*\*  (0.001) | -0.001\*\*  (0.0003) | Ages 12-14 | -0.003\*\*  (0.001) | 0.000  (0.0004) |
| Ages 6 - 10 | -0.003\*\*  (0.001) | -0.001\*\*  (0.0004) | Ages 8 - 14 | -0.003\*\*  (0.002) | 0.000  (0.001) |
| Ages 3 - 10 | -0.005\*\*  (0.002) | -0.002\*\*  (0.001) | Ages 5 - 14 | -0.003  (0.002) | 0.000  (0.001) |
| Ages 0 - 10 | -0.006\*\*  (0.002) | -0.002\*\*  (0.001) | Ages 3 - 14 | -0.003  (0.002) | -0.000  (0.001) |
|  |  |  | Ages 0 - 14 | -0.004  (0.003) | -0.000  (0.001) |
| Observations | 360,592 | 524,262 | Observations | 199,492 | 450,672 |
| Mean | 0.04 | 0.02 | Mean | 0.03 | 0.02 |
| *Notes:* Data from NAEP. Unit of analysis is at the individual level. Each cell represents one regression result. Estimates report the coefficient on Medicaid income eligibility cutoff level and are interpreted as a $10,000 change. Regressions use identical specification used in Table 3. Standard errors are clustered at the state level and reported in parentheses. P-values from F-tests presented in parentheses. 4th Grade math analysis uses 1992, 1996, 2000, 2003, 2005, 2007 and 2009 assessments. 4th Grade reading analysis uses 1992, 1994, 1998, 2002, 2003, 2005, 2007, and 2009 assessments. 8th Grade math analysis uses 1996, 2000, 2003, 2005, 2007 and 2009 assessments. 8th Grade reading analysis uses 1998, 2002, 2003, 2005, 2007, and 2009 assessments. \* represents significance at the 5% level. | | | | | |

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| --- | --- | --- | --- | --- |
| **Table 12. Descriptive Statistics for North Carolina Education Research Data Center Samples** | | | | |
|  | | | | |
|  | 1995 | | 1997 | |
|  | <50% FPL | > 50% FPL | < 50% FPL | >50% FPL |
| *Outcomes* |  |  |  |  |
| Math Test Score (std.) | 0.07 | -0.14 | 0.06 | -0.13 |
| Math Proficient | 0.70 | 0.62 | 0.73 | 0.66 |
| Reading Test Score (std.) | 0.07 | -0.12 | 0.06 | -0.13 |
| Reading Proficient | 0.72 | 0.65 | 0.72 | 0.65 |
| Grade | 7.2  (0.70) | 7.1  (0.74) | 7.2  (0.76) | 7.1  (0.86) |
| Below Legislated Grade | 0.24 | 0.29 | 0.23 | 0.26 |
|  |  |  |  |  |
| *Absences in School Year* |  |  |  |  |
| Absent Less Than 8 days | 0.65 | 0.63 | 0.63 | 0.60 |
| Absent 8 or more days | 0.35 | 0.37 | 0.37 | 0.40 |
| Absent 15 or more days | 0.12 | 0.13 | 0.13 | 0.16 |
| Absent 21 or more days | 0.04 | 0.05 | 0.05 | 0.07 |
|  |  |  |  |  |
| *Demographics* |  |  |  |  |
| Age in December of School Year | 14.1  (0.59) | 14.1  (0.60) | 14.1  (0.60) | 14.1  (0.60) |
| Non-Hispanic White | 0.68 | 0.58 | 0.68 | 0.58 |
| Non-Hispanic Black | 0.26 | 0.37 | 0.27 | 0.36 |
| Hispanic | 0.01 | 0.01 | 0.02 | 0.02 |
| Other Race/Ethnicity | 0.04 | 0.04 | 0.04 | 0.04 |
| Male | 0.50 | 0.51 | 0.50 | 0.51 |
| Parents: < H.S. degree | 0.10 | 0.13 | 0.10 | 0.12 |
| Parents: = H.S. degree | 0.37 | 0.41 | 0.41 | 0.45 |
| Parents: > H.S. degree | 0.52 | 0.46 | 0.49 | 0.42 |
| Limited English Proficiency | 0.004 | 0.01 | 0.01 | 0.01 |
|  |  |  |  |  |
| Obs. | 128,604 | 25,938 | 142,578 | 27,782 |
| Notes: Reporting sample restricted to all students with non-missing end of grade math test scores. 1995 samples uses students born between October of 1980 and October of 1982. 1997 Sample uses students born between October of 1982 and October of 1984. | | | | |

|  |  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  | **Table . – Math and Reading Test Scores and Proficiency Ratings** | | | | | | | | | |
| Full Sample | |  | Math Test Scores | | Math Proficiency | | Reading Test Scores | | Reading Proficiency | |
|  | |  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1997 (Treatment) | |  | 0.081\*\* | 0.138\*\* | 0.034\*\* | 0.054\*\* | 0.088\*\* | 0.130\*\* | -0.011 | 0.019\*\* |
|  | (0.021) | (0.030) | (0.009) | (0.011) | (0.022) | (0.031) | (0.008) | (0.006) |
| Mean | |  | 0.025 | 0.025 | 0.714 | 0.714 | 0.026 | 0.026 | 0.711 | 0.711 |
| Obs. | |  |  |  |  |  |  |  |  |  |
|  | |  |  |  |  |  |  |  |  |  |
|  | |  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1995 (Placebo) | |  | 0.081\*\* | 0.158\*\* | 0.021 | 0.048\*\* | 0.106\*\* | 0.187\*\* | 0.008 | 0.048\*\* |
|  | (0.025) | (0.043) | (0.011) | (0.018) | (0.026) | (0.045) | (0.011) | (0.015) |
| Mean | |  | 0.035 | 0.035 | 0.689 | 0.689 | 0.040 | 0.040 | 0.711 | 0.711 |
| Obs. | |  |  |  |  |  |  |  |  |  |
|  | |  |  |  |  |  |  |  |  |  |
| Diff (1997-1995) | |  | 0.000 | -0.020 | 0.013 | 0.006 | -0.018 | -0.057 | -0.019 | -0.029 |
|  | |  |  |  |  |  |  |  |  |  |
|  | |  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| Diff-RD | |  | 0.000 | -0.020 | 0.013\* | 0.006 | -0.018 | -0.057\*\* | -0.019\*\* | -0.029\*\* |
|  | (0.012) | (0.017) | (0.007) | (0.011) | (0.013) | (0.020) | (0.006) | (0.010) |
| Mean | |  | 0.030 | 0.030 | 0.702 | 0.702 | 0.033 | 0.033 | 0.711 | 0.711 |
| Obs. | |  | 324,902 | 324,902 | 324,923 | 324,923 | 324,952 | 324,952 | 324,964 | 324,964 |
| Notes: In 1997 sample, reported estimated is the effect of birth at October, 1983. In 1995 sample, reported estimated is the effect of birth at October, 1981. Standard errors are reported in parenthesis and are clustered at the birth month cohort level. Diff-RD models report coefficient of interaction between post October birth and 1997 sample indicators. | | | | | | | | | | |

