THE LONG-TERM EFFECTS OF EXPOSURE TO MEDICAID IN EARLY CHILDHOOD

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CHAPTER 1. INTRODUCTION

I. Motivation and Aims

Extensive research in epidemiology, sociology, and economics suggests that childhood circumstances shape health and economic status throughout life (Almond & Currie, 2011; Conti & Heckman, 2014; Montez & Hayward, 2011; Ben-Shlomo & Kuh, 2006). Such findings suggest that policy interventions that improve the economic conditions and health status of children can have long-term benefits. However, few studies have examined the long-term impacts of the major means-tested programs in the United States and there is a particular lack of evidence on programs that specifically target child health. This project examines Medicaid, one of the largest public programs in the U.S. It is motivated by the question: Does exposure to Medicaid in early childhood improve health and economic outcomes in adulthood?

Medicaid is the largest provider of public health insurance for children and pregnant women in the U.S. The program finances 48% of all deliveries and in combination with the Children's Health Insurance Program (CHIP) provides coverage to 35% (28 million) of all children under the age of 19 (Markus et al. 2013; SHADAC 2013). On average the program consumes 16% of a state's budget and 8% of all federal spending (KFF 2012). In 2012, federal Medicaid and CHIP expenditures on children (measured as outlays) were $75 billion, compared to $61 billion for nutrition programs, $60.1 billion for tax expenditures, and $50.4 for income support (Isaacs et al. 2013). Medicaid impacts the lives of a large segment of the U.S. population and requires a significant investment from taxpayers. An accurate comparison of Medicaid's costs and benefits, which is needed to efficiently allocate public dollars across different investments, should include both contemporaneous and longer term effects. This research begins to look at the long-term effects of Medicaid in childhood on later life outcomes.

I present a conceptual model that suggests that Medicaid exerts a positive influence on long term health and economic outcomes (see Chapter 2). In short, Medicaid reduces the price of medical care, which is predicted to increase utilization of medical care by expectant mothers and children. In turn, utilizing effective health services improves childhood health in ways that persist over time, resulting in positive health and economic outcomes in adulthood. I also propose that Medicaid improves the financial security of families by paying for care that a family may have otherwise purchased at higher prices on the private market. The increased economic resources indirectly provided by Medicaid could be spent on productive child investments.

While there are clear conceptual reasons for hypothesizing a positive association between Medicaid and long-term outcomes, isolating the relationship empirically is not straightforward. Medicaid is a voluntary program and there are unobservable factors that influence an eligible family's choice to enroll their child. Omitted variables include parenting style, the child's pre-existing health status, and other factors that are known to be related to child development. A direct comparison of participants versus non-participants will reflect both the impact of Medicaid and the effect of omitted variables.

To avoid bias arising from the unobserved determinants of participation, I measure the impact of exposure to Medicaid policy rather than the impact of participation, *per se*. Medicaid policy varies across time and state as a function of historically determined local appetites for public spending on the poor and sporadic federally driven eligibility expansions. Importantly, policy is not determined by individual families and examining exposure to policy in an intent-to-treat framework is less susceptible to omitted variable bias.

Identifying Medicaid's long-term impact is further complicated by the need for a long follow-up period, which constrains the universe of potential policy changes that can be used as a natural experiment. This project takes advantage of the staggered introduction of Medicaid across the states. Medicaid was passed into law in 1965 and was implemented in the states at different times, mainly between 1966 and 1970. Medicaid's introduction created variation in early life exposure to Medicaid for birth cohorts that are now well into adulthood. The birth cohort born in 1972, when Medicaid was implemented in every state but Arizona, is now 42 years old (2014). Relying on the timing of Medicaid's introduction is also advantageous because, at inception, it targeted an extremely disadvantaged segment of the population that had few insurance alternatives. This will improve my chances of finding an effect because those taking-up coverage were likely uninsured prior to the program. For families targeted by the program, Medicaid likely represented a real change in access to care.

Using quasi-experimental methods I examine the causal chain that links early life exposure to Medicaid to adult health and economic status. The specific aims are to:

1. Estimate the impact of Medicaid's introduction on the utilization of health care by children and women of childbearing age.

2. Estimate the impact of Medicaid's introduction on health at birth.
3. Measure the long-term impact of exposure to Medicaid in childhood on health and socioeconomic outcomes during adulthood.

I examine each aim with a separate dataset because no single source covers the timeframe of Medicaid's introduction and includes health care utilization, childhood health and economic well-being, and long-term follow-up. I address Aim 1 using restricted use data from the 1963-1980 National Health Interview Survey. Aim 2 is examined with the 1964-1969 and 1972 National Natality Surveys and Aim 3 with the Panel Study of Income Dynamics. As will be discussed in more detail in Chapter 3, the timing of Medicaid's introduction across the states was not perfectly random. I use different strategies to account for secular trends that were correlated with the timing of Medicaid's introduction. To that end, all three analyses will be complemented by a rich set of variables that describe local public policy and health care supply as it evolved over time.

II. Chapter Summaries

Chapter 2 develops a conceptual model drawing on literatures from economics, sociology, and epidemiology. The conceptual model considers a dynamic childhood development process in which adult outcomes are determined by interacting factors that influence childhood wellbeing. These factors arise from individual biology, family circumstances, and broader social environments. It is within this dynamic environment of childhood development that I suggest that exposure to Medicaid in early life can have important long-term benefits.

Chapter 3 provides a brief history of the introduction of Medicaid. I describe the provision of health care to the poor prior to Medicaid, the political process that lead to Medicaid's enactment and its implementation in the states. I also discuss program dynamics, such as take-up and crowd-out, which have drawn substantial attention from policy makers and researchers in recent years, but differed in important ways at Medicaid's inception.

In Chapter 4, I estimate the effect of Medicaid's introduction on the utilization of health care among children and mothers of childbearing age. The chapter begins with a brief review of literature concerning the impact of Medicaid on utilization and a discussion of why Medicaid's introduction may have differed from previously studied changes in Medicaid policy. I then describe the estimation data, the National Health Interview Survey, and the empirical strategy. My preferred estimates suggest that Medicaid increased the probability of any annual over-night hospital stay by roughly 3 percentage points for young, low-income children. These findings support the conceptual model which suggests that Medicaid increases health service use.

Chapter 5 considers whether Medicaid's introduction had a positive impact on child health. I operationalize child health using birth weight. Birth weight is an imperfect measure of infant health, but a large literature shows that birth weight is correlated with health and economic attainments in adulthood. Data comes from the 1964-1969 and 1972 National Natality Surveys. My results suggest that introduction of Medicaid reduced the incidence of low birth weight by 4 percentage points in the low-income population. The data suggest that Medicaid's introduction substantially improved health in early life.

In Chapter 6, I present reduced form estimates of the long-term effects of exposure to Medicaid in childhood using data from the Panel Study of Income Dynamics (PSID). The PSID has been tracking families and their descendants since 1968 and it is uniquely suited to studying the role of early life exposures on long term outcomes. My results suggest that exposure to Medicaid in childhood improves adult health, measured using a composite index of chronic conditions that includes information on high blood pressure, heart disease and heart attacks, adult on-set diabetes and obesity. I find no statistical evidence that exposure to Medicaid improves long term economic outcomes. However, my estimates of Medicaid's economic impact are imprecise and my findings are inconclusive.

The volume concludes in Chapter 7 with a discussion of limitations, policy implications, and directions for future research. Evidence from this project suggests that providing public health insurance to low-income children can have long-term benefits. This finding suggests that current valuations of Medicaid and other health insurance expansions, that consider only short or medium term effects, may undervalue public investments in health insurance for the poor.

CHAPTER 2. CONCEPTUAL MODEL AND LITERATURE REVIEW

This chapter develops a conceptual model that links early life exposure to Medicaid with adult outcomes. The model is motivated by a review of empirical evidence on the childhood determinants of adult health and socioeconomic status (SES). The chapter begins by describing a theoretical framework of child development that will organize empirical findings. I then discuss the role of Medicaid in the context of child development and lay out basic hypotheses. Section 2 reviews empirical evidence on the impact of Medicaid and then discusses previous literature that suggests that early life health and economic circumstances have profound consequences on later life. The review pays particular attention to two childhood factors that are plausibly sensitive to Medicaid: parental income and early childhood health.

