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ABSTRACT

Policy-makers have argued that providing public health insurance coverage to the uninsured lowers long-run costs by reducing the need for expensive hospitalizations and emergency department visits later in life. In this paper, we provide evidence for such a phenomenon by exploiting a legislated discontinuity in the cumulative number of years a child is eligible for Medicaid based on date of birth. We find that having more years of Medicaid eligibility in childhood is associated with fewer hospitalizations and emergency department visits in adulthood for blacks. Our effects are particularly pronounced for hospitalizations and emergency department visits related to chronic illnesses and those of patients living in low-income neighborhoods. Furthermore, we find evidence suggesting that these effects are larger in states where the difference in the number of Medicaid-eligible years across the cutoff birthdate is greater. Calculations suggest that lower rates of hospitalizations and emergency department visits during one year in adulthood offset between 3 and 5 percent of the initial costs of expanding Medicaid.

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I. Introduction

One of the goals of publicly-subsidized health insurance is to improve the health of those without insurance. The argument underlying this policy is straightforward: health insurance provides the means to use more, and more timely, medical care, and because of this greater use of care, health is improved. As a result, those who gain coverage may need fewer expensive hospital and emergency department visits later in life due to their improved health, and these long term changes in utilization may partially or completely offset the initial cost of insurance provision. While the intuition behind this argument is strong, the empirical evidence to support it is relatively weak. For example, the Oregon Medicaid Experiment did not find significant health benefits from health insurance during the first two years of coverage and found that the provision of Medicaid increased, rather than decreased, the use of costly hospital and emergency department care (Finkelstein et al. 2012, Taubman et al. 2014).[1]

One limitation of the literature in this area is its relatively short time horizon. Most studies seek to link health insurance to health contemporaneously, or for a few subsequent years. However, the health benefits of insurance may be cumulative and revealed only after a sustained period of insurance and regular use of medical care. In addition, certain types of medical care focus on protecting the patient 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 insurance.

A second limitation of studies evaluating contemporaneous effects of public health insurance on hospitalizations and emergency department visits is that they are unable to isolate the potential health benefit of insurance from the increased access that results from insurance. For example, they cannot separate an improvement in health due to insurance that could ultimately result in fewer hospitalizations from the concurrent access effects of insurance that lower out-of-pocket costs and induce greater use of care including hospitalizations. 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.

[1] Medicaid coverage was associated with improvements in self-reported health, but no change in physical and clinical health measures (Finkelstein et al. 2012, Baicker et al. 2013).

In this paper, we address these issues by examining whether the expansion of Medicaid in the late 1980s and early 1990s improved the health later in life of those affected. Specifically, we exploit plausibly exogenous variation by birthdate in the cumulative number of years an individual was eligible for public health insurance coverage. 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. This discontinuity in eligibility was first identified and used by Card and Shore-Sheppard (2004) to examine contemporaneous changes in insurance coverage. Wherry and Meyer (forthcoming) later demonstrated that the policy led to cumulative differences in childhood eligibility. They estimated that 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 of Medicaid. The outcomes we examine are hospital and emergency department (ED) visits derived from administrative data from all states that make such data available. These databases capture the universe of hospitalizations or ED visits in each state for a given year and provide sufficiently large sample sizes to detect changes in utilization among the young (e.g., age 25) population that we study. In addition to birth year and month, the data also provide information on other patient characteristics including race and median income of the zipcode of residence. This information allows us to examine changes in hospitalization and ED use among groups that were especially likely to be affected by the change in Medicaid policy.

An important contribution of our study is that we are able to isolate the potential health benefits of insurance from any access effects of health insurance on hospitalizations and ED visits. We are able to disentangle the health effect of insurance on utilization from changes in out-of-pocket costs by analyzing the hospitalizations and use of ED care of young adults later in life, when there are no longer policy-driven differences in Medicaid eligibility or out-of-pocket costs between our treatment and control groups that could drive utilization patterns. We examine the effects of coverage one year after the cohorts have experienced the additional Medicaid coverage (at age 15) and ten years later (at age 25), allowing us to capture both immediate and longer-term effects.

Our study is also informative on the dynamic technology of the production of health, a topic of great recent research interest. For example, Heckman (2007) models current capabilities, which include cognitive and non-cognitive ability and health, as a function of initial values and all past and current investments. While he points to several features of this function that research has elucidated, other features have not been empirically determined. Understanding this process is important for example when designing policies to ameliorate disadvantage. We are able to estimate the effects of investments in child health for a disadvantaged population in the pre-teen and early teen years on health over ten years later. We thus advance our understanding of the process of how health at one age is altered by investments at another.

We find no immediate effects of the expansions on health care utilization at age 15. However, we find sizeable effects of Medicaid eligibility in childhood on hospitalizations and emergency department visits at age 25 among black cohorts who gained coverage. Black cohorts born immediately after the cutoff are estimated to experience approximately 7 to 15 percent fewer hospitalizations and 2 to 5 percent fewer emergency department visits at age 25 relative to those born just before the cutoff. Our results are particularly pronounced for hospitalizations and emergency department visits related to chronic illnesses and among patients from low-income zip codes. Additionally, our analysis suggests that these effects are largest in states where the discontinuity in the cumulative number of Medicaid eligible years is greatest. We do not find reductions in the utilization of non-blacks (who experienced smaller gains in Medicaid eligibility at the birth date cutoff), nor do we find effects for hospitalizations related to appendicitis or injury, two conditions that are unlikely to be affected by access to care in childhood. Placebo tests using earlier birth cohorts to estimate breaks at non-discontinuity points indicate that any discontinuities at these false “cutoff” points are small relative to the effects we estimate at the true birth date cutoff.

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 3 and 5 percent of the initial cost of the Medicaid expansions for children were "offset" by lower hospitalization and emergency department usage at age 25 alone, with a substantial fraction of these cost savings accruing to the government in the form of lower hospital payments for publicly-insured patients. If these effects persist, then the size of the cost offset is likely to be even greater. Second, our results highlight the importance of evaluating these programs over a longer time period. Indeed, we find no impact of Medicaid coverage in our analysis of the "immediate" effect at age 15, but do find effects later in life at age 25. These findings suggest that the benefits of insurance may only materialize over a long horizon.

II. Background

High-quality analyses of Medicaid eligibility expansions for children consistently show that Medicaid increases health care utilization, including hospitalizations, in the short term.[2] However, there are fewer studies of the effects of gaining Medicaid on children's health and the evidence from this literature is mixed. A number of studies using parental reports of child health find no evidence of improvement under public insurance, while several papers document significant declines in child mortality.[3] Thus, the effect of gaining health insurance on health remains an important but unanswered question.

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

[2] See evidence of increased hospital use in Dafny and Gruber (2005), Currie and Gruber (1996a) and Boudreaux, Golberstein, and McAlpine (2014). In addition, Currie and Gruber (1996a), Card and Shore-Sheppard (2004), and Currie, Decker and Lin (2008) present evidence indicating an increase in annual doctor visits under expanded public insurance.

[3] For example, Currie, Decker and Lin (2008), Currie and Gruber (1995), De La Mata (2012), and Racine et al. (2001) find no change in subjective measures of child health such as child health status and activity limitations. Meanwhile, Currie and Gruber (1996a, 1996b), Goodman-Bacon (2014), Howell et al. (2010), and Wherry and Meyer (forthcoming) find significant effects on infant or child mortality. Not all studies, however, find mortality improvements; Decker, Almond, and Simon (2015) find no evidence of changes in maternal and child mortality under the rollout of Medicaid. See Howell and Kenney (2012) for further discussion of this literature.

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 (forthcoming) examine the later life mortality of cohorts born before and after the 1983 cutoff date specified in many expansions of Medicaid. They provide evidence linking this 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. Boudreaux, Golberstein and McAlpine (2014) use variation in the timing of the introduction of the Medicaid program across states in the 1960s to identify long-term effects among cohorts with different exposure to the program. They find that those who gained access to Medicaid early in childhood were less likely to report having a chronic illness as an adult. Brown, Kowalski, and Lurie (2015) also use state-level variation in the timing of the Medicaid expansions for children in the 1980s to examine long-term effects and find that cohorts who gained coverage have higher wages, receive lower earned income tax credit payouts, have higher graduation rates and lower mortality as adults. Also relying on state-level variation, Cohodes et al. (forthcoming) find that cohorts who gained coverage in childhood as a result of these Medicaid expansions have higher educational attainment, and Miller and Wherry (2015) find that cohorts whose mothers had higher eligibility rates for prenatal coverage while the cohort was in utero had better health outcomes and fewer hospitalizations in adulthood related to chronic health conditions, as well as higher rates of high school graduation.

In this paper, we add to this literature by exploiting the discontinuity in Medicaid eligibility and coverage among those born around September 30, 1983 to study the effect of childhood Medicaid eligibility on hospitalizations and emergency department visits at age 25. There is only limited study of the long term health effects of Medicaid eligibility, so our paper adds to this literature. In addition, with the exception of Wherry and Meyer (forthcoming), 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 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 allows us to examine the effects of childhood Medicaid coverage in a way that is arguably more credible because it does not rely on using policy changes at the state level as an instrument for eligibility.

III. The Policy Discontinuity

Discontinuity in Eligibility

Prior to the 1980s, eligibility for Medicaid for non-disabled children was primarily limited to children in families receiving cash welfare under the Aid to Dependent Families with Children (AFDC) program. Recipients of AFDC benefits were primarily single-mother families with very low income levels, often well below the poverty line.[4] Beginning in the mid-1980s, Congress took steps to expand eligibility for Medicaid to children not participating in AFDC who would otherwise be ineligible for Medicaid benefits. In a series of legislative acts, eligibility for Medicaid was expanded to all children with family incomes at or below the poverty line, regardless of family structure or participation in the AFDC program.

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 birthdate cutoff faced very different eligibility criteria for Medicaid during their childhood years. Wherry and Meyer (forthcoming) simulate childhood eligibility for public health insurance for cohorts born on either side of this birthdate cutoff.[5] They show that this unique feature of the expansions led to a large discontinuity in the number of years of Medicaid eligibility during childhood for cohorts born at this birthdate.

Given the nature the expansions, the discontinuity was largest for children with family incomes below the poverty line and above AFDC income levels. Figure 1 displays the average number of years of childhood eligibility for public insurance by birth month cohort for children in families

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[4] Income limits for the AFDC program were established by states and ranged from 14 to 79 percent of the federal poverty line in 1989 (U.S. General Accounting Office 1989).

[5] The authors use a random sample of children of ages 0-17 from each year of the 1981-1988 March Supplement to the Current Population Survey (CPS) and estimate eligibility for this pooled sample if born in each month between October 1979 and September 1987. They employ detailed federal and state public health insurance eligibility rules for the years 1979 to 2005 to estimate eligibility status for each month during childhood through age 17. This simulation holds family characteristics, including state of residence, family structure and size, parent employment and family income, constant over the child’s lifetime. See Wherry and Meyer (forthcoming) for additional information.

with incomes below 150 percent of the federal poverty line. The magnitude of the discontinuity in childhood eligibility at the September 30, 1983 cutoff is largest for children in families with incomes between 75 and 100 percent of the poverty line. The gain represents an additional 4.6 years of eligibility during childhood. Children with incomes between 50 and 75 percent of poverty, as well as those with incomes between 25 and 50 percent of poverty, also experience sizeable gains with an additional 3.4 and 2.0 years of eligibility, respectively.

Figure 2 reveals that the gain in eligibility was primarily concentrated at ages 8 to 14 for children born immediately after the birthdate cutoff. This graph plots the share of the September versus October 1983 birth cohorts eligible for public health insurance at each age during childhood by race. Eligibility levels are similar for the two cohorts prior to age 8 and again starting at age 15. These cohorts were approximately 8 years of age at the implementation of the Omnibus Budget Reconciliation Act of 1990 (OBRA90), which required all state Medicaid programs to cover children under age 19 born after September 30, 1983. This legislation was responsible for most of the discontinuity. 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. We examine health care utilization for cohorts born just before and after September 30, 1983 following the differential gain in Medicaid eligibility at age 15 and then 10 years later at age 25.

We also examine differential effects of the expansions by race and by state of residence. 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. This is over twice the average years of eligibility gained by non-black children, who experienced a 9 percentage point gain in eligibility across the birth date threshold that led, on average, to 4.4 additional Medicaid-eligible years throughout childhood.[6]

[6] Wherry and Meyer (forthcoming) estimate eligibility gains by child race and state of residence using a similar methodology but rely on a national sample that draws children from the CPS by race and state cells. The estimates presented here differ slightly in that they rely on state-specific

The gain in Medicaid eligibility for children born after the cutoff also varied by state due primarily to differences in Medicaid policies in place before the expansions and differences in state socioeconomic characteristics.[7] 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% FPL vs. 79% FPL) and the concentration of children in the state with family incomes between AFDC eligibility thresholds and the poverty line.

