CHILDHOOD MEDICAID COVERAGE AND LATER-LIFE HEALTH CARE UTILIZATION

Laura R. Wherry, Sarah Miller, Robert Kaestner, and Bruce D. Meyer

Abstract—Exploiting a discontinuity in childhood Medicaid eligibility based on date of birth, we find that more years of childhood eligibility are associated with fewer hospitalizations in adulthood. For blacks, we find a 7% to 15% decrease in hospitalizations and a suggestive 2% to 5% decrease in emergency department visits, but no similar effect for non-blacks. The effects are pronounced for utilization related to chronic illnesses and for patients living in low-income postal codes. Calculations suggest that lower rates of hospitalizations during one year in adulthood for blacks offset between 2% and 4% of the initial costs of expanding Medicaid for all children.

I. Introduction

ONE goal of publicly subsidized health insurance is to improve the health of those without insurance, which may lead beneficiaries to need fewer expensive hospital and emergency department (ED) visits later in life. Such long-term reductions in utilization could potentially offset the initial cost of insurance provision. However, little empirical evidence exists on the relationship between publicly provided insurance and long-term health and utilization of care.

One limitation of existing research in this area is that it tends to evaluate utilization changes over a relatively short time horizon. Most studies link health insurance to health contemporaneously or for a few subsequent years. However, the health benefits of insurance may materialize only after a sustained period of insurance and regular use of medical care. In addition, certain types of medical care focus on protecting patients from future health risks, and the payoffs from these types of preventive services may not be evident until later in life. In both scenarios, shorter windows of analysis may not be adequate to identify the health benefits of insurance.

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A second limitation of most studies evaluating the contemporaneous effects of public health insurance on hospitalizations and ED visits is that they are unable to isolate the potential health benefit of insurance from the increased access that results from insurance. Health insurance lowers out-of-pocket costs and induces greater use of health care, including hospitalizations. Therefore, even if an individual’s health improves as a result of public insurance coverage, the access effect may dominate in the short term, leading to higher utilization of medical services.

In this paper, we address these issues by examining whether the expansion of Medicaid in the late 1980s and early 1990s improved the later-life health of those affected. Specifically, we exploit plausibly exogenous variation by birth date in the cumulative number of years an individual was eligible for public health insurance coverage during childhood. To phase in the Medicaid expansions, Congress specified that several eligibility expansions for low-income children applied only to children born after September 30, 1983. As a result, children born before September 30, 1983, experienced lower rates of Medicaid eligibility and fewer Medicaid-eligible years in childhood than children born immediately following the cutoff. For example, a child in a family with income just under the federal poverty level (FPL) gained approximately five additional years of Medicaid eligibility during childhood if she were born on October 1, 1983, rather than September 30, 1983. Black children were particularly likely to benefit from the Medicaid expansions, gaining on average more than twice the number of Medicaid-eligible years of white children. We exploit this policy discontinuity as a source of exogenous variation in Medicaid eligibility in order to evaluate the long-term effects on hospital and ED visits.

We focus on changes in health care utilization that occur following the period of coverage gain, when there are no longer policy-driven differences in Medicaid eligibility or out-of-pocket costs for cohorts on either side of the cutoff. This allows us to isolate the potential health benefits of insurance from any contemporaneous access effects resulting from increased health insurance coverage and reduced out-of-pocket costs for care. The gain in Medicaid eligibility for affected cohorts occurred between ages 8 and 14. We examine the effects of coverage one year after the additional Medicaid coverage ended (at age 15) and ten years later (at age 25). We selected these two times to provide information on both short- and longer-term effects. Given that it is not clear ex ante whether one would expect to observe larger effects on health immediately after the coverage gain or at later ages, this approach allows us to provide a first cut at answering this question.

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We find no effect of the Medicaid expansions on health care utilization at age 15, one year after the period of additional Medicaid eligibility ended. However, we find sizable effects of Medicaid eligibility in childhood on hospitalizations at age 25 and weaker evidence of reductions in ED visits. Black children born immediately after the September 1983 cutoff are estimated to have experienced approximately 7% to 15% fewer hospitalizations and 2% to 5% fewer ED visits at age 25 relative to those born just before the cutoff. Our results are particularly pronounced for hospitalizations and ED visits related to chronic illnesses and among patients from low-income postal codes. Examining diseases by category, we find that greater Medicaid eligibility was associated with decreases in hospitalizations for a broad range of conditions. We do not find reductions in the utilization of nonblacks who experienced smaller gains in Medicaid eligibility and take-up of coverage at the birth date cutoff and who have lower rates of illness than black cohorts.

Our results provide several insights that are relevant to current policy debates surrounding the provision of public health insurance and the role of government in expanding coverage. First, our estimates indicate that between 2% and 4% of the initial cost of the Medicaid expansions for all children were offset by lower hospitalizations among black patients at age 25 alone. If these effects persist at older ages, the size of the cost offset is likely to be even greater. Second, we find no impact of Medicaid coverage in the short term at age 15 but do find effects at age 25. These findings suggest that the benefits of insurance may only materialize over a longer time horizon.

II. Background

Analyses of Medicaid eligibility expansions for children consistently show that Medicaid increases health care utilization, including hospitalizations, in the short term. However, there are fewer studies of the effects of gaining Medicaid on children’s health, and the evidence from this literature is mixed. Thus, the effect of gaining health insurance on health remains an important but unanswered question. In addition, one limitation of studies seeking to assess the effect of insurance on health is that they examine how coverage affects health, for example, as measured by hospital admissions, immediately after or within a few years of the coverage expansion. If the health benefits of insurance are realized later, then a contemporaneous or short-run analysis may miss much of the effect of insurance.

An emerging literature on the longer-term effects of health insurance coverage in childhood on later-life outcomes has begun to address this issue. Wherry and Meyer (2016) examine the later-life mortality of cohorts born before and after the September 30, 1983, cutoff date specified in many expansions of Medicaid. They provide evidence linking an increase in childhood eligibility to a later decline in teenage mortality for black children who were more likely to gain eligibility under the expansions than white children. Brown, Kowalski, and Lurie (2015) use state-level variation in the timing of public health insurance expansions for children in the 1980s and 1990s to examine long-term effects and find that cohorts who gained coverage had lower mortality rates as adults. Boudreaux et al. (2016) use state variation in the timing of the introduction of the Medicaid program in the 1960s to identify long-term effects among those who gained access to Medicaid early in childhood and find improvements in adult health as measured by the presence of a chronic health condition. Miller and Wherry (2018) find that cohorts whose mothers had higher eligibility rates for prenatal coverage in the 1980s and 1990s while the cohort was in utero had better health outcomes and fewer hospitalizations in adulthood related to chronic health conditions.