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| **Table . – Observed Grade Outcomes** | | | | | | | |
| Full Sample |  | Grade | | Lower Grade than Law | | Lower Grade than Law (Donut) | |
|  |  | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1997 (Treatment) |  | -0.431\*\* | -0.395\*\* | -0.371\*\* | -0.330\*\* | -0.339\*\* | -0.342\*\* |
|  | (0.061) | (0.101) | (0.044) | (0.044) | (0.030) | (0.027) |
| Mean |  | 7.197 | 7.197 | 0.244 | 0.244 | 0.234 | 0.234 |
| Obs. |  |  |  | 176,034 | 176,034 | 155,965 | 155,965 |
|  |  |  |  |  |  |  |  |
|  |  | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1995 (Placebo) |  | -0.503\*\* | -0.453\*\* | -0.342\*\* | -0.314\*\* | -0.308\*\* | -0.320\*\* |
|  | (0.067) | (0.108) | (0.038) | (0.041) | (0.025) | (0.025) |
| Mean |  | 7.207 | 7.207 | 0.259 | 0.259 | 0.250 | 0.250 |
| Obs. |  |  |  | 158,910 | 158,910 | 140,946 | 140,946 |
|  |  |  |  |  |  |  |  |
| Diff (1997-1995) |  | 0.072 | 0.058 | -0.029 | -0.016 | -0.031 | -0.022 |
|  |  |  |  |  |  |  |  |
|  |  | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| Diff-RD |  | 0.072\*\* | 0.058\*\* | -0.029\*\* | -0.016 | -0.031\*\* | -0.022\*\* |
|  | (0.014) | (0.010) | (0.009) | (0.008) | (0.009) | (0.010) |
| Mean |  | 7.202 | 7.202 | 0.251 | 0.251 | 0.242 | 0.242 |
| Obs. |  | 334,944 | 334,944 | 334,944 | 334,944 | 296,911 | 296,911 |
| Notes: In 1997 sample, reported estimated is the effect of birth at October, 1983. In 1995 sample, reported estimated is the effect of birth at October, 1981. Standard errors are reported in parenthesis and are clustered at the birth month cohort level. Diff-RD models report coefficient of interaction between post October birth and 1997 sample indicators. | | | | | | | |

|  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Table . – Math and Reading Test Scores and Proficiency Ratings** | | | | | | | | | |
| *Panel A: 50 % of Students in School Eligible for Free/Reduced Lunch* | | | | | | | | | |
|  |  |  | |  | |  | |  | |
| 50% FPL |  | Math Test Scores | | Math Proficiency | | Reading Test Scores | | Reading Proficiency | |
|  |  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1997 (Treatment) |  | 0.107\*\* | 0.145\*\* | 0.034\*\* | 0.046\*\* | 0.081\*\* | 0.129\*\* | -0.015 | 0.017 |
|  | (0.027) | (0.044) | (0.011) | (0.016) | (0.028) | (0.045) | (0.012) | (0.021) |
| Mean |  | -0.129 | -0.129 | 0.656 | 0.656 | -0.132 | -0.132 | 0.649 | 0.649 |
| Obs. |  | 27,782 | 27,782 | 27,782 | 27,782 | 27,777 | 27,777 | 27,777 | 27,777 |
|  |  |  |  |  |  |  |  |  |  |
|  |  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1995 (Placebo) |  | 0.034 | 0.086\*\* | 0.003 | 0.026 | 0.066\*\* | 0.137\*\* | -0.007 | 0.034\*\* |
|  | (0.029) | (0.041) | (0.015) | (0.026) | (0.031) | (0.041) | (0.013) | (0.015) |
| Mean |  | -0.145 | -0.145 | 0.620 | 0.620 | -0.124 | -0.124 | 0.646 | 0.646 |
| Obs. |  | 25,938 | 25,938 | 25,942 | 25,942 | 25,930 | 25,930 | 25,936 | 25,936 |
|  |  |  |  |  |  |  |  |  |  |
| Diff (1997-1995) |  | 0.073 | 0.059 | 0.031 | 0.020 | 0.015 | -0.008 | -0.008 | -0.017 |
|  |  |  |  |  |  |  |  |  |  |
|  |  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| Diff-RD |  | 0.073\*\* | 0.059\*\* | 0.031\*\* | 0.020 | 0.015 | -0.007 | -0.008 | -0.017 |
|  | (0.019) | (0.017) | (0.011) | (0.017) | (0.022) | (0.027) | (0.010) | (0.013) |
| Mean |  | -0.137 | -0.137 | 0.639 | 0.639 | -0.128 | -0.128 | 0.647 | 0.647 |
| Obs. |  | 53,720 | 53,720 | 53,724 | 53,724 | 53,707 | 53,707 | 53,713 | 53,713 |
|  |  |  |  |  |  |  |  |  |  |
| *Panel B: < 50 % of Students in School Eligible for Free/Reduced Lunch* | | | | | | | | | |
|  |  |  | |  | |  | |  | |
| < 50 % FPL |  | Math Test Scores | | Math Proficiency | | Reading Test Scores | | Reading Proficiency | |
|  |  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1997 (Treatment) |  | 0.077\*\* | 0.137\*\* | 0.034\*\* | 0.056\*\* | 0.090\*\* | 0.131\*\* | -0.010 | 0.020\*\* |
|  | (0.022) | (0.028) | (0.009) | (0.010) | (0.022) | (0.030) | (0.008) | (0.005) |
| Mean |  | 0.055 | 0.055 | 0.726 | 0.726 | 0.057 | 0.057 | 0.723 | 0.723 |
| Obs. |  | 142,578 | 142,578 | 142,578 | 142,578 | 142,505 | 142,505 | 142,505 | 142,505 |
|  |  |  |  |  |  |  |  |  |  |
|  |  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1995 (Placebo) |  | 0.091\*\* | 0.172\*\* | 0.025\*\* | 0.052\*\* | 0.114\*\* | 0.197\*\* | 0.011 | 0.051\*\* |
|  | (0.026) | (0.044) | (0.010) | (0.016) | (0.026) | (0.047) | (0.011) | (0.015) |
| Mean |  | 0.072 | 0.072 | 0.702 | 0.702 | 0.073 | 0.073 | 0.724 | 0.724 |
| Obs. |  | 128,604 | 128,604 | 128,621 | 128,621 | 128,740 | 128,740 | 128,746 | 128,746 |
|  |  |  |  |  |  |  |  |  |  |
| Diff (1997-1995) |  | -0.014 | -0.035 | 0.009 | 0.004 | -0.024 | -0.066 | -0.021 | -0.031 |
|  |  |  |  |  |  |  |  |  |  |
|  |  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| Diff-RD |  | -0.014 | -0.035 | 0.009 | 0.004 | -0.024 | -0.066\*\* | -0.021\*\* | -0.031\*\* |
|  | (0.014) | (0.021) | (0.007) | (0.011) | (0.015) | (0.025) | (0.008) | (0.014) |
| Mean |  | 0.063 | 0.063 | 0.715 | 0.715 | 0.064 | 0.064 | 0.723 | 0.723 |
| Obs. |  | -0.014 | -0.035 | 271,199 | 271,199 | 271,245 | 271,245 | 271,251 | 271,251 |
| Notes: In 1997 sample, reported estimated is the effect of birth at October, 1983. In 1995 sample, reported estimated is the effect of birth at October, 1981. Standard errors are reported in parenthesis and are clustered at the birth month cohort level. Diff-RD models report coefficient of interaction between post October birth and 1997 sample indicators. | | | | | | | | | |