Table 2 presents estimates of the average eligibility gain at the cutoff for each state in our study.[8] These estimates were calculated using state-specific samples of children from the 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.[9] We use this variation to estimate differential effects across states associated with the policy change.

Discontinuity in Coverage and Utilization

In addition to a demonstrated discontinuity in childhood eligibility, we also measure any corresponding discontinuity in childhood coverage. It is important to bear in mind, however, that even if they did not take active steps to enroll in the program, all children gaining eligibility for

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samples of all children ages 0-17 pooled for the 1981-1988 years of the March CPS. See the appendix for additional discussion.

[7] Although many of the expansions were first introduced as state options, Wherry and Meyer (forthcoming) estimate that, when holding socioeconomic characteristics fixed, the majority of the variation in eligibility at the September 30, 1983 cutoff across states resulted from the federal requirement for all states to cover poor children born after this date rather than from optional state expansions. See the appendix of their paper for additional information.

[8] Some prior work has excluded Arizona from analyses of Medicaid expansions for children due to its late implementation of Medicaid. We include Arizona in this analysis because, although slow to implement Medicaid, the state provided government-supported health care for families on AFDC both prior to and following the introduction of its Medicaid program in 1982 (Freeman and Kirkman-Liff 1985). In addition, the federal mandate to expand eligibility for children born after September 30, 1983 with family incomes up to the poverty line in 1991 applied to all states including Arizona (Congressional Research Service 1988).

[9] California represents a large population and experienced the smallest policy discontinuity in our sample. For that reason, we also estimate models that exclude California from our analysis (see Appendix Tables 4-8). As expected, specifications that exclude California tend to uncover larger but somewhat less precise effects.

Medicaid had “conditional coverage” in that their expenses were covered in the event of hospitalization or the need for costly medical care (Cutler and Gruber 1996). Not only could eligibility be granted retroactively for a period of up to 3 months prior to the date of application, but many states were giving children the opportunity to sign up for Medicaid at the sites where they received health care (Congressional Research Service 1993). Since the value of Medicaid is highest when children are sick, parents are likely to wait until medical care is needed to sign up for coverage (Marton and Yelowitz 2014).

Card and Shore-Sheppard (2004) first examined changes in Medicaid enrollment for children born after September 30, 1983 following the expansions in Medicaid eligibility. The authors found Medicaid take up rates of between 8 and 11 percent among the newly eligible, with little evidence of substitution of public for private coverage (i.e., crowd out). In an analysis of similar spirit, we explore differences in the discontinuity in coverage by child race. We use the pooled 1992-1996 National Health Insurance Survey (NHIS) Health Insurance Supplements to examine changes in reported Medicaid coverage during the last month for cohorts born after September 30, 1983 at ages 8-13.[10] We estimate a simple regression discontinuity model and regress Medicaid coverage on an indicator for birth cohorts October 1983 and later, a quadratic function in birth month cohort interacted with this indicator, and a set of calendar month fixed effects. We estimate this specification using 4-, 3-, and 2-year observation windows of birth month cohorts on either side of the birthdate cutoff. Standard errors are heteroskedasticity-robust and are allowed to be non-independent within birth month cohort cluster. We also use local linear regression to estimate the discontinuity in Medicaid coverage at the September 30, 1983 cutoff relying on two different data-driven optimal bandwidth selectors. More details on these estimation methods are described later in Section V.

Figure 3 plots reported levels of Medicaid coverage for each birth month cohort with a 4-year window on each side of the birthdate cutoff (centered at October 1983). The lines are fitted values from a quadratic regression function in birth month cohort interacted with a dummy variable for cohorts born after September 30, 1983. The graphs for blacks and, to a lesser extent, for all races, show evidence of an increase in Medicaid coverage at the cutoff. When we look

[10] As recommended by the National Center for Health Statistics, we exclude over-sampled Hispanic respondents in the 1992 NHIS in this analysis; however, results are very similar when these respondents are included.

separately at children in households with incomes below the poverty line, we see additional visual evidence of a discontinuity in coverage.

Table 3 presents the corresponding regression estimates. For all races, we see some evidence of an increase in Medicaid coverage with point estimates ranging between 1 and 2 percentage points. However, for the most part, the estimates are not statistically significant. We do, however, 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 zero at the .05 level in four of five specifications and at the .10 level in the remaining case. Given our previous estimate that 17 percent of black children gained eligibility at the cutoff, these estimates represent a take-up rate on the order of approximately 29 to 47 percent. For non-blacks, we do not find a significant increase in Medicaid coverage and the point estimates are much smaller, indicating less than a one-percentage point change in coverage and implying a take-up rate of at most six percent. Examining children with families below the poverty line only, we find an increase in Medicaid coverage of between 6 and 9 percentage points. The estimates are significant at the .01 level in three of five specifications and at the .10 level in the remaining two cases. We find no change among children in families with incomes above the poverty line.

We also examined changes in reports of any insurance coverage during the last month (also in Table 3). The change in overall insurance coverage for black children is not statistically significant, but the coefficients suggest between a 2 and 5-percentage point increase among those born just after the cutoff. The fact that the change in Medicaid coverage is larger than the change in overall insurance coverage suggests that these expansions were associated with some crowd-out (i.e., that some children enrolled in Medicaid who would otherwise have enrolled in private insurance). In additional analyses, we examined changes in the probability of a doctor visit in the last 12 months, as well as any short-stay hospital visits (not related to delivery) (see Appendix Table 1). We find some evidence of increases in doctor visits among black children, although it is not consistently significant across bandwidth choice. All figures associated with this analysis may be found in the appendix (Appendix Figures 1-3).

Summary

Changes in Medicaid eligibility and coverage documented in Figures 1 through 3 and Tables 1 through 3 lead to important empirical implications. There is clear variation in treatment by race, poverty, and state, and the differences range from zero to substantial. Accordingly, if Medicaid coverage has long-term effects on health and use of medical care such as hospitalization, then it is plausible to expect that effects will vary in a way consistent with the variation in treatment.

IV. Data

To conduct our analysis, we combine discharge-level hospital data from three sources. First, we use hospitalization data from the Healthcare Cost and Utilization Project (HCUP) State Inpatient Databases. We purchased 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 discharge-level information on all inpatient hospitalizations that occurred in acute care hospitals 1999 in Arizona, Hawaii, Iowa, New Jersey, New York, Oregon, and Wisconsin, and in 2009, on Arizona, Arkansas, Colorado, Hawaii, Iowa, Kentucky, Maryland, Michigan, Nebraska, New Jersey, New York, North Carolina, Oregon, South Dakota, Utah, Vermont, and Wisconsin.[11] We supplement these data with the census of hospital discharges that occurred in Texas and California in 1999 and 2009, obtained 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 9 states in 1999 and for 19 states in 2009.[12] To our knowledge, this represents all of the available hospital discharge data containing information on the patient's date of birth for these years.

In addition to hospital discharge data, we use data from all State Emergency Discharge Databases available from HCUP in 2009 that include information on the patient's date of birth. These databases provide information on all outpatient emergency department visits that occurred

[11] The inpatient data from Nebraska and North Carolina in 2009 do not have information on patient race and are therefore excluded from all models run by race. Similarly, the inpatient data from Oregon in 1999 do not include information on race and is excluded from all models run by race for this year.
[12] 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 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 emergency department and released the same day, rather than being admitted to the hospital. The data are only more recently available and not available for 1999.

Both the hospital discharge and emergency department 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 zip code (defined as a zip code with median income below $39,999).[13] We classify primary diagnoses as relating to "chronic" or "non-chronic" conditions using the Chronic Condition Indicator software distributed by HCUP.[14] We exclude hospitalizations and ED visits for diagnoses related to pregnancy and delivery.

Combined, our hospitalization data include 689,546 discharge-level observations for diagnoses not related to pregnancy and delivery and 3.9 million emergency department visits in 2009 for patients born between 1979 and 1987. Our hospitalization sample covers approximately 36 percent of the national population in 1999 and 50 percent of the national population in 2009, and our emergency department visit sample covers about 29 percent of the US population.[15] These large sample sizes are critical for our analysis because they allow us to detect changes in hospitalizations and emergency department visits even among young populations with low utilization rates and for conditions that are relatively rare.

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

[14] Downloaded from http://www.hcup-us.ahrq.gov/toolssoftware/chronic/chronic.jsp on 11/11/2014. The HCUP Chronic Conditions Indicator categorizes all diagnosis codes as chronic or not chronic. The definition of a chronic condition requires that it lasts 12 months or longer and that it either (1) places limitations on self-care, independent living, and social interactions; or (2) needs ongoing intervention with medical products, services, and special equipment. The classification was developed based on an existing body of work on the chronicity of conditions and in consultation with a physician panel.

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

Table 4 presents descriptive statistics on hospital and emergency department utilization rates for our sample. The first three columns display hospitalization rates (per 10,000 individuals) for 15 year-olds in 1999, the first year for which we have data. In 1999, there were approximately 260 hospitalizations (not including hospital visits related to pregnancy and delivery) per 10,000 population for all races. Hospitalization rates at this age were higher for blacks, who experienced approximately 323 hospitalizations per 10,000 individuals, and lower for non-blacks, who experienced approximately 253 hospitalizations per 10,000 individuals. About 53 percent of these hospitalizations were for chronic illnesses overall; for blacks, chronic illnesses represented about 60 percent of total hospitalizations. For 15-year-olds, the most common of these chronic illnesses are mental disorders, followed by asthma and diabetes.

The next three columns display hospitalization rates for 25 year-olds in 2009. Hospitalization rates are more common for this age group: among all races, there were 326 hospital visits per 10,000 population; among blacks, there were 517 visits per 10,000 population; among non-blacks, there were 304 visits per 10,000 population. 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 non-blacks. About 57 percent of hospitalizations of black patients were for chronic conditions and about 45 percent of hospitalizations of non-black patients were for chronic conditions. The most common chronic condition for this age group is also mental disorders. The second most common is diabetes and the third most common is asthma.

Emergency department visits are more common than hospitalizations and tend to treat less severe conditions. ED use is described in columns 7 through 9. On average, there are 3,152 emergency department visits per 10,000 individuals in 2009, almost ten times the hospitalization rate. Among blacks, this rate is 5,715 per 10,000 individuals; among non-blacks, it is 2,891 per 10,000 individuals. ED visits tend to be for acute conditions; only 12 percent of ED visits are for chronic illnesses, relative to 47 percent of hospitalizations for this age group. 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 non-blacks.

These descriptive statistics highlight the importance of using a large sample to investigate utilization in these age groups. With a per capita hospitalization rate of under 0.026 in 1999, and

0.033 in 2009, it would be very difficult to detect changes in utilization rates among the relevant cohorts using, for example, survey data. By employing large administrative datasets, we are able to credibly investigate changes in hospitalizations and ED visits even though overall usage rates in these age groups are low.

V. Empirical Strategy

To estimate the impact of childhood Medicaid eligibility on later life hospitalization and ED visits, we use a regression discontinuity approach and compare outcomes for cohorts born just before and after the September 30, 1983 birthdate cutoff. We rely on both a parametric specification (e.g., polynomial) and local linear regression to estimate the discontinuity in outcomes at the birth date cutoff point. These complementary methods offer tradeoffs in terms of bias and variance and are presented together to assess the stability of results (Lee and Lemieux 2010). We use the log number of hospitalizations or ED visits as the dependent variable, which assumes that population trends smoothly across birth month cohorts.[16] 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. Each cohort born between October 1979 and September 1987 is denoted using the integer values $c \in [-48, 47]$, where $c=0$ for the first cohort born after the cutoff (October 1983). We present results for our main specification that relies on a 4-year ($c \in$ [-48, 47]) window of birth month cohorts on either side of the cutoff. Additionally, we show alternative analyses that use 3-year ($c \in$ [−36, 35]) and 2-year ($c \in$ [−24, 23]) windows of birth month cohorts.

The regression specification is given by

16 Wherry and Meyer (forthcoming) find evidence of a decrease in mortality at ages 15-18 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.

$$\log(y_c) = \alpha + \beta D_c + \gamma_0 c + \gamma_1 c^2 + \gamma_2 D_c \cdot c + \gamma_3 D_c \cdot c^2 + \delta_m M_c + \varepsilon_c \qquad (1)$$

where y_c represents the number of hospitalizations or ED visits for a given birth cohort and D_c is an indicator for cohorts born after September 30, 1983 ($c \geq 0$). We include a quadratic function in birth month cohort c that is allowed to differ on both sides of the cutoff point by including an interaction term for those cohorts born after the cutoff. In addition, we include calendar month dummies M_c to control for variation in outcomes related to the link between timing of birth and family characteristics (Buckles and Hungerman 2012). The inclusion of these dummies will also net out the effects of policies that may differentially affect cohorts born in certain months (for example, school entry dates). For each outcome, we present visual evidence by plotting in two-month bins the residuals for birth month cohorts in the 4-year observation window from a regression on the set of calendar month dummies. The fitted line is the result of regressing the residuals on a quadratic function in birth month cohort interacted with the post-September 30, 1983 dummy.