I. A Theoretical Framework of Child Development and the Implications of Medicaid

Over the last 10 to 15 years there has been growing interest in the social sciences on the importance of the early childhood period (age 0-5) in shaping well-being across the life-course. This work is influenced by research in biology, neuroscience and psychology which suggest that human development is a dynamic process that starts at conception and progresses through a sequence of sensitive periods. Negative shocks that disrupt development during the critical stages of early life can cause permanent damage to health, cognitive, and socio-emotional functioning. Conversely, investments made in young children have long-run returns.

Capacity Formation

Traditionally, economists have modeled adult socioeconomic attainments as a function of random chance (i.e. "luck") and skills that have rewards in the labor market and other social domains. These skills, typically called human capital, include a broad set of abilities and traits such as occupation specific skills, cognitive skills, social-psychological characteristics (motivation, perseverance, etc.), and health. Indeed, such characteristics are strongly correlated with measures of adult wellbeing (Heckman, 2006). Human capital is thought be acquired through endowments (i.e. genetic transmission) and through investments such as schooling and work experience. Traditional models of human capital conceptualize childhood as a single period in which skills are accumulated with public, parental, and individual investments (Becker and Tomes, 1986). Using a similar framework, Grossman's (1972) pioneering work on the production of health models health as a stock in which the level of the stock and any investments in an early period depreciate over time.

Both Becker and Tomes (1986) and Grossman (1972) have been highly influential in the economics literature, but neither fully accounts for observed patterns of human development. Research from neuroscience and developmental psychology suggest that childhood does not consist of a single stage, but a hierarchically ordered series of sensitive and critical periods of development (for reviews see Heckman, 2007; Heckman 2006; and Knudsen et al. 2006). Critical periods are unique in their ability to produce a given capacity like cognitive skill and sensitive periods are more productive, but not exclusively capable of producing a capacity. The plasticity of specific neural circuits helps explain a period’s relative sensitivity (Knudsen et al. 2006). For example, language is more readily acquired prior to age 12 suggesting that from birth to age 12 is sensitive period for language development. IQ appears to change prior to age 10, but is completely stable thereafter, suggesting that ages 0-9 is critical stage for IQ. In contrast, non-cognitive skills appear malleable up to the age of 20 (Heckman, 2007). There is consistent evidence from observations in both human and animal samples that experiences in the earliest years of life permanently shape the accumulation of skills across the life course, even when negative experiences are followed by remediation efforts at later ages (Knudsen et al; Heckman 2007; Currie &Almond, 2011).

Literature in neuropsychology and epigenetics also suggests a dynamic and synergistic relationship between “endowed” genetic characteristics and environmental exposures. Endowed characteristics, determined by genetic lottery, matter, but separate additive components for endowment (i.e. nature) and environment (i.e. nurture) are not a sufficient reflection of a developmental process in which genetic expression is partially shaped by environmental inputs (Heckman, 2007). That is, research from epigenetics suggests that the environment can turn on or off a given gene. Physical and social environments “get under the skin”.

Similarly, the impacts of exposures during a given period (whether they be harmful shocks or productive investments) depend on the set of capacities that are brought into that period. For example, attention and socio-emotional skills that are developed prior to formal schooling shape the productivity of the educational experience (Duncan et al, 2007).

A separate but analogous set of ideas in epidemiology suggest that early life is a sensitive period of development that shapes life-long disease risk. Epidemiologists have long recognized the potential for early life events to have lasting health implications. In 1934, Kermack observed that cohort mortality patterns are conditioned by childhood exposures (Kermack et al., 1934). A key explanation of this finding is the “fetal origins hypothesis” credited to David Barker (Barker, 1997). The fetal-origins hypothesis suggests that environmental exposures, such as sub-optimal nutrition or maternal disease, program the fetus with health risks that are expressed in later life. Such risk can remain latent until the adult period, suggesting that health shocks don’t necessarily depreciate with age as suggested by Grossman (1972). These programed traits are adaptive responses to the fetal environment, but are associated with dysfunction among adults. For example, fetal malnutrition is thought to lead to altered glucose metabolism set points which create reduced glucose tolerance in adulthood and increased incidence of diabetes. Similar processes are thought to affect a range of physiological systems. There is also evidence that disease exposures later in the childhood life cycle, particularly infectious diseases, illicit inflammation responses that increase disease risk across the life course (see reviews in Currie & Almond, 2010 and Montez & Hayward, 2010).

Capacity formation is a relatively new economic model that attempts to account for this dynamic picture of health and human capital development (Cunha & Heckman, 2006; Heckman, 2007; Conti & Heckman, 2014). An adult outcome (Y) at time t which measures some relevant dimension of adult well-being (e.g. income) is conceptualized as the output of a set of capacities possessed at time *t* and the effort expended on that outcome (*e):*

$$Y_t = f(\theta_t^C, \theta_t^N, \theta_t^H, e_t)\ , \tag{2.1}$$

where the relevant capacities have been arbitrarily separated into cognitive (θ_t^C), non-cognitive (θ_t^N), and health (θ_t^H). Capacity formation is concerned with the production of capacities. The relevant production technology will capture the developmental process described above. Specifically, it will allow for critical and sensitive stages of development, self-reinforcing capacities (i.e. higher skills in any domain in one period can result in higher skills for any domain in the next period), and dynamic complementarity (i.e. the degree to which investments at a later period are either substitutes or complements to investments made in earlier periods). A constant elasticity of supply (CES) technology satisfies these requirements.[1] Consider an example looking at three periods of life, two childhood periods (t=1 and t=2) and one adult period (t=3). θ_1 are initial conditions, and I_t are private or public investments. Let θ_t be a vector encompassing several skill domains (cognitive, non-cognitive, health, etc.).

$$\theta_3 = f(\theta_1, [\gamma(I_1)^\phi + (1-\gamma)(I_2)^\phi]^{\frac{1}{\phi}}),\ for\ \phi \leq 1\ and\ 0\ \geq \gamma \leq 1. \tag{2.2}$$

In Eq 2.2, ϕ measures the relative complementarity or substitutability of period 1 and period 2 investments. When ϕ is 1 the investments are perfect substitutes, suggesting that forgone investments in the first period can be replaced by later investments and yield

[1] Detailed description of this production technology is given in several other places (e.g. Cunha and Heckman, 2007). Here I describe basic principles.

the same result. When ϕ reaches its lower bound, investments are perfect complements, suggesting both that period 2 investments alone cannot remediate an absence of period 1 investments and that for a given level of period 3 capacities, period 1 investments must be followed by period 2 investment. For example, the impact of an early child education intervention is largest when followed by additional investments at later ages of childhood. The γ term is called a capacity multiplier and measures the direct impact of investments in period 1 on skills in period 3 and the indirect effect of period 1 investments through improving skills in period 2.

The Sociology of Health and SES across the Life-Course

While conceptualizing child development as a dynamic process consisting of multiple periods with interacting investments is a relatively recent development in economics, it has a longer history in sociology. The most recent iteration of conceptual thinking about child development, especially as it applies to health over the life-course, blends together life course theory and theoretical explanations of the SES-health gradient. What is most striking is the similarity of perspectives used in sociology with capacity formation perspectives favored by economists. Indeed, many scholars present their theoretical reasoning using concepts from both perspectives (e.g. Ermisch et al., 2012).

Life-course theory seeks to explain the distribution of adult characteristics as the culmination of trajectories that are shaped on several social and individual margins (Elder, 1998; O'Rand, 1996). The life-course of an individual is embedded in a historical time and place and within a social network. The developmental impact of a life event or transition (e.g. marriage, job entry, child birth, etc.) is conditioned by the timing of the exposure. For example, child birth is significantly different for a teen-ager than a 30 year old. The life-course of an individual is characterized by a high degree of human agency that operates within the constraints of social circumstance. People make choices, such as the level of investment in their children, but the availability of opportunities and actual decisions operate within a social context.