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Table Heterogeneity in Math Achievement by Percentage of Students Eligible for Free or Reduced Lunch** | | | | | | | | |
| *Panel A: Heterogeneity in Math Test Scores by School Poverty Percentage* | | | | | | | | |
|  | 10 % FRL | | 25 % FRL | | 40 % FRL | | 50 % FRL | |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1997 (Treatment) | 0.076\*\* | 0.135\*\* | 0.071\*\* | 0.132\*\* | 0.060\*\* | 0.114\*\* | 0.107\*\* | 0.145\*\* |
| (0.022) | (0.030) | (0.022) | (0.030) | (0.020) | (0.031) | (0.027) | (0.044) |
| Mean | 0.016 | 0.016 | 0.004 | 0.004 | -0.032 | -0.032 | -0.129 | -0.129 |
| Obs. | 167,996 | 167,996 | 160,459 | 160,459 | 116,599 | 116,599 | 27,782 | 27,782 |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1995  (Placebo) | 0.081\*\* | 0.157\*\* | 0.084\*\* | 0.158\*\* | 0.076\*\* | 0.147\*\* | 0.034 | 0.086\*\* |
| (0.025) | (0.042) | (0.026) | (0.043) | (0.025) | (0.039) | (0.029) | (0.041) |
| Mean | 0.026 | 0.026 | 0.012 | 0.012 | -0.028 | -0.028 | -0.145 | -0.145 |
| Obs. | 152,420 | 152,420 | 146,241 | 146,241 | 109,605 | 109,605 | 25,938 | 25,938 |
|  |  |  |  |  |  |  |  |  |
| Diff (1997-1995) | -0.005 | -0.022 | -0.013 | -0.026 | -0.016 | -0.033 | 0.73 | 0.059 |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| Diff-RD | -0.005 | -0.022 | -0.013 | -0.026 | -0.015 | -0.033\*\* | 0.073\*\* | 0.059\*\* |
| (0.013) | (0.018) | (0.013) | (0.017) | (0.010) | (0.013) | (0.019) | (0.017) |
| Mean | 0.021 | 0.021 | 0.008 | 0.008 | -0.030 | -0.030 | -0.137 | -0.137 |
| Obs. | 320,416 | 320,416 | 306,700 | 306,700 | 226,204 | 226,204 | 53,720 | 53,720 |
|  |  |  |  |  |  |  |  |  |
| *Panel B: Heterogeneity in Math Proficiency by School Poverty Percentage* | | | | | | | | |
|  | 10 % FRL | | 25 % FRL | | 40 % FRL | | 50 % FRL | |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1997 (Treatment) | 0.033\*\* | 0.055\*\* | 0.033\*\* | 0.056\*\* | 0.028\*\* | 0.047\*\* | 0.034\*\* | 0.046\*\* |
| (0.009) | (0.011) | (0.009) | (0.011) | (0.009) | (0.012) | (0.011) | (0.016) |
| Mean | 0.712 | 0.712 | 0.708 | 0.708 | 0.695 | 0.695 | 0.656 | 0.656 |
| Obs. | 167,996 | 167,996 | 160,459 | 160,459 | 116,599 | 116,599 | 27,782 | 27,782 |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1995  (Placebo) | 0.021 | 0.048\*\* | 0.023\*\* | 0.050\*\* | 0.022\* | 0.045\*\* | 0.003 | 0.026 |
| (0.011) | (0.018) | (0.011) | (0.018) | (0.011) | (0.019) | (0.015) | (0.026) |
| Mean | 0.686 | 0.686 | 0.681 | 0.681 | 0.667 | 0.667 | 0.620 | 0.620 |
| Obs. | 152,441 | 152,441 | 146,267 | 146,267 | 109,626 | 109,626 | 25,942 | 25,942 |
|  |  |  |  |  |  |  |  |  |
| Diff (1997-1995) | 0.012 | 0.007 | 0.010 | 0.006 | 0.006 | 0.002 | 0.031 | 0.020 |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| Diff-RD | 0.012 | 0.007 | 0.009 | 0.006 | 0.006 | 0.003 | 0.031\*\* | 0.020 |
| (0.007) | (0.011) | (0.008) | (0.011) | (0.007) | (0.012) | (0.011) | (0.017) |
| Mean | 0.699 | 0.699 | 0.695 | 0.695 | 0.681 | 0.681 | 0.639 | 0.639 |
| Obs. | 320,437 | 320,437 | 306,726 | 306,726 | 226,225 | 226,225 | 53,724 | 53,724 |
| Notes: In 1997 sample, reported estimated is the effect of birth at October, 1983. In 1995 sample, reported estimated is the effect of birth at October, 1981. Standard errors are reported in parenthesis and are clustered at the birth month cohort level. Diff-RD models report coefficient of interaction between post October birth and 1997 sample indicators. | | | | | | | | |