We also use local linear regression to estimate the discontinuity in outcomes at the cutoff point. The estimation is conducted with a triangular kernel and we present results that employ two different optimal bandwidth selector procedures proposed by Imbens and Kalyanaraman (2012) and Calonico, Cattaneo, and Titiunik (2014).

These methods estimate the effect of the eligibility expansions averaged across the full sample of children at the cutoff. This is an example of a “fuzzy” RD design because factors other than date of birth determine eligibility for and take-up of public health insurance. Although we do not have information in our data on whether individuals were eligible for or took up public health insurance, we are able to examine outcomes for certain subsamples that were more likely to be affected by the change in Medicaid policy. In particular, we examine outcomes separately by race and income in accord with the variation in treatment documented previously.

We also investigate differences in outcomes by state of residence. As described earlier, states’ socioeconomic characteristics and eligibility criteria in place prior to the expansions led to variation in the size of the gain in Medicaid eligibility for children born after September 30,

1983. We exploit this variation and estimate changes in outcomes associated with the relative size of the discontinuity in childhood eligibility in each state.

We estimate the following specification for the 4-year window of observations around the cutoff:

$$\log(y_{cs}) = \alpha + \beta_0 D_c \cdot G_s + \beta_1 D_c + \gamma_{0s} c_s + \gamma_{1s} c^2{}_s + \gamma_{2s} D_c \cdot c_s + \gamma_{3s} D_c \cdot c^2{}_s + \delta_s + \delta_m M_c + \varepsilon_{cs} \quad (2)$$

where we regress the log of a given state-cohort outcome y_{cs} on an indicator for cohorts born after the cutoff D_c and its interaction with a measure of the size of the discontinuity in each state in eligibility-years G_s. In addition to including state and calendar month of birth fixed effects, we also include second order polynomial trends in birth month cohort that differ on either side of the discontinuity. In more flexible models, we allow these trends to vary by state. This regression is weighted using the state population of individuals in the range of ages used in these regressions, i.e., ages 22 to 30. These estimates are from the 2007 to 2011 5-year estimates of the American Community Survey. Due to the small number of states in our sample, we use the studentized wild bootstrap method with 999 bootstrap repetitions clustered by state for hypothesis testing and constructing confidence intervals. This method has been shown to perform well even when there are relatively few clusters (see Cameron, Gelbach and Miller 2008).

Some state by birth month cohort cells have zero hospitalizations for blacks; if this is the case for any birth month cohort in a state, we drop that entire state when conducting the analysis for the black and non-black subsamples. This leads us to drop two states in our analysis.

VI. Results

Figure 4 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. As seen in the figure, hospitalizations are correlated with age (i.e. birth month), which is the running variable in the RD estimation. As noted earlier, we address the possibility of different trends on either side of the September 30, 1983 cutoff by allowing the polynomial in birth month cohort to have different coefficients on either side of the cutoff. Visually, the figure reveals little evidence of a discontinuity in outcomes at the September 30, 1983 threshold. Estimates of the discontinuity from the regression analysis reported in Table 5 support this conclusion. Overall, we do not find a consistent pattern suggesting changes in hospital utilization in this year. We observe statistically significant decreases in non-pregnancy hospitalizations and non-chronic illness hospitalizations for all races, but only in the local linear regression models. We find some evidence of significant increases in non-pregnancy hospitalizations and chronic-illness hospitalizations for non-black patients, but the coefficient estimates are only significant in some of the specifications that use a polynomial in birth cohort. We also note that the statistical power of the analysis is modest due to both the smaller number of states available for this year and the low rates of utilization for this age group.

Figure 5 displays hospitalization outcomes in 2009 when cohorts born around the cutoff were approximately 25 years old. Table 6 presents the corresponding discontinuity estimates. Among all races, we find no evidence of a significant reduction in hospitalizations for those born after the birthdate cutoff. However, for blacks, there is a notable drop in hospitalizations visible at the cutoff. The regression analysis indicates a reduction in hospitalizations of between 7 and 15 percent for those cohorts born just after the September 30, 1983 date, depending on the specification. All estimates are statistically significant with the exception of the estimate under the global polynomial specification with the largest window of observations, which is significant at the 10 percent level. Furthermore, there is clear evidence of a significant decline in hospitalizations related to chronic illness. Our estimates indicate declines in hospitalizations for chronic illnesses on the order of 11 to 18 percent across specifications. For hospitalizations related to non-chronic illness, the estimated decline is smaller—3 to 11 percent—and only significant when using local linear regression methods. We do not find any evidence of a similar improvement for non-blacks. We find some evidence of an increase in hospitalizations for this group, but the estimates are not significant in most specifications and are not supported by the visual evidence presented in Figure 5.

Figure 6 and Table 7 present similar results for emergency department visits. We find a reduction in rates of ED visits of between 2 and 5 percent among black cohorts born immediately after the birth date cutoff, although the estimates are not significant across all bandwidth choices. When we examine ED visits by their relation to chronic illness, we find evidence of a sizeable decline in visits related to chronic illness (of between 10 and 15 percent). For all races and non-blacks, we find no 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 4,900 fewer inpatient hospitalizations among black cohorts born during the first year after the cutoff at age 25.[17] The change in the probability of gaining eligibility across the birth date cutoff was about 17 percentage points for blacks. If we assume that the reduction in hospitalizations observed in 2009 is entirely a result of the eligibility expansion, the point estimates from our specifications 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 percent 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.[18] Overall, while the point estimates are somewhat large, they are plausible. In addition, the 95% confidence intervals on our estimates allow for the possibility of smaller effect sizes.

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. However, because parents tend to enroll children when they become ill or injured (Marton and Yelowitz 2014), this calculation would describe the local effect on (most likely) the sickest children with the highest rates of hospitalization. This may present an overly optimistic view of what Medicaid coverage expansions can accomplish for the average Medicaid-eligible child. Nonetheless, we perform such a calculation. Based on the estimates presented in Tables 2 and 3, we estimated take-up

[17] Using the Census Estimate that in 2009 there were 617,000 blacks age 25, and that the average hospitalization rate at age 25 for blacks was 517.1 per 10,000 (Table 4).
[18] Case, Lubotsky and Paxson (2002) find that children from low-income families have worse health in childhood, and that the differences between children raised in low- and high-income families become more pronounced as the children grow older and enter adulthood.

rates on the order of 29 to 47 percent.[19] A take-up rate of 29 percent implies that for every 100 black children who enrolled in Medicaid there were between 7.3 and 15.7 fewer hospitalizations at age 25. Meanwhile, a take-up rate of 47 percent implies between 4.5 and 9.7 fewer hospitalizations for every 100 black children who enrolled in Medicaid.

Similarly, our point estimates imply that, nationally, there were approximately 7,100 and 17,600 fewer emergency department visits experienced by blacks born the first year after the cutoff at age 25. Again assuming this reduction is driven entirely by the eligibility expansion, this estimate implies that there were between 6.7 and 16.8 fewer emergency department visits at this age for every 100 black children made eligible as a result of the expansions. Comparing this to average ED use in the population of blacks suggests that gaining an average of 4.8 additional years of Medicaid eligibility in childhood lowers emergency department use by 12 to 29 percent at age 25. However, as we noted previously, baseline ED use among adults who grew up in low-income families is likely higher than average ED use in the population. When scaled by our estimate of 29 percent take-up, this implies that there were between 23.2 and 58.0 fewer ED visits at age 25 for every 100 black children newly enrolled in Medicaid. Using our higher take-up estimate (47 percent) implies between 14.3 and 35.8 fewer ED visits for every 100 new enrollees.

Low-income Zipcodes

Next we examine changes in hospitalizations and ED visits in 2009 for patients from low-income zipcodes (Tables 8-9 and Figures 7-8).[20] If children who grew up in poor families still reside in low-income zipcodes, we might expect to see larger changes for patients from these zipcodes. We find a reduction in total hospitalizations of between 10 and 23 percent among black cohorts in low-income zipcodes born just after September 30, 1983, and the coefficient estimate is statistically significant at the .05 level in four of our five specifications. This range of estimates indicates that the effects are larger in low-income zipcodes than in the full sample of zipcodes (7

[19] These takeup rates as well as others in the literature should be interpreted cautiously since they are subject to substantial biases due to measurement error both in eligibility and reported coverage (Klerman et al. 2009). Note that measurement error in a binary dependent variable generally leads to bias (Hausman, Abrevaya and Scott-Morton 1998).
[20] The HCUP hospitalization data includes information on median income of the patient's zip code in 2009 only, so we are unable to conduct this analysis with the 1999 data.

to 15 percent). In addition, the decline appears to be concentrated among hospitalizations related to chronic illness, where we see reductions of between 15 and 28 percent that are statistically significant in all specifications. There is no significant reduction in hospitalizations for non-chronic illnesses.

Similarly, we find evidence of a larger decrease in ED visits for blacks in low-income zipcodes than for all blacks with estimates ranging between 4 and 6 percent, although the estimates are only significant in certain specifications. There is some evidence for reductions in chronic and non-chronic 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 from persons from low-income zipcodes of all races or non-blacks.

In both the full sample and the low-income sample we estimate different effects across race groups, estimating large reductions for blacks, but not for non-blacks. There are two likely explanations for this heterogeneity. First, as described in Section III, the policy change had a larger impact for blacks, who were more than twice as likely to experience a gain in eligibility. We also find less evidence of take-up of Medicaid coverage among non-blacks as compared to blacks. Second, as noted in Section IV, blacks experience dramatically higher hospitalization rates on average than non-blacks. This difference is particularly pronounced for hospitalizations related to chronic illnesses: for these types of hospitalizations, the utilization rate is more than double among blacks than among non-blacks (as compared to acute conditions, for which the utilization rate is only 34 percent higher among blacks). As a result, it may be the case that the intervention itself was more effective for this race group because this group has a much higher baseline risk.

Heterogeneity by State

In this section, we use the substantial differences across states in the average size of the discontinuity in eligibility-years for cohorts born at the cutoff (see Table 2) as an additional source of variation in our analysis. If our observed changes in utilization across the threshold are indeed driven by differences in Medicaid eligibility in childhood, we might expect the discontinuity in utilization at the cutoff birth date to be larger in states where the change in eligibility across that threshold is greater.

To explore this heterogeneity, we estimate the model described by equation (2). The variable G_s is the estimated size of the discontinuity in eligibility that occurs at the cutoff in state *s* (as reported in Table 2). Our coefficient of interest is the interaction between D_c and the size of the discontinuity in the number of Medicaid-eligible years in state *s*, which captures the marginal effect of an additional year of eligibility on the outcome variable. We estimate two versions of this model: a "flexible" version that allows trends by birth month cohort to vary by state and a "restrictive" version that requires the time patterns to be the same in all states. The flexible version may be preferred because it allows birth month trends to be different across states; however, because our number of observations is small relative to the number of parameters we estimate, the less demanding restrictive version is also appealing. We therefore have elected to report both versions, but note that there is little qualitative difference in results.

Table 10 presents the results using hospitalizations in 2009. We find evidence suggesting that the decrease in hospitalizations is most pronounced for individuals living in states with large discontinuities.[21] For patients of all races, we find that a one year increase in the size of the discontinuity at the birth date cutoff reduces hospitalizations by between 6 and 8 percent, although the effect is only significant at the 10 percent level in the flexible model. We also find some evidence of reductions in chronic and non-chronic illness hospitalizations, although in the flexible model the effects are not statistically significant.

The second panel presents results for black patients. Although we observe negative point estimates associated with increases in the discontinuity size across states, none of the effects are statistically significant, and in this case, we have limited statistical power to detect modest effect sizes. The third panel presents results for non-black patients. Here, we find that a one-year increase in the size of the discontinuity is associated with significant reductions in overall hospitalizations and hospitalizations related to both chronic and non-chronic illnesses. Our results suggest that a one year increase in eligibility is associated with a reduction in non-pregnancy hospitalizations of 8 or 10 percent, depending on the specification. We also find a

[21] The results using hospitalizations in 1999, when birth cohorts born around the cutoff birth date are 15 years old, are reported in Appendix Table 3. As in our original specification, we do not find systematic evidence that those born immediately after the cutoff had fewer hospitalizations at this age.

reduction in chronic illness hospitalizations of 7 or 8 percent and a reduction in non-chronic illness hospitalizations of 10 and 12 percent.