A key concept in the life course tradition is cumulative advantage (O'Rand, 1996). Cumulative advantage suggests that early advantages compound over time resulting in widening gaps across the life course. For example, in cumulative advantage's earliest iteration Merton (1968) proposed that early success in academic professions attracts scarce resources which can be invested in the next period's productivity. Scholars with less relative success in the first period do not attract resources for period 2 and thus begin to fall further and further behind. Cumulative advantage has a clear analog in the complementarities suggested by capacity formation.

A second set of relevant contributions in sociology comes from the study of health and socioeconomic status. The socioeconomic gradient in health is one of the most well documented social phenomena (Link et al., 1995; Mirowsky & Ross, 2003; Marmot, 2001; Haas, 2006). Those with less education, lower-incomes, or less prestigious occupations are more likely to have worse self-reported health status, have higher risks of disease, disability, and mortality. The most prominent theoretical explanation for this relationship is social causation (Haas et al., 2011; Haas, 2006). Sociologists posit that the poor health of those in lower socioeconomic strata is caused by decreased access to health care services, greater exposure to stressors and hazards, and reduced social support and social capital. Mirosky and Ross (2003) propose the SES gradient is primary driven by educational attainment which improves health by promoting individual agency (Mirowsky & Ross, 2003). Link and Phelan (1995) argue that SES is a fundamental cause of health that operates through a complex set of causal mediators that is shaped by a given historical circumstance.

The countervailing position is that the socioeconomic gradient in health is an artifact of health selection—health causes socioeconomic outcomes. One set of hypothesis posits a process of social drift in which those in poor health are contemporaneously selected into lower socio-economic strata due to decreased labor market participation and increased expenditures on health care services (Haas, 2006).

The second type of health selection, termed social stunting by Stephen Haas, borrows insights from life-course theory to explain the health-SES gradient. Social stunting frameworks hold that health, especially during critical periods of development, disrupts human capital accumulation and prevents attainment of positions of power and prestige. Those in poor childhood health attain less human capital and receive a smaller return for a unit of human capital. This is akin to a cumulative advantage/ disadvantage process which posits that "advantage begets advantage" (O'Rand, 1996) .

The social stunting hypothesis also suggests a recursive relationship between health and SES that plays out across an individual life-course and between generations. Haas (2006) comments, "For example, poor childhood health, itself a product of socioeconomic dis-advantage, may lead to lower educational attainment and skill formation, diminished labor market outcomes, lower earned income and wealth accumulation, and fewer resources to invest in the next period's health. In this way, there is a constant interaction between health and SES via both selection and social causation over the life course." This sentiment is echoed by the demographer Alberto Palloni (2006) who argues that poor health in childhood is a function of parental social class and that the health gradient tends to expand with age. In this way, he argues, health is an important causal mechanism in the heritability of social class.

The role of Medicaid in the development of health and economic attainment

The frameworks described above suggest that adult-wellbeing, measured in both in economic and health terms, depends on a diverse set of capacities. These capacities are influenced by endowments and investments which are shaped by the social environment. The timing, sequence, and content of investments matter. In particular, the earliest ages of life are particularly sensitive to investments and shocks, because they are characterized by a high degree of developmental plasticity. In early life, physiological processes that help determine lifelong morbidity are especially sensitive to disease exposure, nutritional deprivation, and other environmental influences than can set off a cascade of "advantage-begets-advantage".

This project considers Medicaid as an investment in young children. The goal of the Medicaid program (described in more detail in Chapter 3) is to provide health insurance coverage to low-income populations. Medicaid reduces the price of medical care, which is predicted to increase utilization of care by expectant mothers and children.

In turn, utilizing effective health services improves childhood health in ways that persist over time. Relevant care includes the provision of prenatal services which could improve health *in utero*, early childhood preventative services such as vaccinations, or the timely treatment of common childhood conditions. Utilization of care by mothers that not is directly related to pregnancy could also indirectly improve the health of children by promoting healthy pregnancies (Badura et al. 2008). Improved health in childhood is predicted to increase health and economic attainments in the adult period.

Medicaid could also improve the financial security of families by paying for care that a family may have otherwise purchased at higher prices on the private market. The increased economic resources indirectly provided by Medicaid could be diverted to productive child investments that require financial funds and time. As will be discussed later, Medicaid may decrease the labor supply of parents. The literature on the effects of parental employment on child outcomes is mixed and the strongest evidence suggests heterogeneous effects across the lifecycle of the child. There is a negative correlation between maternal employment and outcomes in the earliest years of life and null effects during later years of childhood (Ruhm, 2002; Waldogel et al. 2002).

These hypotheses are summarized by the conceptual model described in Figure 2.1. As will be discussed in Chapter 3, the availability of Medicaid is determined by state and federal governments. Other community characteristics, such as other public investments in children or the structure of a state’s health care market, are correlated with Medicaid’s availability. Individual families decide whether or not to participate as function of expected benefits and the costs of enrollment. The benefits of participation flow through child health and economic resource pathways as described above. The figure also shows that participation is correlated with factors such as parenting style that help determine outcomes.

II. Previous Empirical Research on Medicaid and the Early Life Determinants of Adult Success

Figure 2.2 presents a simplified conceptual model of the link between Medicaid and adult outcomes: all confounding variables are denoted with *u;* the dashed lines indicate that not all confounding variables are observed in available data. On the left hand side of the figure I have organized the conceptual model linking Medicaid to outcomes into two stages. The first stage illustrates Medicaid's impact on financial resources, utilization, and health. The second stage describes the impact of childhood health and economic resources on adult outcomes. The following literature review is organized around these stages.

Stage 1: The Short and Medium Term Impacts of Medicaid[2]

Traditionally, the Medicaid program has provided health care benefits to four groups of people: low-income mothers, low-income children, low-income non-elderly disabled, and low-income elderly (Gruber, 2003). This project focuses on Medicaid for low-income mothers and children. Chapter 3 provides a brief history of policy development in the Medicaid program, but a description here, of basic program parameters, will be helpful in interpreting existing evidence on Medicaid's impact. Medicaid is jointly funded and administered by the states and federal governments. While the states must meet mandatory minimum benefit and eligibility floors, they have considerable latitude in designing their programs. Benefits are provided through direct

[2] A previous version of this section appeared in Boudreaux & McAlpine (2013) and is printed here with permissions from ABC-CLIO, Inc.

vendor payments (i.e. physicians and hospitals are paid directly) and provider fees are have traditionally been lower than in the private market or in Medicare. The review below primarily draws on evidence from Medicaid expansions that started in the early 1980's and leaves discussion of evidence from Medicaid's introduction for Chapter 3.

The most applicable empirical evidence supporting the idea that Medicaid coverage in early childhood is associated with downstream benefits comes from a set of papers that have shown that Medicaid eligibility in early childhood is associated with improved health in later childhood. Currie, Decker, and Lin (2008) show that Medicaid eligibility at ages 2-4 is associated with improved self-reported health at ages 9-17. Likewise, using a regression discontinuity approach, Meyer and Wherry (2012) show that increased cumulative eligibility between the ages of 8 to 14 is associated with a 13-20 percent decline in mortality for Black children at ages 15-19. They find smaller effects for White children who were substantially less likely to be eligible for Medicaid. There is also some evidence that public health insurance in early childhood increases academic performance. Levine and Schazenbach (2009) find that eligibility for public health insurance at birth is associated with modest gains in reading, but not math scores for 9 and 14 year olds. The only other study to examine the mid-term effects of Medicaid coverage in childhood found that increased eligibility for Medicaid at ages 5-18 is associated with improved contemporaneous utilization of preventative care, but a 5 year lagged measure of eligibility was not associated with improvement in self-reported health, missed school days, or obesity (De La Mata 2012).

There is scant evidence on the long term impacts of health insurance coverage generally, regardless of its source. A few papers have considered long term mortality outcomes from exposure to health insurance in adulthood. These studies generally find small to null long-term mortality effects from insurance during adulthood (Kronick, 2009; Finkelstein & McKnight, 2008; Card et al., 2009). Two more recent papers (Sommers et al. 2012; Sommers et al. 2014) have found that Medicaid expansions to non-elderly adults and the 2006 Massachusetts health reform led to reduced mortality over a 5-year follow-up period.