In this paper, we add to this small literature by exploiting the discontinuity in Medicaid eligibility and coverage among those born around September 30, 1983, to study the effect of additional childhood Medicaid eligibility at ages 8 to 14 on hospitalizations and ED visits at ages 15 and 25. With the exception of Wherry and Meyer (2016), all previous papers on the long-term effects of Medicaid coverage use state- and year-level variation in Medicaid policy to examine long-term outcomes. Although this empirical approach (i.e., difference-in-differences) has been used many times in the literature, some authors have pointed out its limitations (e.g., the estimates tend to be sensitive to the inclusion of state-specific trends; see Dave et al., 2008). The regression discontinuity design we employ is arguably more credible than the difference-in-difference approach because it does not rely on using policy changes at the state level as the identifying variation (Lee & Lemieux 2010).


3 There are some studies of expanded or more generous coverage that examine health over a two- to five-year period, including Newhouse and the Insurance Experiment Group (1993), Finkelstein et al. (2016), and Sommers, Baicker, and Epstein (2012).

4 Other studies find effects of childhood Medicaid eligibility on non-health outcomes. Brown et al. (2015) find evidence of higher wages and college attendance and lower earned income tax credit payouts in adulthood. Cohodes et al. (2016) find that cohorts that gained childhood Medicaid coverage have higher educational attainment. Levine and Schanzenbach (2009) find improved reading test scores, and Miller and Wherry (2018) find higher high school graduation rates among cohorts who gained coverage in utero.
III. The Policy Discontinuity

Discontinuity in Eligibility Prior to the 1980s, eligibility for Medicaid for nondisabled children was primarily limited to children in families receiving cash welfare under the Aid to Families with Dependent Children (AFDC) program. Beginning in the mid-1980s, Congress took steps to expand eligibility for Medicaid to children not participating in AFDC. In an effort to phase in changes in Medicaid eligibility, Congress specified that many of the legislative changes applied only to children born after September 30, 1983. This provision meant that children born just before and after this birth-date cutoff faced very different eligibility criteria for Medicaid during their childhood years.

Given the nature of the expansions, the discontinuity in eligibility was largest for children in families incomes below the poverty line and above AFDC income levels. For children in families with incomes between 75% and 100% of the poverty line, those born in October 1983 gained, on average, 4.6 years of eligibility during childhood compared to children born one month earlier. Children with incomes between 50% and 75% of poverty and those with incomes between 25% and 50% of poverty also experienced sizable gains with an additional 3.4 and 2.0 years of eligibility, respectively (see appendix figure 1).

The gain in eligibility was primarily concentrated at ages 8 to 14 for children born immediately after the birth-date cutoff. Eligibility levels were similar for children born in September and October 1983 prior to age 8 and again starting at age 15 (see appendix figure 2). We examine health care utilization for cohorts born just before and after September 30, 1983, at the end of the differential gain in Medicaid eligibility—first at age 15 and then ten years later at age 25.

We also examine differential effects of the expansions by race. Black children were more likely to gain eligibility under the expansions (table 1) due to their family incomes. On average, black children born in October versus September 1983 were 17 percentage points more likely to gain Medicaid eligibility. Among those who were made Medicaid eligible, the average gain in eligibility throughout childhood was 4.9 years. The average years of eligibility gained by black children (0.9 eligible years) is over twice the gain for nonblack children (0.4 eligible years), who experienced a 9 percentage point gain in eligibility across the birthdate threshold that led, on average, to 4.4 additional Medicaid-eligible years throughout childhood.

<table>
<thead>
<tr>
<th>Child Population</th>
<th>Percent Gaining Eligibility</th>
<th>Average Gain (in Years) for Children Gaining Eligibility</th>
<th>Average Gain (in Years) for Total Child Population</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Races</td>
<td>10.00</td>
<td>4.54</td>
<td>0.48</td>
</tr>
<tr>
<td>Blacks</td>
<td>17.25</td>
<td>4.91</td>
<td>0.87</td>
</tr>
<tr>
<td>Nonblacks</td>
<td>8.71</td>
<td>4.41</td>
<td>0.41</td>
</tr>
</tbody>
</table>

These cohorts were approximately 8 years old at the implementation of the Omnibus Budget Reconciliation Act of 1990, which required all state Medicaid programs to cover children under age 19 born after September 30, 1983. Later, the State Children’s Health Insurance Program (CHIP) authorized state expansions of public health insurance to children in higher-income families; the CHIP expansions served to close the gap in public eligibility for cohorts born on either side of the cutoff at around age 15.

IV. Data

We begin by examining changes in childhood health insurance coverage and utilization during the period of expanded Medicaid eligibility (ages 8–14) using the 1992–1996 National Health Insurance Survey (NHIS) Health Insurance Supplements. The NHIS provides information on respondent quarter of birth, which allows us to estimate discontinuities at the September 30, 1983, birth date. Our analysis is of a similar spirit to Card and Shore-Sheppard (2004), who first estimated a significant increase in coverage and doctor visits for children born after this date.

To examine changes in later-life health care utilization (age 15 and 25), we use discharge-level hospital data from three sources. First, we use hospitalization data from the Healthcare Cost and Utilization Project (HCUP) State Patient Databases. We acquired all state databases available from HCUP for the 1999 and 2009 years that contained information on the patient’s date of birth. These data provide information on all inpatient hospitalizations that occurred in acute care hospitals in 1999 in Arizona, Hawaii, Iowa, New York, Oregon, and Wisconsin, and in 2009, in Arizona, Arkansas, Colorado, Hawaii, Iowa, Kentucky, Maryland, Michigan, Nebraska, New York, North Carolina, Oregon, South Dakota, Utah, Vermont, and Wisconsin. We supplement these data with the census of hospital discharges that occurred in Texas and California in 1999 and 2009, obtained, respectively, from the Texas Department of State Health Services and the California Health and Human Services Agency, resulting in the complete census of hospital discharges for eight states in 1999 and nineteen states in 2009. To our knowledge, this represents all available state hospital discharge data with patient’s date-of-birth information for these years.

As recommended by the National Center for Health Statistics, we exclude oversampled Hispanic respondents in the 1992 NHIS; results are very similar when they are included.

The 2009 inpatient data from Nebraska and North Carolina do not have information on race and are included from analyses stratified by race. The inpatient data from Oregon in 1999 do not include information on race and are excluded from analyses stratified by race for this year.