In Table 11 we perform a similar analysis using emergency department data. In the top panel we report the results for all races. We find evidence of a decline in ED visits for cohorts born after the cutoff although the estimate is only statistically significant under the flexible model. The coefficient indicates that an additional year of Medicaid eligibility is associated with an 8 percent reduction in ED use at age 25. We also find significant declines for chronic and non-chronic illnesses under the flexible model, where an additional year of Medicaid eligibility is associated with 11 and 7 percent reductions respectively.

Conducting this analysis for blacks, we again find negative point estimates for all model specifications and visit types although none are statistically significant. Among non-blacks, we see that a one year increase in Medicaid eligibility is associated with an 8 percent reduction in overall ED visits, a 15 percent reduction in chronic illness ED visits, and a 7 percent reduction in non-chronic illness ED visits, although as with the results for all races, these effects are only statistically significant in the flexible model.

Because we uncover significant effects for non-blacks and not for blacks in the state-level models, at first glance these results might appear contradictory to those reported in the previous section. However, this is not necessarily the case. First, because our measures of the discontinuity at the state level are race-specific, they account for the fact that the effect of the policy was much smaller on non-blacks than on blacks. The results in the previous section looked at the overall change by race without scaling for the differences in the amount of exposure to treatment. Second, although the effects are not significant, we do observe negative point estimates for all models and visit types among the black sample, suggesting that the effects are indeed larger in states with a larger discontinuity. However, because many states in our sample have small black populations, it is the case that both the estimate of the size of the discontinuity in each state and the birth-month level utilization rates are imprecisely measured. We attempt to account for this by weighting the regressions by the state population; however, we are still forced to drop a large number of birth month observations for blacks due to zeros in the dependent variable.

VII. Sensitivity Analyses

We conduct several sensitivity analyses. First, we estimate the effect of discontinuous Medicaid eligibility on two types of hospitalizations that are unlikely to be affected by medical intervention in childhood: hospitalizations for appendicitis and injury. Second, we use birth month cohorts born between January 1965 and September 1983 to estimate placebo effects at cutoffs where the cohorts did not actually experience a discontinuity in Medicaid eligibility. Third, we examine the sensitivity of the estimates to the exclusion of California, which both comprises a large proportion of our sample and has the smallest discontinuity size of any state in our sample. Finally, we explore the sensitivity of our results to the inclusion of cohort-specific characteristics including measures of health at birth.

Hospitalizations for Acute Conditions

We first present estimates using 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 likely not sensitive to medical care received in the past. For that reason, we believe it is unlikely that coverage in childhood could plausibly 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 2. The first panel shows the results for hospitalizations in 2009 for patients from all zip codes stratified by race. The second panel shows similar results for patients from low-income zip codes. In both panels, we find point estimates that are close to zero, none of which are statistically significant for any race group. Although the confidence intervals are large, we note that the point estimates are smaller in magnitude than those reported for all hospitalizations and chronic illness related hospitalizations and that the direction of the estimates is not consistent, with roughly half of the specifications reporting small, statistically insignificant positive effects and half reporting similarly sized negative effects. Overall, this suggests there was little impact of the policy on these types of visits. This result is consistent with our expectation that these types of visits should not be affected by access to medical care in childhood.

Placebo Estimates at Non-Discontinuity Points

We conduct a second type of placebo test using data on cohorts born prior to the actual eligibility cutoff. We place an artificial "cut off" date in the center of each four year window (eight year period) beginning with cohorts born in January 1965. We then estimate models that mimic our main "global" polynomial specification that uses a 4 year window and the "local" specification that chooses an optimal bandwidth using the Imbens and Kalyanaraman (2012) procedure. Our final placebo "cut off" is placed at September 1979, so that the last month used in the estimation of these placebo effects is September 1983, immediately before the actual change in Medicaid eligibility occurs. This results in 129 "placebo estimates" estimated at birth dates where no policy discontinuity existed. We perform this analysis using the sample of black patients and outcomes where we previously uncovered significant results using traditional inference.

Using these placebo estimates, we construct histograms, which we report in Figures 4 and 5. The effect estimated at the "true" cutoff is shown on the figure as a black vertical line. The two graphs in Appendix Figure 4 show the distribution of placebo statistics for the global polynomial and local linear specifications of the total number of hospitalizations in 2009 for black patients. We find that the true estimate among black patients is large relative to the placebo estimates. Using the global polynomial specification, the true effect exceeds all but 14 (10 percent) of the placebo estimates in absolute value. Using the local linear specification, the true effect exceeds all but 1 (less than 1 percent) of the placebo estimates.

The next two graphs in Appendix Figure 4 show the distribution of placebo estimates for chronic illness related hospitalizations. The effect for blacks estimated at the September 30, 1983 cutoff is larger in absolute value than all but 7 (5.4 percent) of the placebo estimates in the global model and all but 4 (3.1 percent) of the placebo estimates in the local model.

The next four panels of Appendix Figure 4 report the distribution of placebo estimates for hospitalizations of low-income black patients in 2009. Using data on all non-pregnancy hospitalizations, we find that the true effect is larger in magnitude than all but 5 (3.8 percent) of placebo estimates in the global model and it exceeds all placebo effects in the local model. When we conduct this analysis for chronic illness related hospitalizations, we find that the true effect exceeds all placebo estimates in both the local and the global model.

In Appendix Figure 5 we present similar distributions for placebo estimates using emergency department data. Here, our results conform less to the original inference conducted in Tables 7 and 9. We find that the true effect exceeds only about 64 and 45 percent of the placebo effects in magnitude for total hospitalizations in the global and local models, respectively. We find somewhat more promising results for chronic illness related ED visits: using the global model, we find that the true effect exceeds all but 14 percent of the placebo effects and, using the local model, we find that the true effect exceeds all but 6 percent of placebo effects. In the low income sample, we find that the true effect exceeds all but 22 percent of the placebo effects for all non-pregnancy hospitalizations in the global model and a little over half of the placebo effects in the local model.

Overall, the placebo simulations conducted in this section strongly suggest that the estimated effects of the Medicaid policy that we observe on hospitalizations among black cohorts is larger than estimates that we might observe due to chance. In addition, the simulations provide particularly convincing evidence supporting our results for the low-income subsample of blacks. The placebo tests are less convincing when applied to the emergency department results; although the effect of Medicaid on chronic illness emergency department visits estimated at the true discontinuity exceeds the majority of placebo estimates, many of placebo estimates are larger in absolute value than the "true" effect in other models.

Exclusion of California

Throughout our analysis, we include all available states. However, as illustrated in Table 2, some states have relatively small discontinuities in eligibility at the birth date cutoff. In particular, California has the smallest discontinuity in eligibility (about 0.05 years) and represents a large share of the total population in our state sample (about 25 percent). We therefore explore how sensitive our results are to excluding California from our analysis by re-estimating Tables 5 through 9 without California.

We present the results in Appendix Tables 4-8. As expected, when California is excluded, our results tend to be larger and are more likely to be statistically significant. We find that the reform reduces hospitalizations among black patients in 2009 by between 9.5 and 17.0 percent (compared to between 7.1 and 15.4 percent in our original analysis) and all effects are statistically significant; it reduced chronic illness hospitalizations by between 13.2 and 20.1 percent (compared to 10.6 and 18.4 percent). In the low income sample, when California is excluded, we also detect statistically significant reductions in ED visits for all races. These results suggest that ED visits were between 3 and 5 percent lower among patients of all races for those born after the birth date cutoff.

Inclusion of Cohort-Specific Characteristics

Finally, we examine the sensitivity of our estimates to the inclusion of several cohort-specific characteristics drawn from the National Vital Statistics System Birth Data files for 1979 to 1987. We include controls for the following birth outcomes: the fraction of mothers with at least a high school education, fraction of mothers married, and fraction of mothers receiving any prenatal care; the incidence of low birth weight and very low birth weight births; and the number of births. Reported in Appendix Tables 9-11, the results are robust to the inclusion of these covariates.

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

The results presented in this paper provide evidence that expanding Medicaid coverage to children lowers future health care costs by improving health and reducing later life hospital and emergency department use 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.

To conduct this analysis, we estimate models similar to those reported in Tables 6 and 7 but using log of total 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.[22] 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

[22] We do not have total charges for California or Texas, so we impute these values at the three digit diagnosis code level using the charge data from the relevant age group in other states.

Services. We then sum total costs at the birth month cohort level and estimate models as described in equation (2).

The results of this analysis are reported in Table 12 and Figure 9. We find that hospital costs among black cohorts fell by between 8 and 14 percent 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, and 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 \$18.5 and \$32.4 million for black cohorts born the year following the cutoff.[23]

The second panel of Table 11 reports the results for ED costs. We find that the Medicaid expansions reduced ED costs by 5 to 8 percent in 2009. Performing similar calculations as described above, we estimate that the expansions reduced emergency department costs at age 25 among black cohorts born during the following year of between \$6.7 and \$10.6 million.[24]

To arrive at an estimate of the original cost of expanding Medicaid, we rely on the average spending per child enrolled in Medicaid in 1991, which was \$902 per child (in 1991 dollars) (Congressional Research Services (1993)).[25] Multiplying this amount by the average gain in years enrolled in Medicaid using information from Tables 1 and 3 and assuming a 3 percent 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 dollars

[23] 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 percent of all 25-year-old blacks in the U.S. represented in our sample, we estimate total costs for these cohorts at the national level at \$231.6 million.
[24] Similar to the last calculation, we estimate total emergency department costs for the black cohorts born the year before the cutoff at the national level at \$133.3 million based on the total costs of \$28 million observed for these cohorts in our sample states. States in the emergency department sample represent about 21 percent of all 25-year-old blacks in the U.S.
[25] Because most (78 percent) 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.

in 2009.[26] The cost offsets from childhood Medicaid expansions, totaling between $25.2 and $43 million at age 25, therefore represent between 3 and 5 percent of the total cost of the expansions. 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.

Finally, we examine changes in total costs by the source of payment for visits in order to evaluate the incidence of these cost savings. These results are reported in Appendix Table 12. We find that total costs associated with publicly-insured hospital visits fell significantly for black patients, by about 14 percent, although the effects are not statistically significant for all bandwidths. We do not find significant changes in total costs associated with visits for other payers. Total hospital costs excluding pregnancies to public payers for 25 year-old black patients in 2009 were about $125 million. Our results therefore imply that costs to public payers were about $17.5 million lower for individuals born just after the cutoff. Cost savings accruing to the government therefore represent between 54 and 95 percent of the $18.5 to $32.4 million in estimated hospitalization-related cost savings and between 41 and 69 percent of the $25.2 to $43 million in estimated total cost savings.

Among emergency department visits, we find significant reductions of between 7 and 11 percent in costs associated with visits paid for by private insurance. Performing a similar calculation as above, this would imply that ED costs to private insurers were between $3 and $4.8 million lower for individuals born immediately after the cutoff birth date. Our results also indicate that there may have been meaningful reductions in costs for self-pay ED visits, although these effects are not statistically significant in all models. In addition, we find consistently negative, although not statistically significant, effects on costs to public payers.

These calculations indicate that the long-run cost savings from the Medicaid expansions may be quite substantial. Considering the 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. Specifically, our estimates do not incorporate other benefits to government (such as higher

[26] 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 percent (which is the median of the takeup rates calculated based on the estimates in Tables 1 and 3), the $902 cost per year of enrollment per child in 1991, and a 3% discount rate to arrive at our estimate.

income tax receipt and lower earned income tax payments, as found in Brown, Kowalski, and Lurie (2015), or to beneficiaries, such as better education outcomes (Cohodes et al. forthcoming) or lower mortality (Wherry and Meyer forthcoming; Brown, Kowalski, and Lurie 2015).

IX. 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 costs savings that do not materialize until later in life because of improved health. While these long-term cost savings are often cited in policy discussions and debates, very little credible evidence exists on the magnitude of these effects, or even if they are present at all. This is a crucial gap in our understanding of the role of public health insurance coverage as these cost offsets potentially represent a substantial, but previously unaccounted-for, benefit of such programs. There is also limited evidence on the technology of how investments in health in the pre-teen and early teen years affect health among adults. Understanding this process of health production is important in designing policies to ameliorate disadvantage.

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 and emergency department visits at age 25 compared to those born immediately before the cutoff. The effect is particularly pronounced for chronic illness related hospitalizations and ED visits, 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 born one year after the birth date cutoff offset between 3 and 5 percent of the total cost of the expansions we study, and that a large fraction of this cost savings accrued to the government in the form of lower public insurance payments. Our evidence also suggests that health interventions in the pre-teen and early teen years for disadvantaged populations can provide long-term health benefits.