The small, but important set of papers on Medicaid's medium-term effects suggest that the value of Medicaid may extend beyond contemporaneous measures of health and financial well-being. My conceptual model suggests that longer-acting processes work through shorter term effects that persist over time. Below I examine existing evidence on these short term effects, particularly, Medicaid's impact on health insurance coverage, utilization of care, and health outcomes. However, I also discuss broader features of Medicaid program dynamics that have received substantial attention in the literature and shape both the costs and benefits of the program.

The chief goal of the Medicaid program is to increase the level of health insurance coverage in selected segments of the low-income population. Judged on that criterion, the program has been moderately successful. During large expansions of the program that occurred between 1987 and 2010, the rate of public coverage in the non-elderly population rose from 13.6% to 21.4%. Twenty-one of 26 studies reviewed by Howell and Kenny (2012) found that Medicaid expansions significantly decreased uninsurance.

The median effect in these studies was a 7.3 percentage point decline in uninsurance. Despite these gains, the uninsurance rate expanded from 14.4% in 1987 to 21.4% in 2010 (SHADAC, 2013). This trend has largely been driven by a decline in private coverage.

There are two processes that determine how successful Medicaid has been in reducing uninsurance. The first is the extent to which those eligible for Medicaid enroll in the program, a phenomenon termed “take-up”. The second is the degree to which those that take-up Medicaid coverage were previously uninsured. “Crowd-out” is a term used to describe the situation where those enrolling in Medicaid were previously covered in the private market. While switching from private to public coverage may have beneficial effects for enrollees (Leininger, 2010), it will have no net effect on the health insurance coverage rate.

Like all voluntary programs, the participation of eligible beneficiaries in Medicaid is less than perfect. Since the 1980’s, the take-up rate in Medicaid has generally been low and tends to be negatively correlated with the generosity of eligibility standards (Gruber, 2003). At the inception of the program, states that had broad medically indigent programs enrolled smaller portions of the eligible population (Stevens and Stevens, 1974). For example, at inception, New York had a large medically indigent program such that 45% of the population was eligible for the program. However, much like states that had stringent eligibility standards, only 11% of the state’s population enrolled (Stevens & Stevens, 1974).

Rigorous studies of take-up did not begin in earnest until the 1990s. Using the Current Population Survey, Gruber (2003) finds that take-up was nearly perfect during the retrenchment experience of the early 1980s when the program was restricted to the most disadvantaged families. The percent of children (age 0-15) eligible for the program increased from 13% in 1983 to 29% in 1996 as eligibility standards were liberalized (Gruber, 2003). However, the take-up rate fell as eligibility grew: in 1983 take-up was nearly 100%, but by 1996 it was 73% (Gruber, 2003). Nearly half of the 10 million uninsured children in 1996 were eligible for public insurance (Aizer, 2007). More recent work finds take-up among children to be over 80%. However, a large segment of uninsured children remain eligible for Medicaid: 65% of the 7.3 million uninsured children in 2008 (Kenney et al., 2010).

Four hypotheses are generally offered to explain the modest take-up rate in Medicaid: 1) lack of information among the eligible; 2) burdensome application procedures; 3) social stigma associated with enrollment; and 4) poor retention (Aizer, 2007; Sommers, 2007). Policy makers and philanthropic groups have responded to these concerns by implementing more aggressive outreach campaigns and reducing the complexity of the application process. Aizer (2007) finds that an outreach campaign in California consisting of television advertisement and provision of community based application assistants had a meaningful effect on enrollment. Similar work by Ziegenfus (2008) found that a national media campaign by the Robert Wood Johnson Foundation was moderately successful in stimulating enrollment among the eligible. “Express Lane Eligibility” is a new approach that uses administrative records to identify and enroll beneficiaries in other programs. Early evaluations have shown this approach to be successful in increasing the use of Medicaid services among the eligible (Dorn, Hill, & Adams, 2012).

The second issue affecting the impact of Medicaid on insurance status is crowd-out—the tendency for the public provision of insurance to erode private coverage. If persons joining Medicaid are primarily those who were previously covered by private insurance, Medicaid will struggle to accomplish one of its key goals—to increase insurance coverage among the poor. Crowd-out implies that Medicaid substitutes for another form of coverage rather than providing coverage where no other alternative exists. The public cost of covering the uninsured through the Medicaid program is much higher with high levels of crowd-out because for every uninsured individual that gains coverage there are also a number of people who move from private to public financing.

The literature has yielded mixed results on the size of crowd-out. Results depend on the data set used, the specification of the empirical model, and the population of interest (Card and Shore-Sheppard, 2004; Cutler & Gruber, 1996; Dubbay & Kenney, 1996; Gruber & Simon, 2008; LoSasso & Buchmueller, 2004). The crowd-out measures in these studies generally reflect the reduction of private coverage as a percent of the increase in public coverage. Estimates range from null results to 60% (Gruber & Simon, 2008). Large crowd-out estimates imply that Medicaid must spend large amounts of money for every uninsured person it covers.

The basic economic justification for crowd-out is intuitive: faced with a lower-cost option, private plan holders may switch to the cheaper public option if countervailing forces such as social stigma or the uncertainty of future public plan availability are not strong enough to offset the price effect (Gruber & Simon, 2008). However, crowd-out could also occur if employers decide to stop offering health insurance when Medicaid is a viable option for their workers. Cutler and Gruber (1995) find that their 50% crowd-out estimate was mainly driven by employees dropping dependent coverage rather than employers dropping health insurance as a fringe benefit.

Large crowd-out estimates concern policy makers. If the expansion of public programs mainly results in people switching from private to public, then Medicaid is not a cost-effective way to reduce the uninsurance rate. Starting in the late 1990's several anti-crowd-out policies were adopted including waiting times and means-tested premiums and copayments. Evaluations of these policies have also found mixed results. Some studies find they are effective while others find they reduce the take-up of Medicaid among the previously uninsured in addition to take-up among the privately insured (Gruber & Simon, 2008).

A topic that has received far less attention in the literature is the extent to which those that switch from private plans to public plans are better off. Those that switch may have more income to spend on other goods and services such as housing, education, transportation, or retirement and would incur less debt. In fact, if the economic justification for crowd-out holds, then people who move from private to public health insurance coverage should switch precisely because it allows them to retain coverage while increasing their consumption in other areas. On paper Medicaid offers a much more generous plan than many private options. As a result, Medicaid might increase the health of the previously insured by increasing access to effective medical treatments. However, the value of Medicaid vis-a-vis private plans is reduced because the low participation of providers in the Medicaid program makes accessing medical services more difficult even though the services are fully covered. While crowd-out generally does result in higher costs for covering the same number of previously uninsured, it could support other policy objectives such as increasing access to care, reducing the financial risk of illness, or encouraging private spending in other areas that society finds valuable. While it is clearly inefficient to deliver an income transfer via a health insurance scheme, concluding that crowd-out is a pure “bad” is likely not an accurate reflection of economic welfare.

Schaefer et al. (2008) estimates that families switching from private to public insurance receive a cash equivalent transfer of $1,500/yr. However, evidence on how these transfers are spent is thin and mixed. Leininger et al. (2010) finds that CHIP expansions led to a greater savings rate and increased expenditures on transportation. Gruber and Yellowitz (1999) find that Medicaid reduced private savings. Saloner (2013) finds that public health insurance does not reduce food insecurity. In 2008 a group of very low-income childless adults in Oregon were given the chance to apply for Medicaid through a random lottery. Evaluations of this random lottery assignment have shown that after 1 year, participants in the treatment group were 35% less likely to have any out-of-pocket medical expenditures and 25% less likely to have unpaid medical debt compared than their counterparts who lost in the lottery. Another recent working paper on the 2006 Massachusetts health reform finds that the expansion reduced total debt and bankruptcies (Mazumder & Miller, 2014).