Psychiatric hospitals are included in the discharge data from California, Kentucky, Michigan, Oregon, Texas, Utah, and Wisconsin. Other states include visits to psychiatric or other specialty units within general acute care hospitals but not visits to specialty hospitals themselves.
In addition to hospital discharge data, we use data from all State Emergency Discharge Databases available from HCUP in 2009 that include patient’s date-of-birth information. These databases provide the universe of outpatient ED visits that occurred in Arizona, Hawaii, Iowa, Kentucky, New Jersey, New York, Utah, and Wisconsin (obtained from HCUP) and California (obtained from the California Health and Human Services Agency) in 2009. These data cover all visits for which a patient was treated in an ED and released the same day rather than being admitted to the hospital. State ED data are available for 2009 but not for 1999.

Both the hospital discharge and ED data contain information on the diagnoses associated with each visit, total charges, and patient demographics, including race and birth month and year. In 2009 (but not in 1999), we observe whether the patient is from a low-income postal code defined as one with median income below $39,999. We classify primary diagnoses as relating to “chronic” or “nonchronic” conditions using the Chronic Condition Indicator software distributed by HCUP. We exclude hospitalizations and ED visits for diagnoses related to pregnancy and delivery.

Combined, our hospitalization data include 689,546 hospital discharge-level observations and 3.9 million ED visits in 2009 for patients born between 1979 and 1987. Our hospitalization sample covers approximately 36% of the national population in 1999 and 50% of the national population in 2009; our ED visit sample covers 29% of the national population.

The bottom rows of tables 3 to 5 present hospital and ED utilization rates for our sample. Table 3 displays hospitalization rates (per 10,000 individuals) for 15-year-olds in 1999, the first year for which we have data. Hospitalization rates at this age were higher for blacks. About 53% of all hospitalizations were for chronic illnesses overall; for blacks, chronic illnesses represented about 60% of total hospitalizations. For 15-year-olds, the most common of these chronic illnesses are mental disorders, followed by asthma and diabetes.

Table 4 displays hospitalization rates for 25-year-olds in 2009. Hospitalization rates are higher for this age group. We observe a striking difference in hospital utilization rates across race groups, particularly for chronic illnesses: blacks at age 25 have a chronic illness hospitalization rate more than twice that of nonblacks. About 57% of hospitalizations of black patients and about 45% of those for nonblack patients were for chronic conditions. The most common chronic condition for this age group is mental disorders. The second most common is diabetes, and the third most common is asthma.

ED visits are more common than hospitalizations and tend to treat less severe conditions. Rates of ED use (per 10,000 individuals) are described in table 5. The ED visit rate is again higher for blacks. ED visits tend to be for acute conditions, with a lower share for chronic illnesses, relative to hospitalizations, for both race groups. We again observe dramatic differences across races related to chronic illnesses, as blacks experience more than twice the rate of ED utilization for chronic illnesses as nonblacks.

V. Empirical Strategy

To estimate the impact of childhood Medicaid eligibility, we use a regression discontinuity approach and compare outcomes for cohorts born just before and after the September 30, 1983, birth-date cutoff. We rely on both a parametric specification (i.e., polynomial) and local linear regression to estimate the discontinuity in outcomes at the birth-date cutoff point. These complementary methods offer trade-offs in terms of bias and variance and are presented together to assess the stability of results (Lee & Lemieux 2010). For our measures of later-life utilization, we use the log number of hospitalizations or ED visits as the dependent variable rather than using a rate that relies on a hard-to-measure population denominator. Estimates of the RD are interpreted as the proportionate change in the rate of hospitalizations or ED visits.

We first estimate a second-order polynomial regression model that uses observations from monthly cohorts born within a specified window of the cutoff date. The cohorts are indexed by \( c \), where \( c = 0 \) for the first cohort born after the cutoff (October 1983). We present results for our main specification that relies on a four-year window of birth month cohorts on either side of the cutoff (October 1979–September 1987, \( c \in [-48, 47] \)). Additionally, we show alternative analyses that use three-year (\( c \in [-36, 35] \)) and two-year (\( c \in [-24, 23] \)) windows of birth month cohorts.

The regression specification used to examine later-life utilization is given by

\[
\log(Y) = \beta_0 + \beta_1 x + \beta_2 x^2 + \epsilon
\]

where \( Y \) is the utilization rate, \( x \) is the distance from the cutoff date, and \( \epsilon \) is the error term.

Data obtained from HCUP contain a variable indicating that the median income of the patient’s postal code is below $39,999. For data from Texas and California, we use the American Community Survey and patient postal codes to create this variable following the same criteria.

The HCUP Chronic Conditions Indicator categorizes all diagnosis codes as chronic or not chronic. The definition of a chronic condition requires that it lasts twelve months or longer and that it either (a) places limitations on self-care, independent living, and social interactions or (b) needs ongoing intervention with medical products, services, and special equipment.

Calculated using state population estimates from the U.S. Census Bureau.

Rates were calculated using age-specific population estimates by race for these states from the 2009 American Community Survey and the 2000 Census 1% sample downloaded from IPUMS.

All birth month cells have nonzero counts of hospitalizations and ED visits. Our main results for specifications with the outcome variables measured in levels are reported in appendix table 25; the results are very similar. This approach relies on the assumption that population trends smoothly across the cutoff. In additional analyses (appendix tables 27 and 28), we control for birth cohort size, and this does not meaningfully change the results. Wherry and Meyer (2016) find evidence of a decrease in mortality at ages 15 to 18 resulting from the Medicaid expansions for black children born after the cutoff; without adjusting for the corresponding change in the underlying population count at age 25, this biases us against detecting a decrease in later-life hospitalizations or ED visits.

The regression specification for the NHIS analysis is identical except for the use of individual-level data and a binary outcome variable on the left-hand side.
We begin by measuring discontinuities in childhood health insurance coverage to supplement our analysis of the discontinuity in eligibility at the September 30, 1983, birth date. Figure 1 plots reported levels of Medicaid coverage for each birth month cohort within a four-year window on each side of the birth-date cutoff (centered at October 1983). The lines are fitted values from a quadratic regression in birth month cohort interacted with a dummy variable for cohorts born after September 30, 1983. The graph for blacks and, to a lesser extent, for all races, shows evidence of an increase in Medicaid coverage at the discontinuity in eligibility.
cutoff. When we look separately at children in households with incomes below the poverty line, we see additional evidence of a discontinuity in coverage.

Table 2 presents the corresponding regression estimates. For all races, we see some evidence of an increase in Medicaid coverage of between 1 and 2 percentage points; however, for the most part, the estimates are not statistically significant. We see strong evidence of an increase in Medicaid coverage for blacks of between 5 and 8 percentage points depending on the specification. The estimates are significantly different from 0 at the .05 level in four of five specifications and at the .10 level in the remaining case. Given our previous estimate that 17% of black children gained eligibility at the cutoff, these estimates represent a take-up rate on the order of approximately 29% to 47%. For nonblacks, we do not find a significant increase in Medicaid coverage, and the point estimates are much smaller, indicating less than a 1 percentage point change in coverage and implying a take-up rate of at most 6%. Examining children in households by race. The estimates are less precise but indicate an increase in Medicaid coverage of between 7 and 11 percentage points among poor blacks and of 4 and 9 percentage points among poor nonblacks.