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Table Heterogeneity in Reading Achievement by Percentage of Students Eligible for Free or Reduced Lunch** | | | | | | | | |
| *Panel A: Heterogeneity in Reading Test Scores by School Poverty Percentage* | | | | | | | | |
|  | 10 % FRL | | 25 % FRL | | 40 % FRL | | 50 % FRL | |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1997 (Treatment) | 0.083\*\* | 0.128\*\* | 0.081\*\* | 0.129\*\* | 0.076\*\* | 0.129\*\* | 0.081\*\* | 0.129\*\* |
| (0.023) | (0.031) | (0.023) | (0.031) | (0.022) | (0.033) | (0.028) | (0.045) |
| Mean | 0.018 | 0.018 | 0.006 | 0.006 | -0.031 | -0.031 | -0.132 | -0.132 |
| Obs. | 167,921 | 167,921 | 160,385 | 160,385 | 116,538 | 116,538 | 27,777 | 27,777 |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1995  (Placebo) | 0.106\*\* | 0.188\*\* | 0.108\*\* | 0.189\*\* | 0.104\*\* | 0.186\*\* | 0.066\*\* | 0.137\*\* |
| (0.026) | (0.045) | (0.027) | (0.046) | (0.027) | (0.045) | (0.031) | (0.041) |
| Mean | 0.033 | 0.033 | 0.022 | 0.022 | -0.015 | -0.015 | -0.124 | -0.124 |
| Obs. | 152,547 | 152,547 | 146,378 | 146,378 | 109,641 | 109,641 | 25,930 | 25,930 |
|  |  |  |  |  |  |  |  |  |
| Diff (1997-1995) | -0.023 | -0.060 | -0.027 | -0.060 | -0.028 | -0.057 | 0.015 | -0.008 |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| Diff-RD | -0.023 | -0.060\*\* | -0.027 | -0.059\*\* | -0.028\* | -0.057\*\* | 0.015 | -0.007 |
| (0.014) | (0.021) | (0.014) | (0.021) | (0.014) | (0.019) | (0.022) | (0.027) |
| Mean | 0.025 | 0.025 | 0.014 | 0.014 | -0.023 | -0.023 | -0.128 | -0.128 |
| Obs. | 320,468 | 320,468 | 306,763 | 306,763 | 226,179 | 226,179 | 53,707 | 53,707 |
|  |  |  |  |  |  |  |  |  |
| *Panel B: Heterogeneity in Reading Proficiency by School Poverty Percentage* | | | | | | | | |
|  | 10 % FRL | | 25 % FRL | | 40 % FRL | | 50 % FRL | |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1997 (Treatment) | -0.012 | 0.019\*\* | -0.013 | 0.020\*\* | -0.018\*\* | 0.017 | -0.015 | 0.017 |
| (0.008) | (0.007) | (0.008) | (0.007) | (0.009) | (0.009) | (0.012) | (0.021) |
| Mean | 0.708 | 0.708 | 0.704 | 0.704 | 0.691 | 0.691 | 0.649 | 0.649 |
| Obs. | 167,921 | 167,921 | 160,385 | 160,385 | 116,538 | 116,538 | 27,777 | 27,777 |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1995  (Placebo) | 0.008 | 0.049\*\* | 0.009 | 0.050\*\* | 0.011 | 0.051\*\* | -0.007 | 0.034\*\* |
| (0.011) | (0.015) | (0.011) | (0.015) | (0.012) | (0.016) | (0.013) | (0.015) |
| Mean | 0.709 | 0.709 | 0.705 | 0.705 | 0.692 | 0.692 | 0.646 | 0.646 |
| Obs. | 152,559 | 152,559 | 146,390 | 146,390 | 109,646 | 109,646 | 25,936 | 25,936 |
|  |  |  |  |  |  |  |  |  |
| Diff (1997-1995) | -0.020 | -0.030 | -0.024 | -0.030 | -0.029 | -0.034 | -0.008 | -0.017 |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| Diff-RD | -0.020\*\* | -0.030\*\* | -0.022\*\* | -0.031\*\* | -0.028\*\* | -0.035\*\* | -0.008 | -0.017 |
| (0.006) | (0.011) | (0.006) | (0.011) | (0.006) | (0.011) | (0.010) | (0.013) |
| Mean | 0.709 | 0.709 | 0.705 | 0.705 | 0.691 | 0.691 | 0.647 | 0.647 |
| Obs. | 320,480 | 320,480 | 306,775 | 306,775 | 226,184 | 226,184 | 53,713 | 53,713 |
| Notes: In 1997 sample, reported estimated is the effect of birth at October, 1983. In 1995 sample, reported estimated is the effect of birth at October, 1981. Standard errors are reported in parenthesis and are clustered at the birth month cohort level. Diff-RD models report coefficient of interaction between post October birth and 1997 sample indicators. | | | | | | | | |