Despite modest take-up and non-trivial crowd-out, the balance of the evidence suggests that Medicaid has increased health insurance coverage among those who would otherwise be uninsured (Howell & Kenney, 2012). However, the link between Medicaid coverage and the utilization of acute and preventative medical services is not straightforward. Several barriers may impede the utilization of health services by Medicaid enrollees. Lack of providers willing to accept Medicaid patients due to lower fees, insufficient transportation, or lack of beneficiary understanding of program benefits are a few factors that might limit utilization of medical services.

There are a number of challenges to empirically investigating the impact of Medicaid on access to care and health. Medicaid enrollees are different than the uninsured or those with private coverage, making the identification of appropriate control groups difficult. A natural starting point would be to consider the eligible-but-not-enrolled population as the appropriate comparison. However, enrollees could have chosen to become enrolled in Medicaid because they were in poor health. This selection problem could make it appear as if Medicaid reduces health status or increases utilization when it does not, especially in cross-sectional data where the researcher does not know the health status of a subject prior to their enrollment (or non-enrollment) in Medicaid. To overcome these obstacles researchers have relied on a number of approaches that take advantage of state-by-year variation in Medicaid eligibility rules. Variation in eligibility is attractive because it is plausibly unrelated to any one person's attitude or behaviors. Often these studies rely on an empirical method known as intent-to-treat analysis. In studies such as these researchers are interested in the effect of Medicaid eligibility, which is distinct from Medicaid enrollment. The benefit of focusing on eligibility instead of enrollment is that it helps remove confounding effects correlated with the enrollment decision. The drawback is that the results do not represent how actual participation in Medicaid affected an outcome—i.e. it does not represent the effect of Medicaid for those that actually enrolled in the program. Rather, it represents the effect of expanding Medicaid eligibility.

In one of the earliest studies to examine the effect of public insurance expansions on prenatal care and birth outcomes, Haas et al. (1993) investigated rates of late and inadequate prenatal care utilization in Massachusetts. Using administrative data they failed to find a significant effect of expanding Medicaid on prenatal care use. Several other studies using similar methods also failed to find an effect (Howell, 2001). Many studies have found that those enrolled in public coverage programs have lower rates of health care utilization and worse health outcomes than the uninsured or privately covered (Quesnel-Valee, 2004). However, these studies often examined a single state (and are therefore not nationally representative) or used inadequate control groups. A study of Medicaid enrollees that used uninsured individuals as a control group is flawed because those that choose to enroll in Medicaid may do so because they are in poor health. People could have a health condition prior to Medicaid enrollment that the researcher cannot observe. Because the researcher cannot control for unobserved health status, he or she might incorrectly infer from such a study that Medicaid makes people's health worsen, when in fact it was because of poor health that a person is on Medicaid.

A landmark series of studies by Currie and Gruber (1996a; 1996b) used national data and econometric techniques to reduce the effect of unobserved confounders. They found that Medicaid program expansions reduced delayed prenatal care utilization by almost half and decreased the rate of children going without an annual check-up by a similar margin (Currie & Gruber, 1996a; 1996b). Other studies using national data and rigorous methods tend to find similar results (Howell, 2001; Howell & Kenny, 2012). For example, Banthin and Salidin (2003) find that that Medicaid expansions increased the rate of children having at least one physician visit in the previous year by 7.7 percentage points.

Similar to outpatient utilization, Medicaid expansions have increased access and use of hospital services for the poor. Dafney and Gruber (2005) found that a 10% increase in eligibility led to an 8% increase in hospital utilization of children. They found that majority of the effect was for non-ambulatory sensitive conditions (Dafny & Gruber, 2005). Similar results were found by Bermudez and Baker (2003). They found that a 1% increase in CHIP eligibility in California reduced hospitalizations for ambulatory sensitive conditions by 0.41 admissions per 100,000 children.

The effects of Medicaid coverage on access and use of dental care are stronger and more consistent than effects of Medicaid on the use of general medical services (Howell & Kenney, 2012). Infrequent routine dental care among children is a strong predictor of poor dental outcomes and subsequent pain, missed school-days, and compromised nutrition. Mofidi et al. (2002) find that enrollment in North Carolina’s

CHIP program resulted in a 17 percentage point increase in routine dental visits from baseline.

Compared to the access and utilization literature, the study of Medicaid's effect on health is far less conclusive (Howell, 2001; Howell & Kenney, 2012). This is largely because health is a difficult construct to capture and it is affected by a large number of factors that are typically not measured, but are correlated with enrollment in Medicaid, and there is a clear recursive relationship between health and enrollment that can easily bias standard OLS regression estimates (Levy & Meltzer, 2008). Nonetheless, a handful of researchers have attempted to isolate Medicaid's independent effect on health. Health in these studies has been operationalized as birth weight, self-reported health status, disability, and mortality.

Currie and Gruber (1996a) find relatively small effects of Medicaid expansions on infant health. Small returns to health from Medicaid expansions are perhaps not altogether surprising given small take-up rates and apparent crowd-out. However, they find larger impacts among the initial group of lower income persons that were targeted by expansions. For example, a 30% increase in Medicaid eligibility among the lowest income group was associated with a highly significant 7.8% decrease in the incidence of low birth weight and an 11.5% decline in infant mortality. Among the higher income group, the incidence of low birth weight fell by only 0.2% and the mortality rate by 2.9%. In a separate paper, the authors find that the total expansion experience between the early 80's and early 90's (which included expansions to bother lower and higher income populations) reduced the child mortality rate (for children under 15), by 5.1 percent (Currie and Gruber, 1996b)

Lykens and Jargowsky (2002) find that Medicaid expansions during the late 80s and early 90s appeared to decrease the number of acute health conditions and functional limitations among low-income White children under the age of 15, but not among other racial groups. In many studies, results for minority racial groups are hampered by small sample sizes. A priori it is reasonable to expect greater gains by persons from minority racial groups due to the correlation of poverty and race.

Many other studies have examined the connection between Medicaid and health, but due to weak study designs that fail to account for unobserved confounding and selection bias, little confidence can be placed in their results. Hadley (2003) and McWilliams (2011) provide a summary of this literature. The pattern that emerges from existing studies that use stringent study designs is that Medicaid does indeed improve health; however, the effect is often modest, but notably stronger among younger and more disadvantaged populations.

Medicaid's Unintended Consequences

The provision of public health insurance could lead to unintended consequences that could negatively impact child development. For example, Medicaid might provide a disincentive for parents to join the labor force or to marry, both of which could negatively impact children. The evidence on Medicaid's impact on labor force participation is mixed. Two papers find no effect of Medicaid on the labor decisions of women during Medicaid's introduction (Decker & Selk, 2011; Schrumpf, 2011). Baiker et al. (2014) find no statistically significant labor market effects among the low-income childless adults that were affected by the Oregon Medicaid Lottery Experiment. In contrast, a working paper by Dave and colleagues (2013), examining Medicaid expansions in the 1980's, suggests that a 20 percentage point increase in eligibility would result in a 6.5% decrease in employment with larger effects for women at the low end of the socioeconomic ladder. These results agree with a study of contracting eligibility in Tennessee that appeared to lead to increases in labor force participation (Garthwaite, 2014) and with an evaluation of expansion of Wisconsin's Medicaid program which led to declines in labor force participation (Dague et al., 2014).

There has been very little recent work on marriage decisions and Medicaid. Medicaid could affect marriage in two ways. Early in the programs history benefits were restricted to single mothers which provided a direct disincentive to marry. Even after eligibility was liberalized there could be added disincentive to marry because Medicaid represented additional income that did not need to be obtained from a spouse. There is little evidence on the impact of Medicaid and marriage in the current literature. Only Yelowitz (1998) finds a modest impact of Medicaid expansions on marriage decisions.

Another pathway through which Medicaid could negatively impact child development is if participation in Medicaid early in life familiarizes a child with the benefits and application process which thereby decreases the cost of participation in Medicaid during the adult period. If Medicaid participation in childhood causes Medicaid participation instead of more productive engagement in the labor force during the adult period, then the intergenerational transmission of Medicaid could be harmful. There is no direct evidence on this process for the Medicaid program specifically, but evidence from the Aid to Families with dependent Children program does suggest some degree of intergenerational transmission of cash welfare receipt (Pepper, 2000).