Table 2.—Estimates of Effect of Childhood Medicaid Eligibility on Medicaid Coverage at Ages 8–13

<table>
<thead>
<tr>
<th></th>
<th>All Races (1)</th>
<th>Blacks (2)</th>
<th>Nonblacks (3)</th>
<th>Households in Poverty (4)</th>
<th>Households Not in Poverty (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Global polynomial model</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4-year window</td>
<td>0.010</td>
<td>0.054**</td>
<td>–0.003</td>
<td>0.089***</td>
<td>–0.001</td>
</tr>
<tr>
<td></td>
<td>(–0.008, 0.027)</td>
<td>(0.002, 0.105)</td>
<td>(–0.022, 0.016)</td>
<td>(0.044, 0.133)</td>
<td>(–0.013, 0.011)</td>
</tr>
<tr>
<td>3-year window</td>
<td>0.017</td>
<td>0.071**</td>
<td>–0.001</td>
<td>0.091***</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(–0.004, 0.038)</td>
<td>(0.012, 0.129)</td>
<td>(–0.024, 0.022)</td>
<td>(0.038, 0.145)</td>
<td>(–0.013, 0.013)</td>
</tr>
<tr>
<td>2-year window</td>
<td>0.014</td>
<td>0.045*</td>
<td>0.001</td>
<td>0.057*</td>
<td>–0.000</td>
</tr>
<tr>
<td></td>
<td>(–0.010, 0.038)</td>
<td>(–0.004, 0.095)</td>
<td>0.026, 0.029)</td>
<td>(–0.005, 0.118)</td>
<td>(–0.015, 0.014)</td>
</tr>
<tr>
<td><strong>Local linear regression</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>IK bandwidth selector</td>
<td>0.014*</td>
<td>0.051**</td>
<td>0.004</td>
<td>0.086***</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(–0.001, 0.028)</td>
<td>(0.003, 0.099)</td>
<td>(–0.012, 0.019)</td>
<td>(0.032, 0.141)</td>
<td>(–0.010, 0.013)</td>
</tr>
<tr>
<td>CCT bandwidth selector</td>
<td>0.023**</td>
<td>0.077**</td>
<td>0.006</td>
<td>0.073*</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.001, 0.045)</td>
<td>(0.009, 0.145)</td>
<td>(–0.014, 0.027)</td>
<td>(–0.004, 0.150)</td>
<td>(–0.011, 0.017)</td>
</tr>
<tr>
<td>Baseline mean</td>
<td>0.145</td>
<td>0.319</td>
<td>0.111</td>
<td>0.503</td>
<td>0.042</td>
</tr>
<tr>
<td>N</td>
<td>54,410</td>
<td>9,000</td>
<td>45,410</td>
<td>10,609</td>
<td>40,258</td>
</tr>
</tbody>
</table>

15 We also examined changes in Medicaid coverage for children in poor households by race. The estimates are less precise but indicate an increase in Medicaid coverage of between 7 and 11 percentage points among poor blacks and of 4 and 9 percentage points among poor nonblacks.

with some crowding out (i.e., that some children enrolled in Medicaid who would otherwise have enrolled in private insurance). We also examined changes in the probability of a doctor visit in the last twelve months, as well as any short-stay hospital visit (not related to delivery). We find some evidence of an increase in doctor visits among black children. Although it is imprecisely estimated, this positive effect is consistent with Card and Shore-Sheppard (2004) and other research documenting that expanded Medicaid coverage leads to higher rates of utilization.17

Changes in Medicaid eligibility and coverage documented in figure 1, tables 1 and 2, and appendix figures 1 and 2 indicate clear variation in treatment by race. Accordingly, if Medicaid coverage has long-term effects on health and use of medical care such as hospitalization, it is plausible to expect that effects will vary in a way consistent with the variation in treatment. In the analyses that follow, we present results separately for blacks and nonblacks. Results for all races are in appendix table 3 and appendix figures 6 and 7.

B. Discontinuity in Later Life Hospitalizations and ED Visits

Figure 2 presents the profile of log hospitalizations by birth month cohort in 1999, when the cohorts born just on either side of the cutoff are approximately 15 years of age. Visually, the figure reveals little evidence of a discontinuity in outcomes at the September 30, 1983, threshold. Esti-
declines in hospitalizations for chronic illnesses on the
depending on the specification. Our estimates also indicate
cohorts born just after the September 30, 1983, date,
a reduction in hospitalizations of between 7% and 15% for
cohorts born around the cutoff were approximately 25
years old. Table 4 presents the corresponding discontinuity
estimates. For blacks, there is a notable drop in hospitaliza-
tions visible at the cutoff. The regression analysis indicates
a reduction in hospitalizations of between 7% and 15% for
cohorts born just after the September 30, 1983, date,
depending on the specification. Our estimates also indicate
decreases in hospitalizations for chronic illnesses on the
order of 11% to 18% across specifications. For hospitaliza-
tions related to nonchronic illness, the estimated decline is
smaller—3% to 11%—and significant only when using
local linear methods. We do not find any evidence of a similar improvement for nonblacks.

Since data were available for more states in 2009 than in
1999, the sample composition is different for these two ana-
yses. However, we find similar results when we estimate
our model with the 2009 data for only those states that were
available in 1999. This suggests that the inclusion of addi-
tional states is not driving the difference in results between
1999 and 2009. These results are reported in appendix
table 4.

The figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results are presented using two-month bins.