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Table 1. Childhood Medicaid Eligibility Gain for Children Born in October vs. September 1983 by Race Group

	Percent Gaining Eligibility	Average Gain (in Years) for Children Gaining Eligibility	Average Gain (in Years) for Total Child Population
All Races	10.00	4.54	0.48
Blacks	17.25	4.91	0.87
Non-Blacks	8.71	4.41	0.41

Table 2. Average Childhood Medicaid Eligibility Gain for Children Born in October vs. September 1983 by State

	All Races	Blacks	Non-Blacks
Arkansas	1.33	2.31	1.04
Arizona	0.81	1.23	0.79
California	0.05	0.09	0.05
Colorado	0.72	0.88	0.71
Hawaii	0.26	0.35	0.26
Iowa	0.40	0.60	0.39
Kentucky	0.60	0.78	0.59
Maryland	1.10	1.68	0.88
Michigan	0.21	0.30	0.19
Nebraska	0.55	1.16	0.52
New Jersey	0.26	0.56	0.21
New York	0.14	0.19	0.13
North Carolina	0.18	0.27	0.15
Oregon	0.38	0.13	0.39
South Dakota	1.16	2.67	1.15
Texas	1.08	1.29	1.04
Utah	0.16	0.03	0.16
Vermont	0.06	0.02	0.06
Wisconsin	0.23	0.63	0.20

Notes: Weighted averages calculated from simulation of lifetime eligibility if born in September vs. October 1983 for a children ages 0-17 in the pooled 1981-1988 March CPS. See appendix for additional discussion of these estimates and how they differ from Wherry and Meyer (forthcoming).

Table 3. Estimates of Effect of Childhood Medicaid Eligibility on Health Insurance Coverage at Ages 8-13

	All Races		Blacks		Non-Blacks		Households in Poverty		Households Not in Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Medicaid	Any Insurance	Medicaid	Any Insurance	Medicaid	Any Insurance	Medicaid	Any Insurance	Medicaid	Any Insurance
Global polynomial model										
4-Year window	0.010	0.017	0.054**	0.032	-0.003	0.015	0.089***	0.022	-0.001	0.009
	(-0.008, 0.027)	(-0.005, 0.039)	(0.002, 0.105)	(-0.023, 0.086)	(-0.022, 0.016)	(-0.008, 0.037)	(0.044, 0.133)	(-0.017, 0.061)	(-0.013, 0.011)	(-0.012, 0.030)
3-Year window	0.017	0.021*	0.071**	0.047	-0.001	0.017	0.091***	0.046**	0.000	0.008
	(-0.004, 0.038)	(-0.004, 0.045)	(0.012, 0.129)	(-0.017, 0.111)	(-0.024, 0.022)	(-0.007, 0.040)	(0.038, 0.145)	(0.001, 0.092)	(-0.013, 0.013)	(-0.017, 0.033)
2-Year window	0.014	0.006	0.045*	0.014	0.001	0.005	0.057*	0.023	-0.000	-0.001
	(-0.010, 0.038)	(-0.021, 0.032)	(-0.004, 0.095)	(-0.056, 0.084)	(-0.026, 0.029)	(-0.023, 0.033)	(-0.005, 0.118)	(-0.029, 0.074)	(-0.015, 0.014)	(-0.032, 0.030)
Local linear regression										
IK Bandwidth Selector	0.014*	0.019**	0.051**	0.030	0.004	0.017*	0.086***	0.032	0.001	0.009
	(-0.001, 0.028)	(0.000, 0.038)	(0.003, 0.099)	(-0.021, 0.081)	(-0.012, 0.019)	(-0.000, 0.034)	(0.032, 0.141)	(-0.008, 0.072)	(-0.010, 0.013)	(-0.011, 0.029)
CCT Bandwidth Selector	0.023**	0.015	0.077**	0.032	0.006	0.014	0.073*	0.035	0.003	0.004
	(0.001, 0.045)	(-0.008, 0.039)	(0.009, 0.145)	(-0.036, 0.100)	(-0.014, 0.027)	(-0.010, 0.039)	(-0.004, 0.150)	(-0.031, 0.102)	(-0.011, 0.017)	(-0.021, 0.030)
Baseline mean	0.145	0.815	0.319	0.799	0.111	0.818	0.503	0.678	0.042	0.860
N	54,410	58,771	9,000	10,027	45,410	48,744	10,609	11,202	40,258	42,808

Notes: Data from 1992-1996 National Health Interview Survey Health Insurance Supplements. 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. 95% confidence intervals reported in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Rates of Hospital and ED Utilization (per 10,000) for 15 and 25-Year-Olds

	Rate for 15-Year-Olds in 1999			Rates for 25-Year-Olds in 2009					
	Hospitalizations			Hospitalizations			Emergency Department Visits		
	All Races	Black	Non-Black	All Races	Black	Non-Black	All Races	Black	Non-Black
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total visits (excluding pregnancy)	260.44	323.14	253.35	326.44	517.14	303.71	3,152.10	5,714.95	2,890.76
By Relation to Chronic Illness									
visits related to chronic illness	137.90	193.63	131.50	153.66	293.15	137.03	378.23	795.63	335.67
visits not related to chronic illness	122.54	129.52	121.85	172.78	223.99	166.67	2,773.87	4,919.33	2,555.09

Notes: Data for inpatient hospitalizations from states: AZ, CA, HI, IA, NY, OR, TX, and WI (1999 and 2009), as well as AR, CO, KY, MD, MI, NJ, SD, UT, and VT (2009 only). Data for emergency department visits from states: AZ, CA, HI, IA, KY, NJ, NY, UT, and WI. 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. Hospitalizations and ED visits exclude those related to pregnancy and delivery.

*** p<0.01, ** p<0.05, * p<0.1

Table 5. Estimates of Effect of Childhood Medicaid Eligibility on Log Hospitalizations at Age 15 (1999)

	All Races			Blacks			Non-Blacks		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Chronic	Non-Chronic	All	Chronic	Non-Chronic	All	Chronic	Non-Chronic
Global polynomial model									
4-Year window (*N*= 96)	0.001	-0.026	0.020	-0.025	-0.071	0.039	0.042**	0.042	0.041*
	(-0.037, 0.040)	(-0.089, 0.037)	(-0.023, 0.063)	(-0.142, 0.092)	(-0.246, 0.105)	(-0.077, 0.156)	(0.009, 0.074)	(-0.013, 0.096)	(-0.002, 0.084)
3-Year window (*N* = 72)	-0.001	-0.001	-0.003	0.020	0.028	0.022	0.026	0.028	0.025
	(-0.041, 0.039)	(-0.071, 0.069)	(-0.050, 0.044)	(-0.125, 0.166)	(-0.174, 0.230)	(-0.127, 0.170)	(-0.011, 0.064)	(-0.039, 0.095)	(-0.025, 0.074)
2-Year window (*N* = 48)	0.017	0.033	-0.001	0.034	-0.031	0.135	0.060***	0.087**	0.038
	(-0.032, 0.065)	(-0.043, 0.110)	(-0.068, 0.066)	(-0.150, 0.217)	(-0.319, 0.257)	(-0.043, 0.314)	(0.020, 0.100)	(0.017, 0.157)	(-0.030, 0.107)
Local linear regression									
IK Bandwidth Selector	-0.028**	-0.021	-0.041**	-0.013	-0.005	0.014	0.018	0.027	0.004
	(-0.054, -0.002)	(-0.069, 0.027)	(-0.076, -0.005)	(-0.121, 0.094)	(-0.165, 0.155)	(-0.104, 0.133)	(-0.012, 0.049)	(-0.016, 0.069)	(-0.040, 0.048)
CCT Bandwidth Selector	-0.055**	-0.029	-0.067***	-0.021	-0.027	0.009	0.032	0.020	0.038
	(-0.103, -0.008)	(-0.090, 0.033)	(-0.112, -0.022)	(-0.163, 0.122)	(-0.281, 0.227)	(-0.114, 0.132)	(-0.014, 0.078)	(-0.032, 0.072)	(-0.034, 0.110)

Notes: Sample includes birth-month observations from pooled AZ, CA, CO, HI, IA, MD, MI, NJ, NY, TX, VT, and WI data. Models with all races also include OR. 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. 95% confidence intervals reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Estimates of Effect of Childhood Medicaid Eligibility on Log Hospitalizations at Age 25 (2009)

	All Races			Blacks			Non-Blacks		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Chronic	Non-Chronic	All	Chronic	Non-Chronic	All	Chronic	Non-Chronic
Global polynomial model									
4-Year window (*N*= 96)	0.002	0.010	-0.006	-0.071*	-0.106**	-0.025	0.016	0.038*	-0.001
	(-0.025, 0.029)	(-0.029, 0.050)	(-0.036, 0.024)	(-0.144, 0.003)	(-0.198, -0.014)	(-0.108, 0.058)	(-0.012, 0.045)	(-0.004, 0.080)	(-0.037, 0.035)
3-Year window (*N* = 72)	0.003	0.014	-0.007	-0.095**	-0.120**	-0.061	0.022	0.046*	0.001
	(-0.027, 0.033)	(-0.032, 0.061)	(-0.038, 0.023)	(-0.176, -0.013)	(-0.223, -0.017)	(-0.158, 0.036)	(-0.011, 0.054)	(-0.005, 0.097)	(-0.036, 0.038)
2-Year window (*N* = 48)	0.023	0.037	0.010	-0.144***	-0.167**	-0.113*	0.054***	0.081***	0.031
	(-0.014, 0.061)	(-0.025, 0.100)	(-0.029, 0.049)	(-0.247, -0.040)	(-0.301, -0.034)	(-0.232, 0.006)	(0.020, 0.089)	(0.031, 0.132)	(-0.013, 0.075)
Local linear regression									
IK Bandwidth Selector	-0.008	-0.004	-0.014	-0.126***	-0.145***	-0.081**	0.016	0.031	0.002
	(-0.032, 0.016)	(-0.040, 0.032)	(-0.035, 0.007)	(-0.183, -0.068)	(-0.216, -0.073)	(-0.151, -0.012)	(-0.012, 0.043)	(-0.011, 0.072)	(-0.027, 0.031)
CCT Bandwidth Selector	-0.016	-0.003	-0.007	-0.154***	-0.184***	-0.114***	0.013	0.032	0.037
	(-0.048, 0.016)	(-0.047, 0.041)	(-0.043, 0.029)	(-0.228, -0.081)	(-0.278, -0.091)	(-0.179, -0.049)	(-0.026, 0.051)	(-0.013, 0.077)	(-0.017, 0.091)

Notes: Sample includes birth-month observations from pooled AR, AZ, CO, CA, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, VT and WI hospital data. Models with all races also include NC and NE. 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. 95% confidence intervals reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Estimates of Effect of Childhood Medicaid Eligibility on Log Emergency Department Visits at Age 25 (2009)

	All Races			Blacks			Non-Blacks		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Chronic	Non-Chronic	All	Chronic	Non-Chronic	All	Chronic	Non-Chronic
Global polynomial model									
4-Year window (*N*= 96)	-0.008	-0.018	-0.007	-0.037**	-0.101*	-0.027*	-0.003	0.002	-0.003
	(-0.029, 0.012)	(-0.049, 0.013)	(-0.029, 0.015)	(-0.071, -0.003)	(-0.210, 0.007)	(-0.057, 0.004)	(-0.024, 0.019)	(-0.026, 0.029)	(-0.026, 0.020)
3-Year window (*N* = 72)	-0.009	-0.025	-0.006	-0.049**	-0.153**	-0.031*	-0.001	0.005	-0.002
	(-0.032, 0.014)	(-0.063, 0.013)	(-0.030, 0.018)	(-0.088, -0.010)	(-0.279, -0.027)	(-0.065, 0.004)	(-0.024, 0.022)	(-0.029, 0.039)	(-0.026, 0.023)
2-Year window (*N* = 48)	0.015	0.011	0.016	-0.028	-0.148*	-0.007	0.024*	0.049**	0.020
	(-0.010, 0.041)	(-0.030, 0.051)	(-0.011, 0.043)	(-0.076, 0.020)	(-0.306, 0.010)	(-0.051, 0.037)	(-0.003, 0.050)	(0.009, 0.088)	(-0.009, 0.049)
Local linear regression									
IK Bandwidth Selector	0.010	-0.014	0.014	-0.022*	-0.123***	-0.009	0.019	0.014	0.018
	(-0.011, 0.032)	(-0.039, 0.010)	(-0.010, 0.037)	(-0.045, 0.002)	(-0.210, -0.036)	(-0.031, 0.012)	(-0.006, 0.044)	(-0.008, 0.037)	(-0.008, 0.045)
CCT Bandwidth Selector	0.017	-0.014	0.025	-0.022*	-0.142***	-0.001	0.025*	0.012	0.027
	(-0.011, 0.045)	(-0.040, 0.011)	(-0.007, 0.056)	(-0.045, 0.002)	(-0.237, -0.048)	(-0.024, 0.022)	(-0.004, 0.054)	(-0.011, 0.035)	(-0.007, 0.060)