In summary, a large literature has examined the connection between Medicaid policy, take-up of coverage and subsequent gains in utilization of care and health outcomes. As a whole this literature supports the notion that Medicaid could improve childhood well-being in a way that translates into longer term gains. It is possible that such gains could be offset by unintended consequences of Medicaid. However, the small set of papers that have found that Medicaid coverage earlier in the childhood predicts medium term health and academic achievement suggests that on balance Medicaid is a worthwhile investment in children.

Stage 2: Childhood Determinants of Adult Success

There are a wide range of childhood factors that shape economic and health outcomes in adulthood including family and neighborhood environments, acute and chronic stressors, and peer group influences. This review focuses on two factors that are most plausibly related to the availability of Medicaid: childhood economic resources and childhood health.

The most frequently studied childhood predictor of adult success is poverty (Heckman, 2008). Family income during childhood is highly predictive of economic status during adulthood: roughly 50% of low-income US children are low-income in adulthood – a correlation that outpaces intergenerational patterns in Canada and Europe (Corak, 2006). Parental income helps determine the level of investment families are able to make in their children in terms of nutrition, educational opportunities, neighborhood environments, and health. Estimates on the role of parental income on the outcomes of children later in life vary by setting and research design, but nearly all find a positive and significant association between family income and adult attainment. Haveman and Wolfe's classic review suggests that a 10% increase in parental income during childhood translateds to a 1-3% increase in later life earnings (Haveman and Wolfe, 1995). Duncan et al (1998) study the effect of family income in early childhood on later life earnings using the Panel Study of Income Dynamics (PSID). The effect sizes they observe are substantial: an increase of $3,000 for children age 0-5 is associated with a 20% gain in annual earnings during adulthood. The effect they find is primarily driven by the number of hours the adult child spends working. Another important finding from this study is that economic disadvantage in early childhood (ages 0-5) has much stronger effects than economic disadvantage later in childhood (Duncan et al., 1998).

Low-income during childhood also leads to later life health disadvantage. Case et al. 2002 observe that parental income is associated with child health status and that the health of low-income children erodes faster than those with higher incomes (Case et al., 2002). A number of other studies have linked poverty in early childhood to health in mid to late adulthood. For example, childhood poverty is associated with increased mortality, cardio-vascular disease, diabetes, hypertension and other highly prevalent diseases.

Importantly, while this work is often unable to make causal inferences, it does demonstrate that these patterns persist after controlling for economic status later in life (Raphael, 2011).

While studies in this vein often control for a number of potential confounders that could obfuscate the relationship, they remain open to the criticism that some underlying unobserved factor, such as genetic endowment or the propensity to invest in children, is responsible for the link between childhood deprivation and adult outcomes. One approach for dealing with this is to examine naturally occurring "shocks" that lower everyone's income. By comparing those who experienced the shock to control groups from the same geographic area, but prior to or after the shock, researchers argue that they control for any unobserved factors that might confound the relationship between early deprivation and health. For example, van den Berg (2006) using data from the Netherland finds that children born during economic recessions have a lower life expectancy (by 2.5 years) than those born during boom years. They argue that families with low-incomes are particularly sensitive to macro-economic conditions (van den Berg et al., 2006). While studies that examine macro-economic conditions and cohort events provide insight into influence of early life financial resources on later life, it should be noted that they are not perfectly analogous to studies that examine variation between families. The effect of economically depressed communities is likely different than the independent effect of family level poverty.

Impact of Early Childhood Health on Later Life

The impact of family income during childhood on later life is likely substantial, however, the ability of Medicaid to free-up family income may be small because those without sufficient resources may opt to forgo medical care before taking up coverage. However, the ability of Medicaid to improve childhood health, especially among extremely disadvantaged groups with few insurance alternatives, is potentially larger than the income effect. In the review that follows I demonstrate that early childhood health is strong predictor of health and economic attainments in adulthood.

Most studies measure early childhood health using birth weight (Almond and Currie, 2011). It is most often operationalized as low birth weight—a dichotomous variable indicating less than 2500 grams (5.5 pounds). Birth weight is an appealing measure of early health because it is objective, sensitive to *in utero* exposures and can improve with intervention. Birth weight is correlated with infant mortality and is often included in available data sets. However, it is not a comprehensive reflection of health.

Unique insights on birth weight come from fixed effect studies of twins that control for underlying genetic differences. However, twins are often lighter than singleton infants, which limits the generalizability of twin comparisons. Variation in birth weight in twin studies comes from random fluctuations in nutrient intake caused by womb position. Almond et al (2005) find that heavier twins have no advantage in 1-year mortality compared to their lighter twin, suggesting that birth weight is not a perfect proxy for infant health (Almond et al., 2005). Indeed, different health domains correlated with low birth weight are responsible for high infant mortality rates among low birth weight infants. The research by Almond and colleagues does not suggest that low birth weight signals nothing about health, but rather it is an incomplete representation of health early in life. They argue that a policy intervention targeted solely at low birth weight will not remediate all the consequences of poor infant health. Despite these limitations, birth weight, thought of as a proxy for underlying health rather than a health state of itself, has proved to be a robust predictor of adult attainments across a variety of settings (Almond and Currie, 2011).

A number of researchers have linked low birth weight to adult diseases including heart disease, hypertension, diabetes, respiratory conditions, mental disorders and reduced cognitive function (see the review in Almond and Currie, 2011). The fetal origins hypothesis remains one of the leading explanatory mechanisms for explaining correlations between birth weight and later disease; however, the explanation is not without controversy (Huxley, Neil, and Collins 2002).

Perhaps the first the study to document the association of low birth weight on a non-health outcomes in adulthood was conducted with a British cohort born in the 1950's (Currie and Hyson, 1999). Currie and Hyson (1999) observe that low birth weight is associated with decreased educational attainment, employment, and adult health after controlling for a number of family background characteristics. Other work discovered that prenatal and early childhood health had direct effects on health and economic status at age 42, controlling for health and economic markers in earlier adulthood (Case et al., 2005). Haas et al (2011) using a rich set of social security earnings data linked to survey responses finds that those in poor health early in life have an earnings disadvantage that expands over the course of the working career.

While these and other studies provide correlational evidence, they are unable to identify a causal relationship between infant health and later outcomes. Even with longitudinal data, unobserved factors such as genetic endowments or community characteristics could be responsible for the observed relationship. To overcome this obstacle a number of studies have adopted more stringent designs.

Fixed effects studies examine the difference in adult attainments between siblings or twins that had different birth weights—thereby controlling for any shared and unobserved covariate. Berhman and Rosenweig (2005) studied the effect of birth weight adjusted for gestational age in a Minnesota cohort (1936-1955) of mono-zygotic (MZ) female twin-pairs. Like Almond et al. (2005) , their identifying assumption is that birth weight differences within twin pairs is caused by random fluctuations in nutrient intake related to womb position. Therefore, their fixed-effects MZ estimator represents the effect of birth weight, net of genetics and family environments (assuming that parents treat each twin the same). They find that a 1lb increase in birth weight would result in .3 more years of schooling and an increase in wages by 7% (~at age 40-60). They compared their twin estimator to OLS results that controlled only for the education of the parents and the fathers earnings. The OLS estimates were in the expected direction and significant, but underestimated the MZ estimator by 50%. However, their study suffered from small a sample size, survey measures with unknown measurement properties, attrition and item non-response. However, a study of twins in Europe using a rich set of administrative records and overcomes many of these limitations also found that birth weight was associated with improved education, earnings, BMI, and IQ (Black et al., 2007). Similar results from large samples have been found in the U.S. context (Figlio et al 2013; Royer, 2009).

Non-twin sibling studies provide less control over different endowments, but are informative. Using sibling differences in the PSID, Johnson and Schoeni (2011a) find the low birth weight babies in the U.S. are 1/3 less likely to graduate from high school, had 10% less earnings by age 25 and 15% less earnings by age 35. They also find that the effects of birth weight on adult health are mediated by late childhood health.