<table>
<thead>
<tr>
<th>Table 3.—Estimates of Effect of Childhood Medicaid Eligibility on Log Hospitalizations at Age 15 (1999)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Global polynomial model</td>
</tr>
<tr>
<td>4-year window (N = 96)</td>
</tr>
<tr>
<td>(−0.142, 0.092)</td>
</tr>
<tr>
<td>3-year window (N = 72)</td>
</tr>
<tr>
<td>(−0.125, 0.166)</td>
</tr>
<tr>
<td>2-year window (N = 48)</td>
</tr>
<tr>
<td>(−0.150, 0.217)</td>
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<tr>
<td>Local linear regression</td>
</tr>
<tr>
<td>IK bandwidth selector</td>
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<tr>
<td>(−0.121, 0.094)</td>
</tr>
<tr>
<td>CCT bandwidth selector</td>
</tr>
<tr>
<td>(−0.163, 0.122)</td>
</tr>
<tr>
<td>Rate of hospitalization (per 10,000)</td>
</tr>
</tbody>
</table>

Sample includes birth-month observations pooled from Arizona, California, Hawaii, Iowa, New York, Texas, and Wisconsin data. All global regression models include birth-month fixed effects and a quadratic function in birth month cohort interacted with an indicator that the birth month cohort is October 1983 or later. The 95% confidence intervals are reported in parentheses: ***p < 0.01, **p < 0.05, and *p < 0.1.
The bottom half of figure 3 and table 5 present results for ED visits. We find a reduction in rates of ED visits of between 2% and 5% among black cohorts born immediately after the birthdate cutoff, although the estimates are not significant across all windows of observation. When we examine ED visits by their relation to chronic illness, we find evidence of a sizable decline in visits related to chronic illness (between 10% and 15%). For nonblacks, we find no significant evidence of a similar reduction in ED visits.

Assuming similar effect sizes and hospitalization rates across other states, our point estimates imply that, nationally, there were approximately 2,200 to 4,800 fewer inpatient hospitalizations among black cohorts born during the first year after the cutoff at age 25.\(^{18}\) The change in the

\(^{18}\) Using the census estimate that in 2009 there were 617,000 blacks age 25 and the average hospitalization rate at age 25 for blacks was 517.1 per 10,000 (table 4).
probability of gaining eligibility across the birth-date cutoff was about 17 percentage points for blacks. Our point estimates from our specifications therefore imply that there were between 2.1 and 4.6 fewer hospitalizations at age 25 for every 100 black children who were made eligible for (on average) 4.8 additional years of Medicaid eligibility as a result of the expansions. This reduction for eligible children is large relative to the average rate of hospitalization among all 25-year-old blacks, representing 41% to 88% fewer hospitalizations relative to that average. However, because the children that were affected by these expansions were in poor households and because the poor tend to be in worse health than the general population, it is likely that their baseline hospitalization rates would be higher than that of a typical black 25-year-old (see, e.g., Case, Lubotsky, & Paxson 2002). For the particular cohorts that we study, the presence of a chronic condition during childhood was significantly higher for individuals enrolled in Medicaid compared to those who were not.19

We can further scale these estimates by take-up rates to arrive at the effect of Medicaid coverage rather than Medicaid eligibility on hospitalizations later in life. Based on the estimates presented in tables 1 and 2, we estimated take-up rates on the order of 29% to 47%. A take-up rate of 29% implies that for every 100 black children newly enrolled in Medicaid, there were between 7.2 and 15.9 fewer hospitalizations relative to that average. However, compared to 25.6% of children not enrolled in Medicaid, 33.8% of those enrolled in Medicaid had at least one chronic health condition. Similarly, rates of overnight hospitalization were higher for Medicaid enrollees.

TABLE 5.—ESTIMATES OF EFFECT OF CHILDHOOD MEDICAID ELIGIBILITY ON LOG EMERGENCY DEPARTMENT VISITS AT AGE 25 (2009)

<table>
<thead>
<tr>
<th>Global polynomial model</th>
<th>All</th>
<th>Chronic</th>
<th>Nonchronic</th>
<th>All</th>
<th>Chronic</th>
<th>Nonchronic</th>
</tr>
</thead>
<tbody>
<tr>
<td>4-year window (N = 96)</td>
<td>-0.071*</td>
<td>-0.106**</td>
<td>-0.025</td>
<td>-0.016</td>
<td>-0.038**</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(-0.144, 0.003)</td>
<td>(-0.198, -0.014)</td>
<td>(-0.108, 0.058)</td>
<td>(-0.012, 0.045)</td>
<td>(-0.004, 0.080)</td>
<td>(-0.037, 0.035)</td>
</tr>
<tr>
<td>3-year window (N = 72)</td>
<td>-0.095**</td>
<td>-0.120**</td>
<td>-0.061</td>
<td>-0.026</td>
<td>-0.046**</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(-0.176, -0.013)</td>
<td>(-0.223, -0.017)</td>
<td>(-0.158, 0.036)</td>
<td>(-0.011, 0.054)</td>
<td>(-0.005, 0.097)</td>
<td>(-0.036, 0.038)</td>
</tr>
<tr>
<td>2-year window (N = 48)</td>
<td>-0.144***</td>
<td>-0.167**</td>
<td>-0.113*</td>
<td>-0.054***</td>
<td>-0.081***</td>
<td>0.031</td>
</tr>
<tr>
<td></td>
<td>(-0.247, -0.040)</td>
<td>(-0.301, -0.034)</td>
<td>(-0.232, 0.006)</td>
<td>(-0.020, 0.089)</td>
<td>(-0.031, 0.132)</td>
<td>(-0.013, 0.075)</td>
</tr>
<tr>
<td>Local linear regression</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>IK bandwidth selector</td>
<td>-0.126***</td>
<td>-0.145**</td>
<td>-0.081**</td>
<td>0.016</td>
<td>0.031</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(-0.183, -0.068)</td>
<td>(-0.216, -0.073)</td>
<td>(-0.151, -0.012)</td>
<td>(-0.012, 0.043)</td>
<td>(-0.011, 0.072)</td>
<td>(-0.027, 0.031)</td>
</tr>
<tr>
<td>CCT bandwidth selector</td>
<td>-0.154***</td>
<td>-0.184***</td>
<td>-0.114***</td>
<td>0.013</td>
<td>0.032</td>
<td>0.037</td>
</tr>
<tr>
<td></td>
<td>(-0.228, -0.081)</td>
<td>(-0.278, -0.091)</td>
<td>(-0.179, -0.049)</td>
<td>(-0.026, 0.051)</td>
<td>(-0.013, 0.077)</td>
<td>(-0.017, 0.091)</td>
</tr>
<tr>
<td>Rate of hospitalization</td>
<td>517.14</td>
<td>293.15</td>
<td>223.99</td>
<td>303.71</td>
<td>137.03</td>
<td>166.67</td>
</tr>
</tbody>
</table>

Sample includes birth-month observations from pooled Arizona, Arizona, Colorado, California, Hawaii, Iowa, Kentucky, Maryland, Michigan, New Jersey, New York, Oregon, South Dakota, Texas, Utah, Vermont, and Wisconsin hospital data. All global regression models include birth month fixed effects and a quadratic function in birth month cohort interacted with an indicator that the birth month cohort is October 1983 or later. The 95% confidence intervals are reported in parentheses: ***, ***, ***, * p < 0.1, ***, ***, ***, * p < 0.05, and * p < 0.1.