Notes: Sample includes birth-month observations from pooled AZ, CA, HI, IA, KY, NJ, NY, UT, and WI ED 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. 95% confidence intervals reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Estimates of the Effect of Childhood Medicaid Eligibility on Log Hospitalizations in Low-Income Zipcodes at Age 25 (2009)

	All Races			Blacks			Non-Blacks		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Chronic	Non-Chronic	All	Chronic	Non-Chronic	All	Chronic	Non-Chronic
Global polynomial model									
4-Year window (*N*= 96)	0.012	0.007	0.017	-0.100	-0.148**	-0.032	0.052*	0.070*	0.038
	(-0.033, 0.058)	(-0.056, 0.070)	(-0.030, 0.065)	(-0.225, 0.024)	(-0.293, -0.003)	(-0.181, 0.118)	(-0.001, 0.106)	(-0.011, 0.151)	(-0.021, 0.096)
3-Year window (*N* = 72)	-0.004	-0.011	0.002	-0.150**	-0.188**	-0.092	0.046	0.062	0.032
	(-0.058, 0.050)	(-0.086, 0.065)	(-0.052, 0.056)	(-0.294, -0.005)	(-0.353, -0.023)	(-0.266, 0.082)	(-0.023, 0.114)	(-0.038, 0.162)	(-0.039, 0.103)
2-Year window (*N* = 48)	0.004	-0.019	0.025	-0.227**	-0.280***	-0.150	0.078*	0.080*	0.075
	(-0.060, 0.068)	(-0.101, 0.064)	(-0.043, 0.092)	(-0.412, -0.043)	(-0.467, -0.094)	(-0.376, 0.075)	(-0.005, 0.161)	(-0.015, 0.174)	(-0.020, 0.171)
Local linear regression									
IK Bandwidth Selector	-0.007	-0.025	0.001	-0.169**	-0.235***	-0.081	0.036	0.037	0.035
	(-0.045, 0.031)	(-0.080, 0.030)	(-0.039, 0.040)	(-0.306, -0.032)	(-0.386, -0.084)	(-0.235, 0.073)	(-0.017, 0.089)	(-0.034, 0.108)	(-0.019, 0.090)
CCT Bandwidth Selector	-0.012	-0.047	0.019	-0.170**	-0.257***	-0.080	0.041	0.038	0.068*
	(-0.058, 0.034)	(-0.113, 0.020)	(-0.036, 0.074)	(-0.308, -0.031)	(-0.433, -0.081)	(-0.245, 0.085)	(-0.023, 0.106)	(-0.049, 0.125)	(-0.010, 0.146)

Notes: Sample includes birth-month observations from pooled AR, AZ, CA, CO, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, VT, and WI hospital data. Models with all races also include NC and NE. 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. 95% confidence intervals reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Estimates of Effect of Childhood Medicaid Eligibility on Log Emergency Department Visits in Low-Income Zipcodes at Age 25 (2009)

	All Races			Blacks			Non-Blacks		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Chronic	Non-Chronic	All	Chronic	Non-Chronic	All	Chronic	Non-Chronic
Global polynomial model									
4-Year window (*N*= 96)	-0.026	-0.045	-0.024	-0.057**	-0.129*	-0.045**	-0.017	-0.012	-0.017
	(-0.075, -0.002)	(-0.095, 0.025)	(-0.064, 0.013)	(-0.106, -0.008)	(-0.283, 0.025)	(-0.086, -0.004)	(-0.056, 0.022)	(-0.068, 0.044)	(-0.059, 0.024)
3-Year window (*N* = 72)	-0.040*	-0.062*	-0.037*	-0.064**	-0.134	-0.052**	-0.033	-0.033	-0.032
	(-0.081, 0.000)	(-0.130, 0.006)	(-0.079, 0.004)	(-0.123, -0.005)	(-0.308, 0.039)	(-0.102, -0.003)	(-0.080, 0.014)	(-0.100, 0.034)	(-0.081, 0.016)
2-Year window (*N* = 48)	-0.013	-0.039	-0.009	-0.036	-0.169	-0.012	-0.005	0.012	-0.007
	(-0.058, 0.032)	(-0.129, 0.051)	(-0.053, 0.035)	(-0.112, 0.041)	(-0.380, 0.042)	(-0.076, 0.052)	(-0.054, 0.044)	(-0.063, 0.087)	(-0.059, 0.045)
Local linear regression									
IK Bandwidth Selector	-0.019	-0.044*	-0.018	-0.035*	-0.114	-0.026*	-0.016	-0.018	-0.015
	(-0.045, 0.007)	(-0.093, 0.006)	(-0.046, 0.011)	(-0.073, 0.002)	(-0.258, 0.031)	(-0.053, 0.002)	(-0.049, 0.017)	(-0.061, 0.026)	(-0.050, 0.019)
CCT Bandwidth Selector	-0.013	-0.052*	-0.007	-0.034	-0.136*	-0.010	-0.008	-0.018	-0.006
	(-0.054, 0.027)	(-0.108, 0.005)	(-0.050, 0.037)	(-0.076, 0.008)	(-0.290, 0.018)	(-0.040, 0.020)	(-0.056, 0.041)	(-0.071, 0.035)	(-0.058, 0.045)

Notes: Sample includes birth-month observations from pooled AZ, CA, HI, IA, KY, NJ, NY, UT, and WI ED 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. 95% confidence intervals reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 10. Estimates of Effect of State Childhood Medicaid Eligibility Gain on Hospitalizations At Age 25 By Race

	Post x Size of Discontinuity Restricted Model	Post x Size of Discontinuity Flexible Model	N
All Races			
Log Total Hospitalizations (excluding pregnancy)	-0.080***	-0.064*	1824
	(-0.112, -0.050)	(-0.139, 0.007)	
By Relation to Chronic Illness			
log hospitalizations related to chronic illness	-0.064*	-0.069	1824
	(-0.138, 0.005)	(-0.198, 0.042)	
log hospitalizations related to non-chronic illne	-0.099***	-0.067	1824
	(-0.145, -0.053)	(-0.149, 0.016)	
Blacks			
Log Total Hospitalizations (excluding pregnancy)	-0.047	-0.017	960
	(-0.161, 0.066)	(-0.389, 0.357)	
By Relation to Chronic Illness			
log hospitalizations related to chronic illness	-0.036	-0.023	959
	(-0.179, 0.108)	(-0.608, 0.554)	
log hospitalizations related to non-chronic illne	-0.077	-0.010	960
	(-0.201, 0.044)	(-0.173, 0.154)	
Non Blacks			
Log Total Hospitalizations (excluding pregnancy)	-0.098***	-0.081***	960
	(-0.114, -0.082)	(-0.108, -0.053)	
By Relation to Chronic Illness			
log hospitalizations related to chronic illness	-0.084***	-0.069***	960
	(-0.153, -0.015)	(-0.120, -0.018)	
log hospitalizations related to non-chronic illne	-0.116***	-0.102***	960
	(-0.134, -0.097)	(-0.140, -0.064)	

Notes: 2009 hospitalization data are from AR, AZ, CA, CO, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, VT, and WI. Models using all races also include data from NC and NE. In addition to the indicator for cohorts born after the cutoff and its interaction with the size of the discontinuty and state fixed effects, the flexible regression specification also includes state-specific quadratic functions in birth month cohort that are interacted with the indicator for cohorts born after the cutoff. Clustered wild bootstrap 95% confidence intervals are reported in brackets and were used for hypothesis

*** p<0.01, ** p<0.05, * p<0.1

Table 11. Effect of State Childhood Medicaid Eligibility Gain on ED Visits At Age 25 By Race

	Post x Size of Discontinuity Restricted Model	Post x Size of Discontinuity Flexible Model	N
All Races			
Log Total ED Visits in 2009	-0.038	-0.077***	864
	(-0.129, 0.052)	(-0.123, -0.030)	
By Relation to Chronic Illness			
log ED visits related to chronic illness	-0.020	-0.113***	864
	(-0.157, 0.117)	(-0.196, -0.029)	
log ED visits related to non-chronic illness	-0.043	-0.073***	864
	(-0.129, 0.043)	(-0.112, -0.034)	
Blacks			
Log Total ED Visits in 2009	-0.042	-0.037	862
	(-0.115, 0.058)	(-0.169, 0.071)	
By Relation to Chronic Illness			
log ED visits related to chronic illness	-0.025	-0.030	787
	(-0.159, 0.101)	(-0.497, 0.428)	
log ED visits related to non-chronic illness	-0.049	-0.042	862
	(-0.129, 0.032)	(-0.172, 0.089)	
Non Blacks			
Log Total ED Visits in 2009	-0.027	-0.079***	864
	(-0.117, 0.063)	(-0.109, -0.048)	
By Relation to Chronic Illness			
log ED visits related to chronic illness	-0.013	-0.143**	864
	(-0.182, 0.153)	(-0.296, -0.000)	
log ED visits related to non-chronic illness	-0.031	-0.071***	864
	(-0.113, 0.052)	(-0.083, -0.060)	

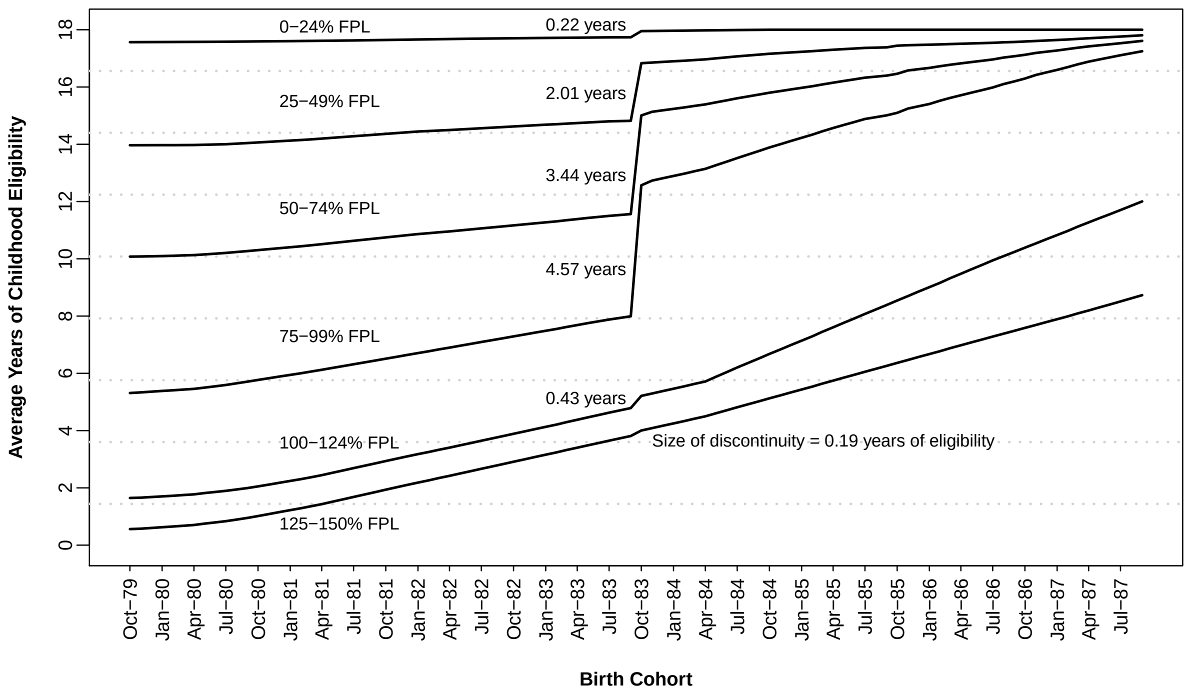
Notes: 2009 ED data are from AZ, CA, HI, IA, KY, NJ, NY, UT, VT and WI. In addition to the indicator for cohorts born after the cutoff and its interaction with the size of the discontinuty and state fixed effects, the flexible regression specification also includes state-specific quadratic functions in birth month cohort that are interacted with the indicator for cohorts born after the cutoff. Clustered wild bootstrap 95% confidence intervals are reported in brackets and were
*** p<0.01, ** p<0.05, * p<0.1

Table 12. Effect of Medicaid on 2009 Logged Total Hospital and ED Costs By Payer for Each Birth Cohort, By Race