A second stream of research uses sharp exogenous events as natural experiments. These events are plausibly unrelated to individual endowments, parenting style, and community characteristics and can be used to create treatment and control groups. The most famous of these cohort events was the Dutch Hunger Winter (Roseboom et al., 2001; Schulz, 2010). The Dutch Hunger Winter was a famine event that occurred during the Nazi occupation of the Netherlands towards the end World War II. Food rations were constricted quickly and then were liberalized rapidly after the German withdrawal. The event provides variation in exposure to famine to subjects in utero—depending on when the subjects were conceived in relation to the start of the famine. Exposure to the famine has been linked to a number of adult disease outcomes such as obesity, heart disease, diabetes, affective disorders and schizophrenia. The effect sizes are often relatively large (e.g. odds ratios around 2-3). One of the most important findings from this work is that the timing of exposure *in utero* was found to illicit different disease outcomes (Roseboom et al., 2001).

Other famine, pandemic and diurnal fasting events have been studied (e.g. Chen and Zhou 2006; Almond and Mazumder 2011). For example, Almond (2006) finds that US children *in utero* during the influenza epidemic of 1918-19 were 15% less likely to graduate from high school, received 5-9% lower wages, and had substantially lower occupational status scores compared to children born just prior to or after the epidemic. Pandemics likely disrupt communities in important ways that are correlated with, but not the same as an individual level health shocks.

Less is known about the effects of health insults that occur in middle childhood on later life. Correlational evidence from the Health and Retirement Study suggests that serious illness during childhood is associated with cancer, heart disease, lung conditions, and rheumatism in adulthood (Blackwell, Hayward, and Crimmins 2001). Smith (2011) finds that 25-47 year old survey participants that retrospectively report better health prior to age 16 have improved income and employment, but not education. Furthermore, birth weight effects identified by Johnson and Schoeni (2011b) are independent of later childhood health status, but they find that retrospectively reported childhood health status is also significantly related to later disease (controlling for birth weight) at magnitudes comparable to the effects of birth weight.

Long Term Impact of Health Interventions

Growing evidence on the impact of early life health on health and economic status in adulthood has led to growing interest in examining the role of early life health interventions on health and economic outcomes in adulthood. Bleakly (2007) finds that hookworm eradication among U.S. school children in the South during the turn of the 20th century lead to increased adult income and economic returns to schooling. Hospital desegregation during the civil rights era led to a dramatic convergence in the Black-White infant mortality gap (Almond et al., 2007). Almond and Chay (2006) have linked these early life health gains to reduced rates of hypertension and diabetes among Black persons in adulthood. Perhaps the most interesting finding is that children born to Black women born during the late 1960's when integration was complete were far less likely to be low birth weight compared the children of women born during the early 1960's. They provide several convincing robustness checks including finding a very small effect among White women (Almond & Chay, 2006). Overall their results suggest that public policy can alter the intergenerational transfer of health along the birth weight dimension.

Bharadwaj, Løken, and Neilson (2013) show that the infants born just below the very low birth weight threshold, who receive more intensive medical care compared to their slightly heavier counterparts (due to medical guidelines rather than individual health) score higher on standardized academic tests at school age. Bhalorta and Venkatarmani (2011) find that sulfa antibiotic use, which dramatically decreases the incidence of childhood pneumonia, is associated with higher education and income and decreased disability in adulthood. Lee (2013) finds that mandatory school vaccination laws increase high school completion, labor force participation, wages, and returns to schooling.

Noticeably missing from this literature are natural experiments that gauge if contemporary public programs targeted at early childhood health, such as Medicaid, affect later life attainments (Almond & Currie, 2011). However, there is analogous work that evaluates programs the more broadly focus on enriching the economic and family environments of children. Garces et al. (2002) found that White children who attended Head Start, a preschool program for disadvantaged children that provides education, parenting, and health and nutrition assistance, are 20% less likely to drop out high-school compared to their siblings that did not attend. The most well-known examples of the long term impact of comprehensive early childhood intervention programs come from the Carolina Abecedarian Project and the Perry Preschool Program. Each project consisted of a randomly assigned treatment consisting of a high quality intensive early childhood intervention for children born into disadvantaged families. Due the random assignment of the treatment, evaluations of these programs are more easily able to isolate causal effects. Both have demonstrated consistent findings that early childhood intervention leads to improved income and educational attainment in adulthood, and decreased welfare participation and involvement in the criminal justice system (Conti & Heckman, 2014). Recent evidence from the Abecedearian Project suggests that treated children have substantially lower measured blood pressure and metabolic syndrome in their mid-30’s (Cambell et al, 2014).

A final study that is worth lengthier attention is a recent evaluation of the long-term effects of the food stamp program (FSP) (Hoynes et al, 2013). This work shares similar methodological features to the current project. FSP provides nutrition assistance to low income families and is thought of as a “near cash” program because the food voucher provided to participants often does not exceed their pre-intervention food budgets. Thus, the additional resources provided by FSP can serve to increase non-food budgets. Therefore, the impact of FSP is often interpreted as an income effect rather than a nutrition effect. The study by Hoynes and colleagues (2013) uses county-level variation in the timing introduction of FSP in the 1960’s and ‘70s. They use this variation to create a treatment variable that captures the fraction of time from birth to age 5 that a person was exposed to a food stamp program. The actual operationalization of the variable considers only when FSP started in their county of birth so there is likely some measurement error related to the geographic mobility of the sample. They assess the effect of this exposure to a food stamp program from birth to age 5 on a number of health outcomes (including health status and conditions) and economic outcomes (education, earnings, family income). They use a difference and difference approach implemented with a fixed effect model (fixed effects for year and county of birth), controlling for exogenous demographic variables and county-by-time variables that capture changes in other public programs and the health care environment. They also use a linear time trend in year of birth to capture secular trends that co-vary with exposure. Their health measures are taken after each subject is over 18 and their economic outcomes after each person is over the age of 25.

Because the fraction of the population that likely participated in FSP was relatively small, they attempt to isolate the effect of the treatment on the treated using a triple difference frame-work. Specifically, they interact the treatment variable with group specific FSP participation rates that are measured in a specific year (1978) in the full PSID sample (i.e. it is not geographically specific). This strategy compares the treatment effect for people that were more (or less) likely to participate in the program.

Results from Hoynes and colleagues' triple difference analysis suggest that a 10% increase in early life exposure to FSP improves health status and the presence of chronic conditions by 1.5-2% (statistically significant). Their findings from the economic outcomes they assessed are mixed. They find a highly significant effect of exposure to food stamp programs with the log of family income, but a negative (and insignificant) effect to education. The relationship between exposure to food stamp programs and earnings, employment, and poverty status were all in the expected direction, but were not significant.

III. Summary

The evidence reviewed in this chapter suggests the following:

a. Medicaid appears to increase utilization of care and improve early life health. However, the effects, especially in terms of health, appear to be more robust in lower-income populations.
b. There is limited and inconsistent evidence that Medicaid improves the financial resources for children in low-income families. However, a few

studies find that Medicaid eligibility in adult populations without children has a financial impact. Such gains could be offset by disincentives to participate in the labor market or to marry.

c. The benefits of Medicaid appear to persist over the medium term
d. There is consistent evidence that economic resources and health in early life influences health and economic status in adulthood.
e. There is growing evidence that childhood health interventions that supply direct care and policies that increase access to care (i.e. desegregation) have long run health and economic impacts.

My research builds on this literature by examining the long-term impact of Medicaid exposure in early life on health and economic outcomes. To measure Medicaid's impact I rely on variation in exposure to Medicaid that resulted from the programs staggered introduction across the states. In the chapter that follows I describe the history of Medicaid's introduction.

CHAPTER 3. A BRIEF HISTORY OF MEDICAID'S INTRODUCTION[3]

This chapter provides a brief history of Medicaid's introduction. I describe the system of welfare medicine that existed prior to Medicaid's enactment and argue that it left a large fraction of the poor without access to basic medical services. I then describe the eligibility and benefit policies of the new program and contend that Medicaid substantially increased public health insurance and had the potential to increase access to health services for the poor. I discuss the state-level variation in adoption timing that I use in later chapters to identify Medicaid's short and long-run impacts. Here, I discuss some of the challenges of using Medicaid's introduction as a natural experiment.