19 Authors’ calculation based on 1992–1996 NHIS Health Insurance Supplements for cohorts born October 1979 to September 1983. Compared to 25.6% of children not enrolled in Medicaid, 33.8% of those enrolled in Medicaid had at least one chronic health condition. Similarly, rates of overnight hospitalization were higher for the Medicaid enrolled group.
100 black children who enrolled in Medicaid. However, these estimates are likely to overstate the magnitude of the effects among children who took up Medicaid since counts of Medicaid enrollment reported in survey data are often well below administrative record counts. \(^{20}\) This measurement error in reported coverage is likely to lead to an underestimate of changes in coverage associated with the Medicaid expansions. \(^{21}\) In addition, since parents are more likely to enroll children who are most likely to benefit from coverage (e.g., who are sick or have high medical needs; see Marton & Yelowitz 2014), the effect of coverage for this group is likely larger than the effect would be for all children who gained Medicaid eligibility if they took up coverage.

Similarly, our point estimates imply that nationally, there were approximately 7,100 to 17,600 fewer annual ED visits experienced at age 25 by blacks born the first year after the cutoff. This estimate implies that there were between 6.7 and 16.8 fewer ED visits at this age for every 100 black children made eligible as a result of the expansions. This suggests that gaining an average of 4.8 additional years of Medicaid eligibility in childhood lowers ED use by 12% to 29% at age 25. When scaled by our estimate of 29% take-up, this implies that there were between 23.1 and 57.9 fewer ED visits at age 25 for every 100 black children newly enrolled in Medicaid. Using our higher take-up estimate (47%) implies between 14.3 and 35.7 fewer ED visits for every 100 new enrollees. However, as we noted previously, baseline ED use among adults who grew up in low-income families (and took up Medicaid coverage) is likely higher than average ED use in the population.

We did not find evidence of similar reductions in utilization among nonblack children. The difference in the estimated effects between blacks and nonblacks is plausible for two reasons. First, black children were much more likely to benefit from these expansions than other children (as seen in table 1). In addition, we did not observe a similar take-up of Medicaid coverage for nonblacks as compared with blacks (table 2). Second, blacks experience a rate of chronic illness hospitalization and chronic illness ED use that is more than twice that of nonblacks (see table 5). As blacks are at a substantially higher risk of using these types of medical services, they may have the most to gain from expanded access to health care in childhood.

Finally, we also explore the effects of the discontinuity in eligibility on hospitalizations and ED visits for black patients using narrower disease categories based on the International Classification of Diseases, Ninth Revision Standard. Although these estimates tend to be imprecisely estimated due to the small number of hospitalizations that fall within each category, our estimates suggest that Medicaid eligibility was associated with a reduction in hospitalizations for a broad range of disease groups. Further discussion is provided in the appendix, and the results are reported in appendix tables 5 through 8.

### VII. Exploiting Variation in Extent of Treatment

#### A. Low-Income Postal Codes

Motivated by estimates in appendix figure 1 and table 2 that show particularly large changes in Medicaid eligibility and coverage for children in low-income households, we next examine changes in hospitalizations and ED visits in 2009 for patients living in low-income postal codes (tables 6 and 7 and figure 4). If the information were available in our data, we would ideally examine the later health care utilization of individuals who were poor (and eligible for the Medicaid expansions) during childhood. However, if children who grew up in poor families still reside in low-
income postal codes, we might expect to see larger changes for patients from these codes.

Estimates in table 6 indicate a reduction in total hospitalizations of between 10% and 23% among black cohorts in low-income postal codes born just after September 30, 1983, and the coefficient estimate is statistically significant at the .05 level in four of our five specifications. In addition, the decline appears to be concentrated among hospitalizations related to chronic illness, where we see reductions of between 15% and 28% that are statistically significant in all specifications. There is no significant reduction in hospitalizations for nonchronic illnesses.

Similarly, we find evidence of a larger decrease in ED visits for blacks in low-income postal codes than for all blacks, with estimates ranging between 4% and 6%, although the estimates are significant only in certain specifications. There is some evidence of reductions in chronic and nonchronic illness–related ED visits as well, but the estimates are not consistently significant.

Finally, as in our previous analysis, we find no evidence of a significant reduction in hospitalizations or ED visits among nonblack persons from low-income postal codes. However, in most models, we are unable to rule out small reductions in utilization for nonblacks. In addition, the use of current low-income postal code as a proxy for childhood poverty status may be less accurate for nonblacks given a higher rate of mobility over time for these cohorts compared to the black cohorts we study.

In appendix tables 9 and 10, we report results using a more extreme measure of poverty in which we designate postal codes as low poverty if they fall in the 90th percentile of the poverty rate or higher. As expected, reductions in hospitalizations and ED visits for patients from these postal codes are even larger than those reported in tables 6 and 7.

Using the 1997 National Longitudinal Survey of Youth (NLSY97), we examine interstate migration over the time period of our study, focusing on a five-year cohort centered at age 15 at the start and age 27 at the end. Forty-two percent of this cohort changes states over the time period, with rates about 10 percentage points lower for either blacks or the poor. We also find similar differences by race in the intercounty migration of these cohorts.

### B. State Variation in Treatment

In this section, we use estimated differences across states in the average size of the discontinuity in eligibility-years for cohorts born at the cutoff as an additional source of variation in our analysis. The gain in Medicaid eligibility for children born after the cutoff varied by state due primarily to differences in Medicaid policies in place before the expansions and differences in state socioeconomic characteristics. For example, the impact of the requirement that states cover all poor children would depend on both prior eligibility levels determined by the state’s AFDC eligibility threshold (e.g., 14% versus 79% federal poverty level) and the concentration of children in the state with family incomes between AFDC eligibility thresholds and the poverty line.

Appendix table 1 presents estimates of the average eligibility gain at the cutoff for each state in our study. These estimates were calculated using state-specific samples of children from the Current Population Survey (CPS) and therefore capture the average magnitude of the discontinuity at the cutoff given a state’s eligibility rules and distribution of family characteristics. The size of the discontinuity in eligibility varies from 0.05 years of eligibility in California to 1.33 years of eligibility in Arkansas.

Given this variation across states in the estimated discontinuity in childhood eligibility, one might expect the discontinuity in utilization at the cutoff to be larger in states where the change in eligibility across that threshold is greater. A potential concern with this source of variation, however, is that preexpansion state eligibility rules and state socioeconomic characteristics may be correlated with additional types of heterogeneity that could affect the implementation of the Medicaid expansions, access to care, or trends in the

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22 In appendix tables 9 and 10, we report results using a more extreme measure of poverty in which we designate postal codes as low poverty if they fall in the 90th percentile of the poverty rate or higher. As expected, reductions in hospitalizations and ED visits for patients from these postal codes are even larger than those reported in tables 6 and 7.