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Table Heterogeneity in Grade Promotion by Percentage of Students Eligible for Free or Reduced Lunch** | | | | | | | | |
| *Panel A: Heterogeneity in Grade by School Poverty Percentage* | | | | | | | | |
|  | 10 % FRL | | 25 % FRL | | 40 % FRL | | 50 % FRL | |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1997 (Treatment) | -0.432\*\* | -0.396\*\* | -0.431\*\* | -0.396\*\* | -0.433\*\* | -0.395\*\* | -0.369\*\* | -0.339\*\* |
| (0.061) | (0.101) | (0.061) | (0.101) | (0.062) | (0.101) | (0.059) | (0.097) |
| Mean | 7.196 | 7.196 | 7.192 | 7.192 | 7.181 | 7.181 | 7.127 | 7.127 |
| Obs. | 173,652 | 173,652 | 165,957 | 165,957 | 120,905 | 120,905 | 28,989 | 28,989 |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1995  (Placebo) | -0.504\*\* | -0.453\*\* | -0.506\*\* | -0.455\*\* | -0.511\*\* | -0.461\*\* | -0.522\*\* | -0.449\*\* |
| (0.068) | (0.108) | (0.068) | (0.109) | (0.068) | (0.110) | (0.070) | (0.111) |
| Mean | 7.206 | 7.206 | 7.204 | 7.204 | 7.195 | 7.195 | 7.134 | 7.134 |
| Obs. | 156,770 | 156,770 | 150,448 | 150,448 | 112,873 | 112,873 | 26,915 | 26,915 |
|  |  |  |  |  |  |  |  |  |
| Diff (1997-1995) |  |  |  |  |  |  |  |  |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| Diff-RD | 0.072\*\* | 0.058\*\* | 0.075\*\* | 0.060\*\* | 0.078\*\* | 0.066\*\* | 0.153\*\* | 0.072\*\* |
| (0.014) | (0.010) | (0.014) | (0.011) | (0.015) | (0.013) | (0.023) | (0.014) |
| Mean | 7.201 | 7.201 | 7.198 | 7.198 | 7.188 | 7.188 | 7.130 | 7.201 |
| Obs. | 330,422 | 330,422 | 316,405 | 316,405 | 233,778 | 233,778 | 55,904 | 330,422 |
|  |  |  |  |  |  |  |  |  |
| *Panel B: Heterogeneity in Grade below Legislated Grade by School Poverty Percentage (Donut)* | | | | | | | | |
|  | 10 % FRL | | 25 % FRL | | 40 % FRL | | 50 % FRL | |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1997 (Treatment) | -0.388\*\* | -0.264\*\* | -0.385\*\* | -0.261\*\* | -0.378\*\* | -0.256\*\* | -0.402\*\* | -0.279\*\* |
| (0.054) | (0.055) | (0.054) | (0.055) | (0.053) | (0.056) | (0.050) | (0.057) |
| Mean | 0.252 | 0.252 | 0.254 | 0.254 | 0.261 | 0.261 | 0.284 | 0.284 |
| Obs. | 166,632 | 166,632 | 159,275 | 159,275 | 115,979 | 115,979 | 27,758 | 27,758 |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| 1995  (Placebo) | -0.352\*\* | -0.246\*\* | -0.349\*\* | -0.245\*\* | -0.342\*\* | -0.239\*\* | -0.329\*\* | -0.265\*\* |
| (0.048) | (0.054) | (0.047) | (0.054) | (0.046) | (0.054) | (0.039) | (0.041) |
| Mean | 0.268 | 0.268 | 0.268 | 0.268 | 0.274 | 0.274 | 0.314 | 0.314 |
| Obs. | 150,258 | 150,258 | 144,197 | 144,197 | 108,218 | 108,218 | 25,874 | 25,874 |
|  |  |  |  |  |  |  |  |  |
| Diff (1997-1995) |  |  |  |  |  |  |  |  |
|  |  |  |  |  |  |  |  |  |
|  | Linear | Quad. | Linear | Quad. | Linear | Quad. | Linear | Quad. |
| Diff-RD | -0.031\*\* | -0.022 | -0.031\*\* | -0.021\* | -0.031\*\* | -0.021\* | -0.055\*\* | -0.036\* |
| (0.009) | (0.011) | (0.009) | (0.012) | (0.008) | (0.012) | (0.010) | (0.020) |
| Mean | 0.243 | 0.243 | 0.244 | 0.244 | 0.251 | 0.251 | 0.283 | 0.283 |
| Obs. | 292,889 | 292,889 | 280,553 | 280,553 | 207,211 | 207,211 | 49,602 | 49,602 |
| Notes: In 1997 sample, reported estimated is the effect of birth at October, 1983. In 1995 sample, reported estimated is the effect of birth at October, 1981. Standard errors are reported in parenthesis and are clustered at the birth month cohort level. Diff-RD models report coefficient of interaction between post October birth and 1997 sample indicators. | | | | | | | | |

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| --- | --- | --- | --- |
| **Take-up Rates Among Poor and Non-Poor in the South Census Region** | | | |
|  | | | |
| **Panel A: Take-up Rates Among South Census Population with Income<100% FPL** | | | |
| Bandwidth | Linear | Quadratic | Cubic |
| +/- 36 months | 0.15\*\*  (0.04) | 0.14\*\*  (0.06) | 0.16\*\*  (0.07) |
| Obs. | 3279 | 3279 | 3279 |
|  |  |  |  |
| +/- 24 months | 0.15\*\*  (0.05) | 0.14\*\*  (0.07) | 0.15  (0.08) |
| Obs. | 2185 | 2185 | 2185 |
|  |  |  |  |
| +/- 12 months | 0.15  (0.07) | 0.16  (0.08) | 0.12  (0.08) |
| Obs | 1135 | 1135 | 1135 |
|  |  |  |  |
| Mean at Cutoff | 0.463 | 0.463 | 0.463 |
|  |  |  |  |
| **Panel B: Take-up Rates Among South Census Population with Income>100% FPL** | | | |
| Bandwidth | Linear | Quadratic | Cubic |
| +/- 36 months | 0.01  (0.01) | 0.01  (0.01) | 0.01  (0.01) |
| Obs. | 9889 | 9889 | 9889 |
|  |  |  |  |
| +/- 24 months | 0.01  (0.01) | 0.005  (0.01) | 0.01  (0.01) |
| Obs. | 6623 | 6623 | 6623 |
|  |  |  |  |
| +/- 12 months | 0.02  (0.01) | -0.004  (0.01) | -0.01  (0.01) |
| Obs | 3354 | 3354 | 3354 |
|  |  |  |  |
| Mean at Cutoff | 0.05 | 0.05 | 0.05 |
| Data from 1992-1996 Integrated Health Interview Surveys prepared by the University of Minnesota Populations Studies Center. Estimates are Medicaid take-up rates at the discontinuity threshold. Regression includes year indicators and include interaction terms between polynomial and post-cutoff cohort variable to allow flexibility in polynomial specification before and after the cutoff date. | | | |