	All Races		Blacks		Non-Blacks	
	(1)	(2)	(3)	(4)	(5)	(6)
	Hospital Costs	ED Costs	Hospital Costs	ED Costs	Hospital Costs	ED Costs
Global polynomial model						
4-Year window (*N*= 96)	-0.005	-0.007	-0.078*	-0.045*	0.007	-0.001
	(-0.033, 0.023)	(-0.028, 0.014)	(-0.164, 0.007)	(-0.093, 0.004)	(-0.029, 0.043)	(-0.022, 0.021)
3-Year window (*N* = 72)	0.003	-0.007	-0.077	-0.079***	0.019	0.004
	(-0.031, 0.037)	(-0.032, 0.017)	(-0.180, 0.027)	(-0.133, -0.024)	(-0.021, 0.058)	(-0.020, 0.028)
2-Year window (*N* = 48)	0.020	0.018	-0.130*	-0.073**	0.054**	0.032**
	(-0.021, 0.061)	(-0.011, 0.047)	(-0.270, 0.011)	(-0.139, -0.007)	(0.008, 0.101)	(0.002, 0.062)
Local linear regression						
IK Bandwidth Selector	-0.009	0.006	-0.099***	-0.051**	0.017	0.021*
	(-0.030, 0.013)	(-0.012, 0.023)	(-0.161, -0.036)	(-0.093, -0.009)	(-0.015, 0.048)	(-0.001, 0.042)
CCT Bandwidth Selector	-0.013	0.010	-0.137***	-0.062***	0.019	0.025*
	(-0.048, 0.023)	(-0.013, 0.032)	(-0.200, -0.073)	(-0.104, -0.020)	(-0.020, 0.058)	(-0.001, 0.051)

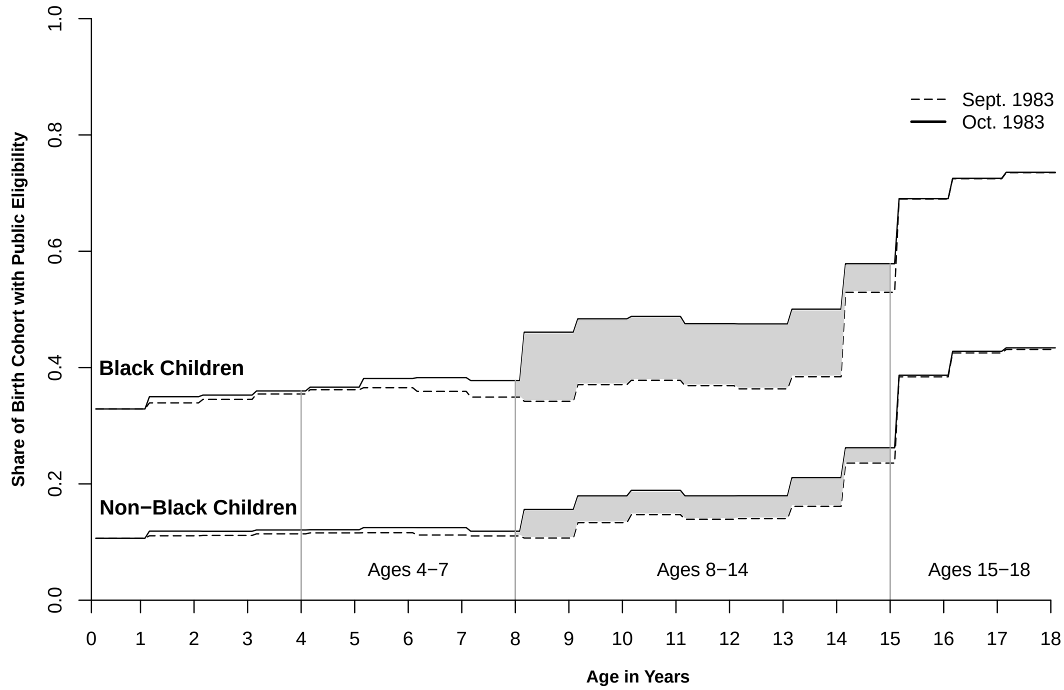
Notes: Sample includes AR, AZ, CA, CO, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, VT and WI. Results for all races also include NC and NE. All global regression models include birth month fixed effects and a quadratic function in birth month cohort interacted with an indicator that the birth month cohort is October 1983 or later. 95% confidence intervals reported in parentheses; *** p<0.01, ** p<0.05, * p<0.10.

Figure 1. Average Years of Childhood Eligibility for Medicaid/SCHIP by Birth Cohort and Family Income (%FPL)



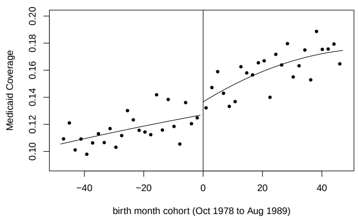
Source: Wherry and Meyer (2014).

Figure 2. Average Public Eligibility at Each Age of Childhood by Birth Month Cohort and Child Race

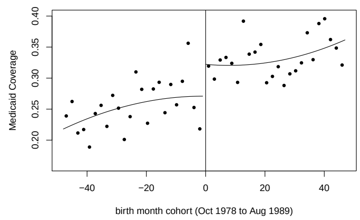


Notes: Weighted average calculated using the characteristics and state of residence of a sample of black or non−black children of ages 0−17 in the 1981−1988 March CPS. See Wherry and Meyer (2014) for additional information.

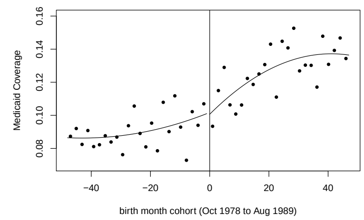
Figure 3: Medicaid Coverage in Childhood, Ages 8 to 13, NHIS



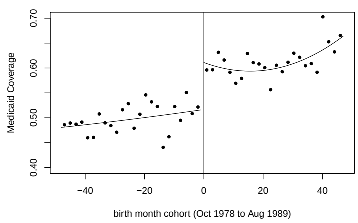
(a) All Races



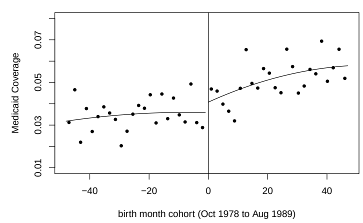
(b) Blacks



(c) Non-Blacks



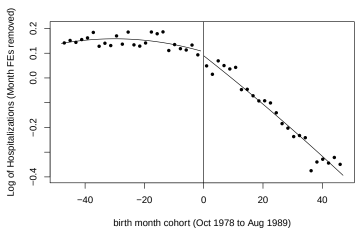
(d) Households below poverty level



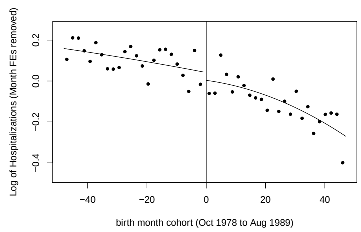
(e) Households above poverty level

Source: Authors' calculations from the National Health Interview Survey, 1992-1996. Cohorts born in 1983 are between the ages of 8 and 13 in these figures. The trend is estimated using children between the ages of 4 and 17.

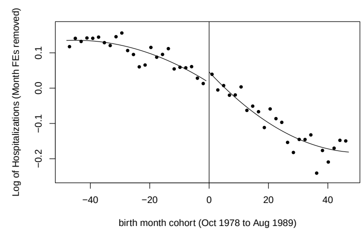
Figure 4: 1999 Hospitalizations, Calendar Month of Birth Fixed Effects Removed



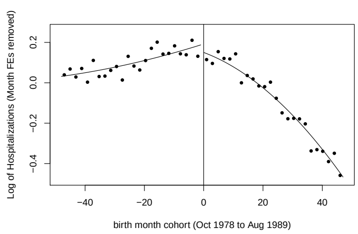
(a) All Hospitalizations, All Races



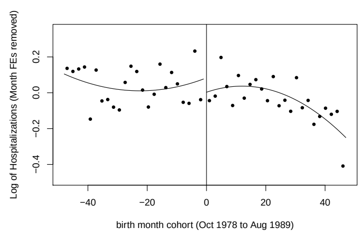
(b) All Hospitalizations, Blacks



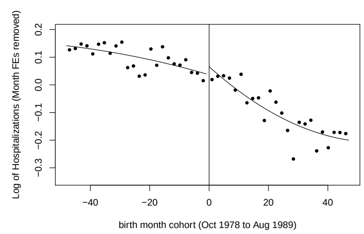
(c) All Hospitalizations, Non-Blacks



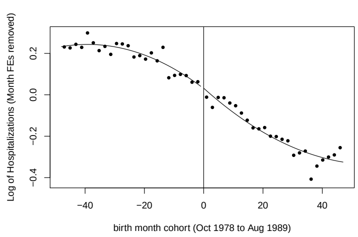
(d) Chronic Illness Hospitalizations, All Races



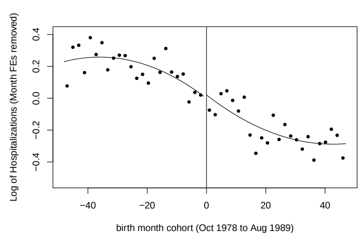
(e) Chronic Illness Hospitalizations, Blacks



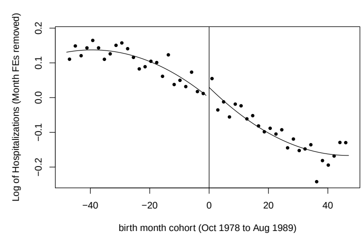
(f) Chronic Illness Hospitalizations, Non-Blacks



(g) Non-chronic Illness Hospitalizations, All Races



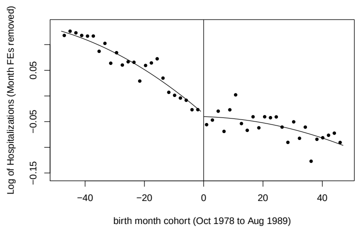
(h) Non-chronic Illness Hospitalizations, Blacks



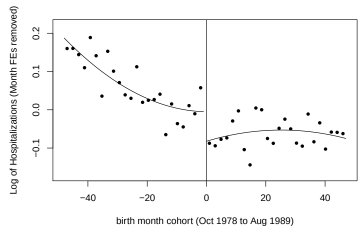
(i) Non-chronic Illness Hospitalizations, Non-Blacks

Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all hospitalizations that occurred in 1999 in AZ, CA, IA, NY, OR, TX, HI, UT, and WI.

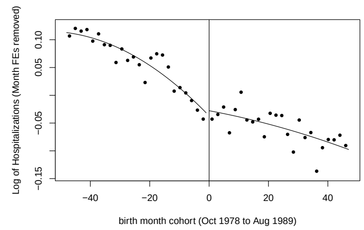
Figure 5: 2009 Hospitalizations, Calendar Month of Birth Fixed Effects Removed



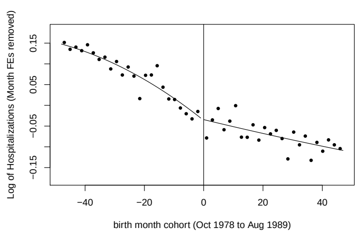
(a) All Hospitalizations, All Races



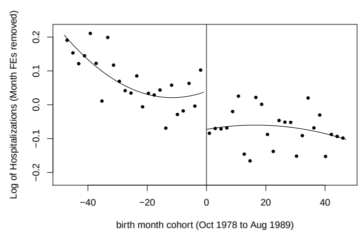
(b) All Hospitalizations, Blacks



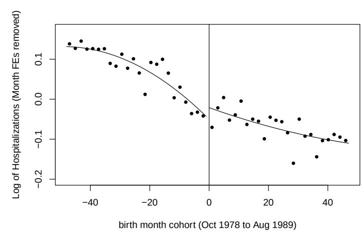
(c) All Hospitalizations, Non-Blacks



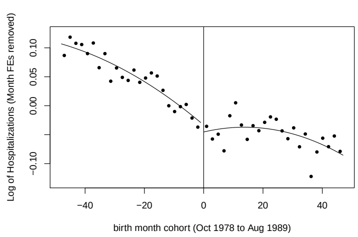
(d) Chronic Illness Hospitalizations, All Races



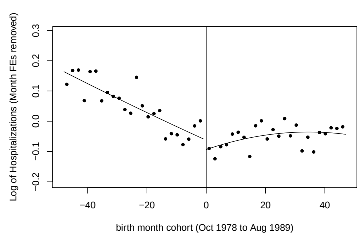
(e) Chronic Illness Hospitalizations, Blacks



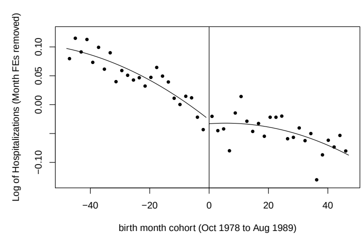
(f) Chronic Illness Hospitalizations, Non-Blacks



(g) Non-chronic Illness Hospitalizations, All Races



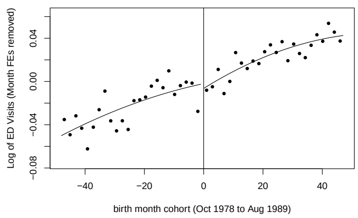
(h) Non-chronic Illness Hospitalizations, Blacks



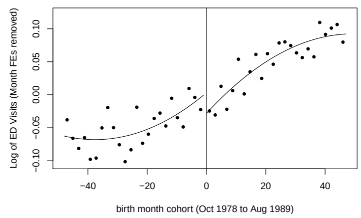
(i) Non-chronic Illness Hospitalizations, Non-Blacks

Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all hospitalizations that occurred in 2009 in AR, AZ, CA, CO, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, and WI. Figures for all races also include NE and NC.