23 Using the 1997 National Longitudinal Survey of Youth (NLSY97), we examine interstate migration over the time period of our study, focusing on a five-year cohort centered at age 15 at the start and age 27 at the end. Forty-two percent of this cohort changes states over the time period, with rates about 10 percentage points lower for either blacks or the poor. We also find similar differences by race in the intercounty migration of these cohorts.

24 California represents a large population and experienced the smallest policy discontinuity in our sample. For that reason, we also estimate models that exclude California (see appendix tables 16–20), which tend to uncover larger effects.
underlying health of the population. For this reason, this source of variation in expanded Medicaid eligibility is not as clearly exogenous as birth date. There also is likely measurement error in the assigned discontinuity in Medicaid eligibility, particularly in the later period, as individuals may have moved out of their childhood state. For these reasons, we are cautious about placing too much weight on the results from the state analyses.

To explore this heterogeneity, we estimate equation (1) separately for each state using 2009 data. We then plot the estimate for each state against its estimated discontinuity in eligibility years with 95% confidence intervals. Appendix
figures 8 and 9 present the resulting scatter plots for each outcome by race group.\textsuperscript{25} For the most part, the individual state-level estimates are not statistically distinguishable from 0. However, the figures do suggest a negative relationship between the change in utilization at the cutoff and the size of the discontinuity in Medicaid eligibility years at the cutoff.

In the appendix, we formalize this analysis in a regression framework by interacting the eligibility discontinuity size with the cutoff indicator. Although not always statistically significant, the point estimates indicate a negative relationship between utilization and the size of the discontinuity for almost all model specifications and race groups. We also report results using hospitalizations in 1999, when birth cohorts born around the cutoff birth date are 15 years old. As in our main RD analysis, we do not find systematic evidence that those born immediately after the cutoff had fewer hospitalizations at this age.

\section*{VIII. Sensitivity Analyses}

\textbf{A. Hospitalizations for Acute Conditions}

As a sensitivity analysis, we estimate changes in hospitalizations for appendicitis and injuries for all patients and the low-income sample in 2009 by race group. Both appendicitis and injury are acute conditions that are less likely to be sensitive to medical care received in the past. For that reason, we believe it is less plausible that coverage in childhood would substantially influence hospitalizations for these conditions. If we find effects on these types of hospitalizations, it may indicate that the assumptions of our RD model are incorrect.

The results of these analyses are reported in appendix table 21. The first panel shows the results for hospitalizations in 2009 for patients from all postal codes stratified by race. The second panel shows similar results for patients from low-income postal codes. In both panels, we find point estimates that are close to 0, none of which are statistically significant. Overall, this suggests there was little impact of the policy on these types of visits, consistent with our expectation that these visits should not be affected by access to medical care in childhood.

\textbf{B. Placebo Estimates at Nondiscontinuity Points}

We conduct a second type of placebo test using data on cohorts born prior to the actual eligibility cutoff. We place an artificial cutoff date in the center of each two-, three-, and four-year window beginning with cohorts born in January 1965. We then estimate models that mimic our global polynomial specifications using these window sizes and the local specifications that employ our two optimal bandwidth selectors. The last month used in the estimation of these placebo effects is September 1983, immediately before the actual change in Medicaid eligibility occurs. We perform this analysis using the sample of black patients for ED visits and hospitalizations and those related to chronic illnesses.

We then compare the distribution of these placebo estimates to the effect estimated at the “true” cutoff (see the appendix). The fraction of placebo estimates that indicate an effect larger in magnitude than our true estimate provides a \( p \)-value.\textsuperscript{26} We find that the placebo estimates for hospitalizations for blacks are only larger than the true estimate 10.8\% (four-year bandwidth, also not statistically significant using standard inference), 4.5\% (three-year bandwidth), and less than 1\% (two-year bandwidth) of the time under the global polynomial specification. Under the local linear specifications, less than 1\% of the placebo estimates exceed the true estimate for both bandwidth selectors. For chronic illness–related hospitalizations, only 5.4\%, 5.2\%, and 5.6\% of the placebo estimates in the global model using four-, three-, and two-year bandwidths and only 3.1\% and 2.3\% of the placebo estimates in the local models exceed the true estimate.

Conducting a similar analysis on the low-income sample, only 3.8\% of placebo estimates are larger in magnitude than the true estimate in the four-year global model, and none is larger in the three-year and two-year global models or in the local model that uses the IK bandwidth selector. In the local model that uses the CCT bandwidth selector, the true effect exceeds all but 4.6\% of the placebo effects. When we conduct this analysis for chronic illness–related hospitalizations, we find that the true effect exceeds at least 99\% of the placebo estimates in all five specifications.

When examining ED visits, our results conform less to the original inference conducted in tables 5 and 7. Most model specifications exhibit \( p \)-values substantially higher than conventional significance levels when looking at all ED visits. We find somewhat more encouraging results for chronic illness–related ED visits: using the global model, we find that 10.8\% (four-year window), 2.6\% (three-year window), and 7.3\% (two-year window) of the placebo effects are larger in magnitude than the true estimate. In the local models, we find that 5.4\% of placebo effects (IK bandwidth selector) and 2.3\% of placebo effects (CCT bandwidth selector) are greater than the true estimate. However, in the low-income sample, we find much higher \( p \)-values when using the placebo distribution, although these estimates were for the most part not statistically significant when using traditional methods of inference (see table 7).

Overall, the placebo simulations conducted in this section indicate that the estimated effects of the Medicaid policy on chronic illness–related hospitalizations, only 5.4\%, 5.2\%, and 5.6\% of the placebo estimates in the global model using four-, three-, and two-year bandwidths and only 3.1\% and 2.3\% of the placebo estimates in the local models exceed the true estimate.

\textsuperscript{25} The population size of each state for individuals in the range of ages used in this analysis (ages 22 to 30) is indicated by the size of each data point.

\textsuperscript{26} These placebo tests rely on a different set of assumptions, specifically the stationarity of the overall process over a long period of time rather than the independence of individual observations. We are presenting these in order to provide a wide range of evidence but do not claim that the \( p \)-values derived from these tests are more accurate than those produced with standard inference.
hospitalizations among black cohorts are larger than estimates that we might observe due to chance. The simulations provide particularly convincing evidence supporting our results for the low-income subsample of blacks. On the other hand, the placebo tests for ED visits suggest that the evidence for Medicaid effects is not as strong as suggested by the conventional test statistics. Although the effect of Medicaid on chronic illness ED visits estimated at the true discontinuity exceeds the majority of placebo estimates, many of the placebo estimates are larger in absolute value than the true effect in other models.