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| --- | --- | --- | --- | --- | --- |
|  | **Table 1. Baseline Descriptive Statistics by Insurance Coverage in 1996** | | | | |
|  | |  |  |  |  |
| *Out of Pocket Expenditures* | |  | Uninsured | Medicaid | Private |
| Total | |  | 120  (346) | 35.4  (167.3) | 215  (561) |
| Office-based Visit Exp. | |  | 28.7  (70.0) | 8.62  (55.9) | 60.3  (186) |
| Dr. Visit Exp. | |  | 26.6  (67.8) | 7.53  (54.3) | 50.5  (150) |
| Outpatient Exp. | |  | 1.42  (16.9) | 0.37  (4.77) | 4.55  (48.1) |
| Inpatient Exp. | |  | 11.7  (181) | 2.66  (65.9) | 5.34  (100) |
| ER Exp. | |  | 13.8  (112) | 3.28  (68.1) | 6.79  (52.3) |
| Dental Exp. | |  | 37.3  (234) | 5.43  (69.4) | 97.3  (450) |
| Vision Exp. | |  | 10.1  (47.9) | 2.00  (18.6) | 11.3  (44.8) |
| RX Exp. | |  | 16.4  (54.3) | 12.1  (67.1) | 27.0  (81.0) |
| *Services Utilization* | |  |  |  |  |
| Office-based Visits | |  | 1.23  (3.72) | 2.33  (4.72) | 3.07  (5.75) |
| Dr. Visits | |  | 0.97  (2.54) | 2.00  (3.21) | 2.55  (3.80) |
| Outpatient Visits | |  | 0.06  (0.43) | 0.14  (0.84) | 0.15  (0.88) |
| Inpatient Visits | |  | 0.02  (0.12) | 0.06  (0.30) | 0.03  (0.38) |
| ER Visits | |  | 0.11  (0.40) | 0.21  (0.62) | 0.15  (0.45) |
| Dental Visits | |  | 0.49  (1.64) | 0.50  (1.16) | 1.46  (2.64) |
| Number of RX prescriptions | |  | 0.98  (2.55) | 2.50  (4.90) | 2.52  (4.60) |
| *Demographics* | |  |  |  |  |
| Age | |  | 9.7  (5.1) | 7.4  (5.0) | 9.1  (4.9) |
| Male | |  | 0.52 | 0.51 | 0.50 |
| White | |  | 0.34 | 0.29 | 0.66 |
| Black | |  | 0.15 | 0.26 | 0.12 |
| Hispanic | |  | 0.47 | 0.41 | 0.18 |
| Other | |  | 0.04 | 0.04 | 0.04 |
| Head of Household: Married | |  | 0.67 | 0.46 | 0.82 |
| Head of Household: H.S. or less | |  | 0.83 | 0.89 | 0.51 |
| Family Income | |  | $33,371  (27,789) | $20,527  (23,141) | $57,609  (36,467) |
| Observations | |  | 861 | 1,932 | 3,362 |

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| **Table 2. Impact of CHIP/Medicaid Expansions on Out of Pocket Expenditures** | | | | | | | |
| **Service Type** |  | Total | Dr. Visits. | Inpatient | ER | RX | Dental Care |
| ***Self-pay*** |  |  |  |  |  |  |  |
| Out of pocket Payments  (OLS) |  | 7.38  (6.23) | 1.47  (1.98) | -2.77  (1.76) | -1.52\*\*  (0.76) | 1.95  (1.26) | 9.99\*\*  (4.97) |
| Out of pocket Payments  (Poisson) |  | 0.031  (0.030) | 0.043  (0.044) | -0.33\*\*  (0.14) | -0.21\*\*  (0.08) | 0.05  (0.03) | 0.07  (0.05) |
| Mean |  | 165 | 42 | 6.32 | 6.92 | 31 | 69 |
|  |  |  |  |  |  |  |  |
| Any Out of Pocket Payments |  | 0.011\*\*  (0.005) | 0.008  (0.006) | -0.0001  (0.001) | -0.001  (0.002) | 0.013\*\*  (0.005) | -0.003  (0.004) |
| Mean |  | 0.61 | 0.41 | 0.009 | 0.05 | 0.39 | 0.19 |
|  |  |  |  |  |  |  |  |
| ***Utilization*** |  |  |  |  |  |  |  |
| Number of visitsa  (OLS) |  |  | 0.028  (0.046) | -0.003  (0.002) | -0.007  (0.005) | 0.129  (0.066) | 0.006  (0.004) |
| Number of visits  (Poisson) |  |  | 0.018  (0.018) | -0.109  (0.056) | -0.032  (0.030) | 0.063\*\*  (0.037) | -0.0001  (0.019) |
| Mean |  |  | 2.66 | 0.035 | 0.16 | 2.19 | 0.95 |
|  |  |  |  |  |  |  |  |
| Any Visitsa |  |  | 0.013\*\*  (0.006) | -0.003\*\*  (0.002) | -0.004  (0.004) | 0.013\*\*  (0.006) | 0.007  (0.007) |
| Mean |  |  | 0.68 | 0.029 | 0.12 | 0.48 | 0.39 |
| Observations |  |  | 96,493 | 96,493 | 96,493 | 96,493 | 96,493 |
| Estimates represent effect of a $10,000 increase in the Medicaid eligibility cutoff. Sample is all children between ages 0-17. Out-of-pocket payments consist of annual payments made. Controls include state and year fixed effects, race/ethnicity, sex, age indicators, education of the head of household, head of household marital status, and family size indicators. Standard errors are clustered at the state.  a. For prescription medicines (RX), visits refers to number of annual prescribed medicines. | | | | | | | |