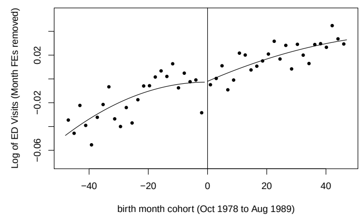
Figure 6: 2009 Emergency Department Visits, Calendar Month of Birth Fixed Effects Removed



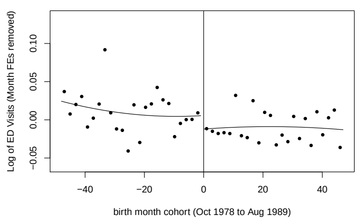
(a) All ED Visits, All Races



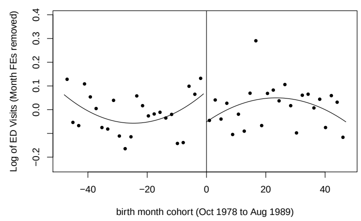
(b) All ED Visits, Blacks



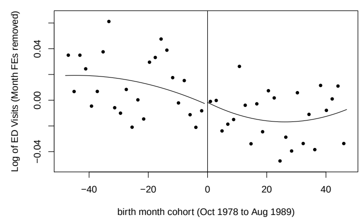
(c) All ED Visits, Non-Blacks



(d) Chronic Illness ED Visits, All Races

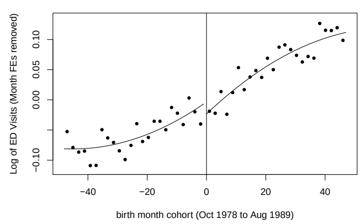


(e) Chronic Illness ED Visits, Blacks

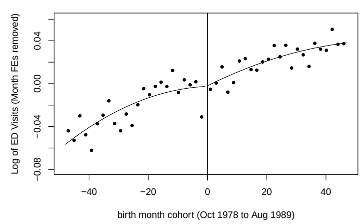


(f) Chronic Illness ED Visits, Non-Blacks

(g) Non-Chronic Illness ED Visits, All Races



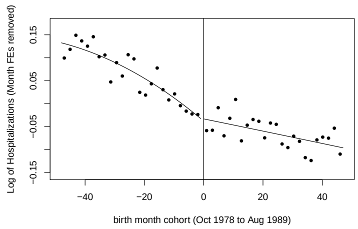
(h) Non-Chronic Illness ED Visits, Blacks



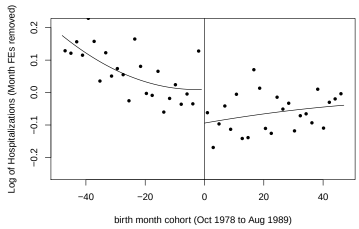
(i) Non-Chronic Illness ED Visits, Non-Blacks

Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all emergency department visits that occurred in 2009 in AZ, CA, HI, IA, KY, NJ, NY, UT, and WI.

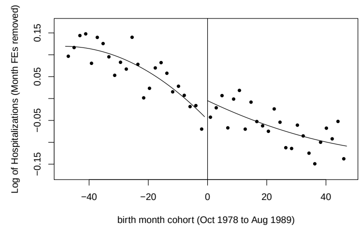
Figure 7: 2009 Hospitalizations, Patients from Low-Income Zipcodes, Calendar Month of Birth Fixed Effects Removed



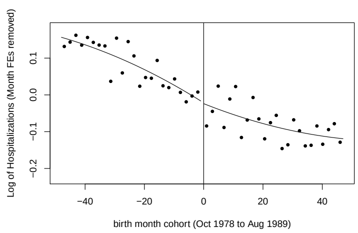
(a) All Hospitalizations, All Races



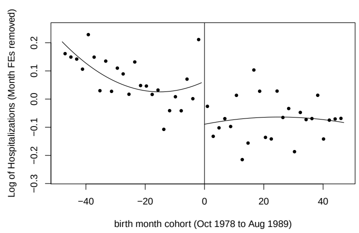
(b) All Hospitalizations, Blacks



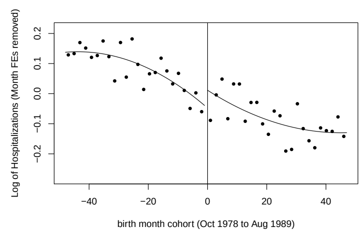
(c) All Hospitalizations, Non-Blacks



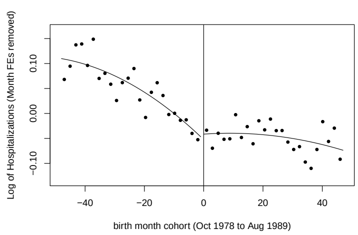
(d) Chronic Illness Hospitalizations, All Races



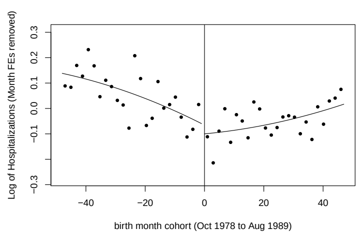
(e) Chronic Illness Hospitalizations in 2009, Blacks



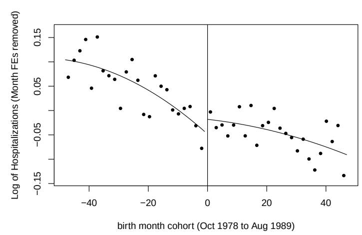
(f) Chronic Illness Hospitalizations in 2009, Non-Blacks



(g) Non-chronic Illness Hospitalizations, All Races



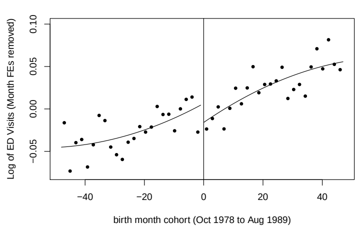
(h) Non-chronic Illness Hospitalizations in 2009, Blacks



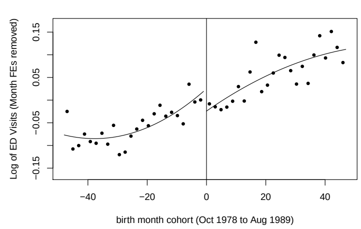
(i) Non-chronic Illness Hospitalizations in 2009, Non-Blacks

Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all hospitalizations of patients from low-income zipcodes (zipcodes with median income lower than $39,999) that occurred in 2009 in AR, AZ, CA, CO, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, and WI. Figures for all races also include NE and NC.

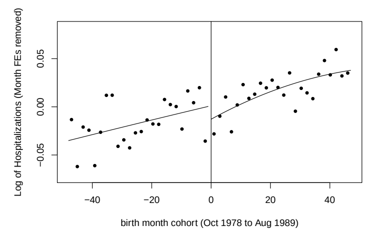
Figure 8: 2009 Emergency Department Visits by Patients from Low-Income Zipcodes, Calendar Month of Birth Fixed Effects Removed



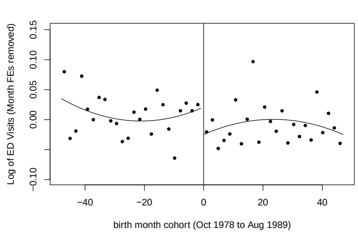
(a) All ED Visits, All Races



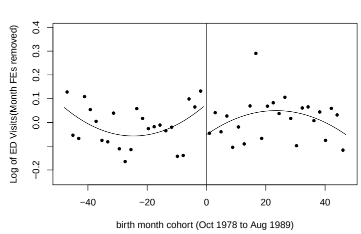
(b) All ED Visits, Blacks



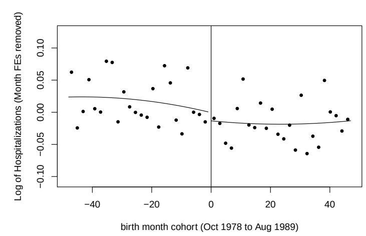
(c) All ED Visits, Non-Blacks



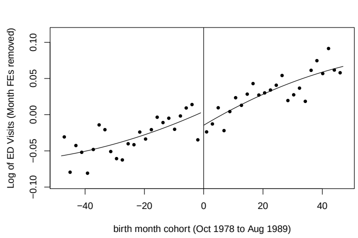
(d) Chronic Illness ED Visits, All Races



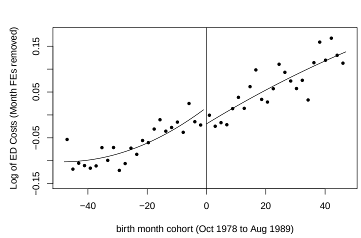
(e) Chronic Illness ED Visits, Blacks



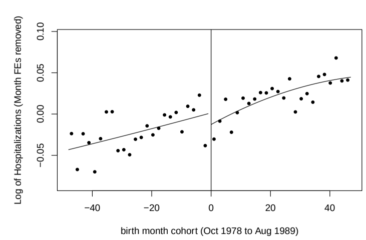
(f) Chronic Illness ED Visits, Non-Blacks



(g) Non-chronic Illness ED Visits, All Races



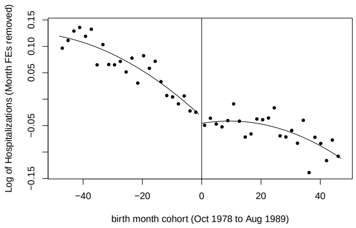
(h) Non-chronic Illness ED Visits, Blacks



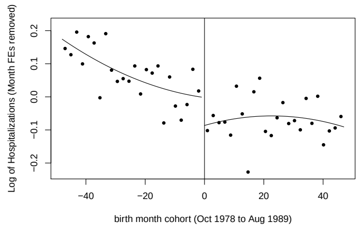
(i) Non-chronic Illness ED Visits, Non-Blacks

Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all emergency department visits by patients from low-income zipcodes (zipcodes with median income below $39,999) that occurred in 2009 in AZ, HI, IA, KY, NJ, NY, UT, and WI.

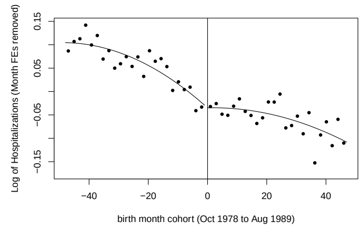
Figure 9: 2009 Hospital Costs, Calendar Month of Birth Fixed Effects Removed



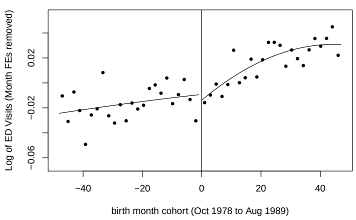
(a) Total Hospital Costs, All Races



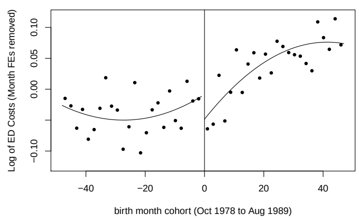
(b) Total Hospital Costs, Blacks



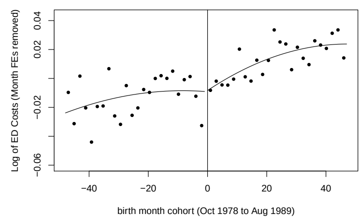
(c) Total ED Costs, Non-Blacks



(d) Total ED Costs, All Races



(e) Total ED Costs, Blacks



(f) Total ED Costs, Non-Blacks

Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all hospitalizations that occurred in 2009 in AR, AZ, CA, CO, HI, IA, MD, MI, NJ, NY, OR, SD, TX, UT, and WI and ED visits from AZ, CA, HI, IA, KY, NJ, NY, UT, and WI. Hospital cost figures for all races also include NE and NC.