C. Endogenous Mobility

In our analyses, we assume that population levels and characteristics trend smoothly across birth month cohorts. While interstate migration is substantial, it should not affect the estimates in our main model unless there is differential mobility at the cutoff date. One concern might be, for instance, that if individuals move out of state in response to receiving additional coverage, this mobility may bias the estimates. To further investigate this concern, we use restricted access NHIS data for the years 2004 to 2013 with information on both state of birth and state of residence for the cohorts in our sample. We reestimate equation (1) with a dependent variable indicating whether the respondent resides in a state different from his or her state of birth at age 25. The results are reported in appendix table 22. We find no evidence of a change in mobility for cohorts born after the September 30, 1983, cutoff.

IX. Was the Upfront Cost of the Medicaid Expansions Offset by Lower Utilization Later in Life?

The results presented in this paper provide compelling evidence that expanding Medicaid coverage to children lowers future health care costs by improving health and reducing later-life utilization among those who gain eligibility. In this section, we provide back-of-the-envelope calculations on the magnitude of these cost savings relative to the upfront cost of expanding Medicaid. We focus on savings related to reduced hospitalizations given the strong evidence for this finding, but similar analyses for ED visits may be found in the appendix.

To conduct this analysis, we estimate a model similar to that reported in table 4 but using the log of total hospital costs by birth month cohort as the dependent variable. We calculate total hospital costs by applying HCUP cost-to-charge ratios to the discharge-level data on total charges. These cost-to-charge ratios are designed to estimate the resource cost of a hospital visit using data from accounting reports collected by the Centers for Medicare and Medicaid Services. We then sum total costs at the birth month cohort level and estimate models as described in equation (1). The results of this analysis are reported in appendix table 23 and appendix figure 16.

We find that hospital costs among black cohorts fell by between 9% and 14% for those born immediately after the birth date cutoff. These reductions in costs are highly significant in the local linear models, but only marginally significant or not significant in the global polynomial specifications. Using the point estimates, assuming that the results apply to all states, and using the total hospitalization costs of those born the year before the cutoff as a baseline, our analysis implies that the Medicaid expansions reduced total hospital costs at age 25 by between $20.8 and $32.4 million for black cohorts born the year following the cutoff.

To arrive at an estimate of the original cost of expanding Medicaid, we rely on the average spending per nondisabled child enrolled in Medicaid in 1991, which was $902 per child (in 1991 dollars) (Congressional Research Service, 1993). Multiplying this amount by the average gain in years enrolled in Medicaid using information from tables 1 and 2 and assuming a 3% discount rate, this implies that the total cost of the eligibility expansions for all children born during the year following the September 30, 1983, cutoff was approximately $910 million in 2009. The cost offsets from childhood Medicaid expansions generated by reductions in hospitalizations among black cohorts at age 25 therefore represent between 2% and 4% of the total cost of the expansions to children of all races. If the reduction in utilization we observe at age 25 persists for several years, the cost offsets associated with these expansions will be even larger. In additional analyses reported in the appendix, we find evidence suggesting that a portion of cost savings accrued to the government in the form of reductions in publicly subsidized health care utilization.

These calculations indicate that the long-run cost savings from the Medicaid expansions may be quite substantial.

27 The total cost of 2009 hospitalizations for blacks born between October 1982 and September 1983 in our sample states was $88 million. With approximately 38% of all 25-year-old blacks in our sample, we estimate total costs at the national level to be $231.6 million.

Note that we rely on an estimate of average cost of Medicaid per child for these calculations since we were unable to access the administrative data required to estimate the marginal cost of the additional Medicaid coverage per child under the expansions. Because most (78%) of our variation in the discontinuity is a result of the Omnibus Reconciliation Act of 1990 that was implemented in 1991, we use 1991 as our base year in these calculations.

31 The Census estimate for the total number of 25-year-olds in 2009 is 4,264,000. We multiply this estimate by the 0.48 year average gain in childhood eligibility, a take-up rate of 29% (which is the median of the take-up rates calculated based on the estimates in tables 1 and 2), the $902 cost per year of enrollment per child in 1991, and a 3% discount rate to arrive at our estimate.

32 Reductions in ED visits are excluded from these calculations. If we include cost savings from reduced ED visits, total costs savings in 2009 would be between $27.5 million and $43 million, representing 3% to 5% of the total cost of these expansions to children of all races.
Considering other research on the long-run effects of these expansions on other outcomes, the true cost offsets of the Medicaid expansions might be larger still.

X. Conclusion

Policies that expand public health insurance coverage tend to increase utilization and, thus, the total resources devoted to health care spending in the economy in the short term. However, there may be longer-term consequences that do not materialize until years later if increased access to care improves health and productivity over the life course. While these long-term effects are often cited in policy discussions, very little credible evidence exists on the magnitude of these effects or even if they are present at all.

Long-term health improvements may translate into important cost offsets if they reduce the need for future spending on expensive medical care, which could represent a substantial, but previously unaccounted-for, benefit of public insurance expansions.

In this paper, we provide evidence of such effects by exploiting a discontinuity in the number of years a child is eligible for Medicaid based on his or her date of birth. Because several of the early Medicaid coverage expansions to poor children applied only to children born after September 30, 1983, children born immediately after this cutoff received more years of Medicaid eligibility throughout childhood. Among blacks, who were most likely to be affected by these expansions, we find that those born immediately after the cutoff had a significant reduction in hospitalizations at age 25 compared to those born immediately before the cutoff. The effect is particularly pronounced for chronic illness–related hospitalizations, among patients in low-income neighborhoods, and in states where the size of the eligibility discontinuity was large. A back-of-the-envelope calculation based on our point estimates suggests that these reductions in utilization for the cohorts of black children born one year after the birth date cutoff offset between 2% and 4% of the total cost of the expansions for this cohort. Additionally, we find suggestive evidence that those born immediately after the cutoff experienced fewer ED visits at age 25, although these results are not consistently statistically significant. This evidence suggests that health interventions in the preteen and early teen years for disadvantaged populations can provide long-term health benefits.

An important limitation to this work is that we observe hospitalizations and ED visits at only two points in time for the affected cohorts. We examine these outcomes at age 15, one year following the period of elevated Medicaid coverage, and then ten years later at age 25. While we observe a sizable reduction in health care utilization for black cohorts at age 25, we are unable to pinpoint at what age this effect emerges and whether it is sustained. An additional limitation is that we are unable to identify the specific mechanisms leading to the observed changes. Future opportunities to analyze the health care utilization and health of these cohorts over time will be critical to establishing a more complete picture of the long-term effects of childhood Medicaid coverage.

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