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| **Table 3. Impact of CHIP/Medicaid Expansions on Out of Pocket Expenditures – Heterogeneity by Education of Head of Household** | | | | | | | |
| **Panel A: High school or Less Educated** | | | | | | | |
| **Service-Type** |  | Total | Dr. Visits. | Inpatient | ER | RX | Dental Care |
| ***Self-pay*** |  |  |  |  |  |  |  |
| Out of pocket Payments  (OLS) |  | 3.01  (4.47) | 2.25  (2.21) | -1.13  (1.48) | -1.32  (1.09) | 0.92  (1.18) | 4.30  (4.34) |
| Out of pocket Payments  (Poisson) |  | 0.027  (0.031) | 0.081  (0.079) | -0.25  (0.23) | -0.21  (0.12) | 0.03  (0.04) | 0.06  (0.08) |
| Mean |  | 112 | 27 | 4.83 | 6.24 | 26 | 41 |
|  |  |  |  |  |  |  |  |
| Any Out of Pocket Payments |  | 0.016\*\*  (0.007) | 0.016  (0.009) | 0.0003  (0.001) | 0.001  (0.003) | 0.022\*\*  (0.007) | -0.006  (0.004) |
| Mean |  |  | 0.31 | 0.008 | 0.04 | 0.35 | 0.13 |
|  |  |  |  |  |  |  |  |
| ***Utilization*** |  |  |  |  |  |  |  |
| Number of visitsa  (OLS) |  |  | 0.054  (0.061) | -0.004  (0.002) | -0.006  (0.006) | 0.132  (0.086) | 0.040  (0.025) |
| Number of visits  (Poisson) |  |  | 0.031  (0.028) | -0.112\*\*  (0.056) | -0.016  (0.037) | 0.073  (0.038) | 0.045  (0.025) |
| Mean |  |  | 2.28 | 0.037 | 0.17 | 2.06 | 0.71 |
|  |  |  |  |  |  |  |  |
| Any Visitsa |  |  | 0.018\*\*  (0.007) | -0.004\*\*  (0.002) | -0.001  (0.005) | 0.022\*\*  (0.008) | 0.013  (0.007) |
| Mean |  |  | 0.64 | 0.031 | 0.13 | 0.46 | 0.32 |
| Observations |  |  |  | 66,094 | 66,094 | 66,094 | 66,094 |
|  |  |  |  |  |  |  |  |
| **Panel B: Greater than High School Educated** | | | | | | | |
|  | | | | | | | |
| **Service-Type** |  | Total | Dr. Visits. | Inpatient | ER | RX | Dental Care |
| ***Self-pay*** |  |  |  |  |  |  |  |
| Out of pocket Payments  (OLS) |  | 18.2  (14.7) | 2.08  (4.33) | -5.77  (6.01) | -1.93  (0.97) | 4.08  (2.11) | 21.0\*\*  (9.08) |
| Out of pocket Payments  (Poisson) |  | 0.038  (0.053) | 0.032  (0.056) | -0.44  (0.25) | -0.26\*\*  (0.12) | 0.08  (0.05) | 0.09  (0.07) |
| Mean |  | 280 | 75 | 9.57 | 8.38 | 42 | 129 |
|  |  |  |  |  |  |  |  |
| Any Out of Pocket Payments |  | 0.004  (0.006) | 0.001  (0.009) | -0.0005  (.002) | -0.006  (0.003) | -0.003  (0.005) | 0.001  (0.007) |
| Mean |  | 0.79 | 0.62 | 0.012 | 0.06 | 0.50 | 0.32 |
|  |  |  |  |  |  |  |  |
| ***Utilization*** |  |  |  |  |  |  |  |
| Number of visitsa  (OLS) |  |  | -0.003  (0.111) | -0.002  (0.003) | -0.010  (0.006) | 0.111  (0.071) | -0.064  (0.049) |
| Number of visits  (Poisson) |  |  | 0.001  (0.030) | -0.100  (0.104) | -0.073  (0.044) | 0.045  (0.028) | -0.037  (0.023) |
| Mean |  |  | 3.51 | 0.030 | 0.14 | 2.49 | 1.48 |
|  |  |  |  |  |  |  |  |
| Any Visitsa |  |  | 0.001  (0.007) | -0.002  (0.002) | -0.010\*\*  (0.004) | -0.003  (0.006) | -0.005  (0.009) |
| Mean |  |  | 0.77 | 0.025 | 0.11 | 0.54 | 0.52 |
| Observations |  |  | 30,399 | 30,399 | 30,399 | 30,399 | 30,399 |
| Estimates represent effect of a $10,000 increase in the Medicaid eligibility cutoff. Sample is all children between ages 0-17. Out-of-pocket payments consist of annual payments made. Controls include state and year fixed effects, race/ethnicity, sex, age indicators, education of the head of household, head of household marital status, and family size indicators. Standard errors are clustered at the state.  a. For prescription medicines (RX), “visits” refers to number of annual prescribed medicines. | | | | | | | |

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| **Table 4. Impact of CHIP/Medicaid Expansions on Out of Pocket Expenditures – Heterogeneity by Race** | | | | | | | |
| **Panel A: Non-white** | | | | | | | |
| **Service-Type** |  | Total | Dr. Visits | Inpatient | ER | RX | Dental Care |
| ***Self-pay*** |  |  |  |  |  |  |  |
| Out of pocket Payments  (OLS) |  | 8.30  (5.00) | 5.93\*\*  (2.36) | -5.33  (3.78) | -0.71  (0.76) | 1.85\*\*  (0.84) | 6.03  (4.27) |
| Out of pocket Payments  (Poisson) |  | 0.074  (0.058) | 0.27\*\*  (0.12) | -0.49\*\*  (0.16) | -0.12  (0.11) | 0.07  (0.05) | 0.14  (0.14) |
| Mean |  | 87 | 21 | 6.03 | 5.20 | 20 | 28 |
|  |  |  |  |  |  |  |  |
| Any Out of Pocket Payments |  | 0.020\*\*  (0.009) | 0.016  (0.010) | -0.0004  (0.001) | 0.002  (0.002) | 0.025\*\*  (0.007) | 0.001  (0.005) |
| Mean |  | 0.48 | 0.27 | 0.007 | 0.03 | 0.31 | 0.10 |
|  |  |  |  |  |  |  |  |
| ***Utilization*** |  |  |  |  |  |  |  |
| Number of visitsa  (OLS) |  |  | 0.104  (0.082) | -0.009\*\*  (0.002) | -0.003  (0.008) | 0.193\*\*  (0.068) | 0.026  (0.029) |
| Number of visits  (Poisson) |  |  | 0.061  (0.046) | -0.238\*\*  (0.060) | -0.0003  (0.046) | 0.117\*\*  (0.038) | 0.020  (0.050) |
| Mean |  |  | 1.98 | 0.034 | 0.15 | 1.78 | 0.59 |
|  |  |  |  |  |  |  |  |
| Any Visitsa |  |  | 0.26\*\*  (0.009) | -0.007\*\*  (0.002) | -0.001  (0.006) | 0.031\*\*  (0.008) | 0.019\*\*  (0.009) |
| Mean |  |  | 0.61 | 0.029 | 0.11 | 0.42 | 0.30 |
| Observations |  |  | 49,225 | 49,225 | 49,225 | 49,225 | 49,225 |
|  |  |  |  |  |  |  |  |
| **Panel B: White** | | | | | | | |
|  | | | | | | | |
| **Service-Type** |  | Total | Dr. Visits | Inpatient | ER | RX | Dental Care |
| ***Self-pay*** |  |  |  |  |  |  |  |
| Out of pocket Payments  (OLS) |  | 19.1\*\*  (8.57) | 0.05  (2.85) | -0.19  (1.46) | -2.27  (1.17) | 4.18\*\*  (1.65) | 21.1\*\*  (6.47) |
| Out of pocket Payments  (Poisson) |  | 0.029  (0.034) | -0.002  (0.043) | -0.01  (0.21) | -0.25\*\*  (0.11) | 0.05  (0.04) | 0.07  (0.06) |
| Mean |  | 257 | 67 | 7.10 | 9.07 | 44 | 115 |
|  |  |  |  |  |  |  |  |
| Any Out of Pocket Payments |  | -0.0003  (0.005) | -0.001  (0.007) | -0.001  (0.002) | -0.004  (0.003) | 0.002  (0.007) | -0.008  (0.007) |
| Mean |  | 0.75 | 0.56 | 0.012 | 0.06 | 0.50 | 0.28 |
|  |  |  |  |  |  |  |  |
| ***Utilization*** |  |  |  |  |  |  |  |
| Number of visitsa  (OLS) |  |  | -0.0002  (0.095) | 0.001  (0.004) | -0.010  (0.006) | 0.110  (0.079) | -0.005  (0.035) |
| Number of visits  (Poisson) |  |  | 0.002  (0.025) | -0.005  (0.010) | -0.047  (0.035) | 0.037  (0.028) | -0.013  (0.019) |
| Mean |  |  | 3.50 | 0.037 | 0.17 | 2.73 | 1.36 |
|  |  |  |  |  |  |  |  |
| Any Visitsa |  |  | -0.0002  (0.007) | -0.001  (0.003) | -0.007  (0.004) | -0.0004  (0.008) | -0.004  (0.008) |
| Mean |  |  | 0.76 | 0.030 | 0.13 | 0.56 | 0.49 |
| Observations |  |  | 42,328 | 42,328 | 42,328 | 42,328 | 42,328 |
| Estimates represent effect of a $10,000 increase in the Medicaid eligibility cutoff. Sample is all children between ages 0-17. Out-of-pocket payments consist of annual payments made. Controls include state and year fixed effects, race/ethnicity, sex, age indicators, education of the head of household, head of household marital status, and family size indicators. Standard errors are clustered at the state.  a. For prescription medicines (RX), “visits” refers to number of annual prescribed medicines. | | | | | | | |

**Appendix – Revisiting Take-up and Crowd-Out in MEPS sample**

To check Medicaid expansions do indeed increase Medicaid coverage and reduce uninsured rates, I begin by running take-up and crowd-out estimates of the expansions. This is not a new contribution to the literature (i.e. LoSasso and Buchmueller 2005; Currie and Gruber 1996), but it does ensure the observed impacts in the MEPS sample resemble estimates from previous studies, especially considering the mechanisms of the effects are certainly thought to occur *through* increases in Medicaid coverage and reductions in uninsured status.

These take-up and crowd-out estimates are presented in Table 2. Coefficients are interpreted as a $10,000 increase in the state Medicaid eligibility cutoffs. As the table shows, the expansions are effective in increasing Medicaid coverage and reducing uninsured rates. Among samples more likely to be impacted by these expansions, the magnitudes of these effects are larger. Expanding the eligibility cutoff by $10,000 increases coverage by 2.8 percentage points among the full sample. To contextualize this, moving eligibility from 133% of the FPL to 200% in 1996 (as was the case for young children) was roughly equivalent to a $10,000 increase, while moving eligibility from 50% to 200% of the FPL in 1996 (as was often the case for older children) is roughly equivalent to a $20,000 increase.

The decrease in the uninsured rates follows the increase in these Medicaid rates. Private insurance coverage remains mostly unchanged as a result, although estimates are all small and negative. This suggests crowd-out (the reduction in private insurance coverage as a result of an expansion in public insurance) plays a limited role at best.

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| **Appendix Table 1. Impact of CHIP/Medicaid Expansions on Insurance Coverage** | | | | | | |
| *Coverage* |  | Full Sample | High school or Less | Non-White | Greater than High school | White |
| Medicaid |  | 0.028\*\*  (0.011) | 0.036\*\*  (0.012) | 0.043\*\*  (0.011) | 0.006  (0.009) | 0.013  (0.010) |
| Uninsured |  | -0.021\*\*  (0.007) | -0.030\*\*  (0.008) | -0.036\*\*  (0.007) | -0.003  (0.006) | -0.004  (0.006) |
| Private Insurance |  | -0.002  (0.007) | -0.002  (0.009) | -0.002  (0.010) | 0.000  (0.007) | -0.005  (0.010) |
| Observations |  | 96,493 | 66,094 | 49,225 | 30,399 | 42,328 |

1. For example, the World Health Organization reports poorer individuals are more susceptible to chronic illnesses due to “material deprivation and psychosocial stress, higher levels of risk behavior, unhealthy living conditions and limited access to good-quality health care.” (http://www.who.int/chp/chronic\_disease\_report/part2\_ch2/en/index1.html) [↑](#footnote-ref-1)
2. This works in both directions: Gross and Notowodigdo (2010) find that more than a quarter of personal bankruptcies in low-income households occur due to out-of-pocket medical costs. [↑](#footnote-ref-2)
3. By 1991, all states were required to have an AFDC Unemployed Parents program in place (Gruber 1997). [↑](#footnote-ref-3)
4. For example, Alabama increased FPL cutoffs from 15% of FPL to 200% of the FPL and Texas increased FPL from 17% to 100% of the FPL  [↑](#footnote-ref-4)
5. For example, the 2003 4th grade math assessment tests student skills in Number Properties and Operations, Measurement, Geometry, Data Analysis, Statistics, and Probability, and Algebra. These skills are assessed across 10 blocks of questions. Students are randomly assigned two of these blocks. [↑](#footnote-ref-5)
6. The term “High-income schools” refers to students attending schools in which 0-25% of students eligible for free or reduced lunch. “Low-income schools” refers to schools with greater than 75% of the student body eligible for free or reduced lunch. [↑](#footnote-ref-6)
7. Income thresholds appear to increase as children age. While it is true that at any given point in time, income cutoffs are highest for infants, this is not true as the sample ages. The higher observed income cutoffs at later ages are a result of increased generosity over time (not with increased generosity with age). [↑](#footnote-ref-7)
8. The table identifies these as years 8-10, but this might vary since children are either 9 or 10 in 4th grade. Thus for 9-year-olds, the range is really 8-9. All age ranges for 9 and 10 year olds are identical for earlier age ranges. Early NAEP assessments do not include the date of assessment – discussion with NCES indicated to use the December of the previous year as the timing of assessment (i.e. the test date of a 1996 assessment is December of 1995). However, in later assessments when date of test is available, I find some schools may participate in the assessment as late as March of the assessment year. To mitigate this issue, all contemporaneous effects include eligibility in the year of the assessment as well as the year prior to the assessment. [↑](#footnote-ref-8)
9. https://www.medicaid.gov/medicaid-chip-program-information/by-population/by-population.html [↑](#footnote-ref-9)
10. For example, children born in September are likely to be the youngest in their grade while those born in October are likely to be the oldest in their grade. Age-for-grade can have large effects on within-grade achievement, although these effects are generally found to fade with time. See Elder and Lubotsky (2009) and Kaestner and Lubotsky (2015). [↑](#footnote-ref-10)
11. *New York Times*, February 12, 2005. [↑](#footnote-ref-11)
12. Footnote here on copayments in CHIP program. [↑](#footnote-ref-12)