I. The Lead-Up to Medicaid's Introduction

The financing of medical care through insurance became increasingly important in the middle part of the 20th century. Medical technology and its associated costs advanced at a steady clip, buoyed by large federal investments in medical research and education (Starr, 1982). During this period health insurance through employment was subsidized in the tax code, solidifying insurance as a benefit of employment. Escalating costs coupled with the lack of an organized system of affordable health insurance outside of employment led to a growing segment of population that could not afford basic medical services.

[3] Some passages in this chapter were adapted from Boudreaux and McAlpine (2013) and are printed with permission from ABC-CLIO, Inc.

In the void of a national health insurance program, attention turned to incremental proposals that extended public health benefits to well defined “deserving” populations. The definition of these deserving populations, the generosity of public benefits, and the general administrative structure of the American welfare state was solidified in the original Social Security Act of 1935. America’s liberal welfare philosophy was based on the idea that benefits ought to be provided in a manner that minimized the role of the government vis-a-vis the private market. Benefits were restricted to groups that were not expected to work in the formal labor market—single mothers with children, the blind and disabled, and the aged. Any group of persons that could theoretically work (i.e. able bodied men) were excluded from the system even if the only work available would not lift them out of poverty (Piven & Cloward, 1972; Quadango, 2005; Stevens & Stevens, 1974).[4] The administration of the welfare system was state-based which enabled local authorities to set means-testing and benefit levels in reference to local wages (Piven & Cloward, 1972).

The original Social Security legislation focused on cash transfers that beneficiaries were to use for all of their expenses, including medical care. However, the escalating cost of care and meager cash benefits meant that beneficiaries had little access to medical services. Under the Eisenhower administration, amendments to the Social Security Act in 1950 provided optional federal matching funds to the states for direct reimbursement of providers that served those on the public assistance rolls (i.e. vendor

[4] That introduction of the Earned Income Tax Credit in 1975 departed from this pattern by providing cash transfers to the working poor. Changes in AFDC that emphasized work also suggest changing norms about the role of women in the workforce. Changes in work expectations among welfare participants were also a shift away from education and training to participation in the labor force regardless of the nature of the work (Moffitt, 2002).

payments). The 1950 amendments provided some relief to the poor. By 1960 hospital utilization had improved and the program incurred over $500 million in expenses ($3.8 billion in 2012 dollars) (Stevens & Stevens, 1974).

However, not all states participated, and the aged-poor were left out of the program (Stevens & Stevens, 1974). The case of the aged-poor generated legislative and public debate. In a series of national studies, the elderly were portrayed as universally poor or at risk for poverty due to the health problems that accompany aging and the high costs of medical care (Corning, 1969). In addition, the elderly had developed an effective lobbying base in the National Council of Senior Citizens for Health Care that was advocating for coverage (Corning, 1969).

In response, the Kerr-Mills program was adopted in 1960 to help finance the medical care of the aged-poor through direct vendor payments. The Kerr-Mills program contained many features that were later to be adopted by the Medicaid program. First, it recognized a new category of deserving beneficiary – medical indigents whose income was too high to be considered poor, but whose medical expenses exceeded income. Second, it provided an open ended federal grant to the states (i.e. an entitlement) that was devoid of limits on individuals or aggregate state-level expenditures. Opponents of universal coverage for the aged erroneously predicted that that the Kerr-Mills program would satisfy reformers (Stevens & Stevens, 1974).

Despite Kerr-Mills and the earlier expansions under Eisenhower, the provision of medical care to the poor continued to be highly fragmented through the middle 1960s. In 1961, thirty-three states provided some form of hospital benefit to mothers and children receiving public assistance and thirty-two provided physician benefits (U.S. Committee on Ways and Means, 1961). Medical benefit levels through the public assistance programs varied considerably, but for mothers and dependent children they were generally small. For example, in the 9 most generous states, the average medical expenditure for AFDC recipients was $4.00 per beneficiary per month. To put that level in context, adjusting for age and sex, the general population averaged $8.75 per person per month in medical expenses (Committee on Ways and Means, 1961).

The poor on public assistance may have turned to other sources to finance medical spending. For example, Maternal and Child Health Services grants and public hospital systems provided some services to local populations. Several programs related to the War on Poverty (starting in 1964) had a health component. However, these programs were locally administered and varied substantially across and within states. In its entirety, the system of welfare-medicine was generally recognized as not fully meeting the medical needs of the poor (Stevens & Stevens, 1974; Quadango, 2005; U.S. Committee on Ways and Means, 1961).

The poor could have also turned to private sources of charity care, but there is little direct evidence on the activities of third-party charities or medical providers that offered services without payment. National survey data suggests that low-income children not only were less likely to have private hospital insurance, but were less likely to use hospital or physician services compared to their higher income counterparts. Figure 3.1 describes the unadjusted proportion of young children, in 1963-64, that had private hospital insurance, had an overnight hospital stay or a regular physician check-up in the 12-months prior to interview. Roughly 5 percent of low-income children stayed overnight at a general short-stay hospital compared to 7% of high-income children ($p<0.001$). Twenty-one percent of low-income children had a regular physician check-up compared to 58% of high-income children ($p<0.001$). These data suggest that the combination of public assistance medical benefits, other public programing, and private charities did not equalize the provision of medical services across the income distribution.

II. Medicaid's Enactment

Continued advocacy from the elder lobby for a more robust health benefit coupled with a Democratic sweep of congress in 1964 lead to passage the Social Security Amendments of 1965. The legislation established two federally financed medical care programs: Medicare (Title XVIII) and Medicaid (Title XIX). The two programs took dramatically different approaches to eligibility and funding. Medicare is available to almost everyone on their 65th birthday; it is federally administered and program rules are uniform across states. It originally included hospital insurance coverage ("Part A") financed through payroll taxes, and an option to purchase subsidized insurance coverage for physician and other medical services ("Part B"). In contrast, Medicaid is a federal grants-in-aid program funded through general revenues. States receive federal matching funds in return for meeting a minimum set of eligibility and benefit standards, beyond which they can opt to expand. The policy differences between Medicaid and Medicare can be explained by two forces. The elderly, relative to the non-aged poor, had a well-organized lobby and were able to extract a more generous benefit package from the legislative process. The elderly were also not expected to participate in the labor market so there was a weaker desire among state-level stakeholders to design Medicare in a way that encouraged employment (Quadango, 2005; Stevens & Stevens, 1974).

Medicaid continued the existing practice of supplying medical care to the poor through direct payments to providers (at levels below private rates[5]) with joint state and federal financing. There were five essential changes relative to the existing system of vendor payments: 1) a requirement that states cover all people on public assistance; 2) a mandate that all states cover a set of essential benefits with no cost-sharing and an extended set of services at the states' option; 3) a consolidation of the existing public assistance medical care programs (which had previously differed by the public assistance category (i.e. women and children versus the aged-poor); 4) increased federal participation in financing that included the removal of federal spending caps that previously limited states' desire to participate in the vendor payment system; and 5) an extension of coverage to the medically needy at the states' option (Holahan, 1975).

At inception, Medicaid was a "sleeper" program that was heavily over-shadowed by Medicare in public discourse (Stevens & Stevens, 1974). Federal cost-estimates, based on experience with the previous vendor-payment programs, assumed that Medicaid would remain a small program. However, early projections substantially underestimated appetite for public health insurance programs in the states. Some states, such as New York, used Medicaid's optional provisions to extend eligibility to half of the state's

[5] The fee structure for physician services varied across the states. The typical fee schedule was 75% of usual and customary charges in the local market (Holahan, 1975).

population. Such efforts quickly resulted in unsustainable costs and eligibility was tightened. While the program was conceived as an incremental change, it turned into the largest policy change affecting health care for the poor in American history. Prior to Medicaid roughly 1 in 100 children received health care services that were financed by the public assistance vendor payment programs (or about 1 in 3 children receiving cash benefits). After Medicaid's adoption that share grew to 1 of every 10 children or nearly every child that received cash-assistance (Goodman-Bacon, 2013).

Over time Medicaid was expanded from a narrowly defined system of welfare medicine dedicated to providing medical services to distinct groups of low-income persons to a broader program that was part of a larger arrangement intended to achieve universal coverage. Starting, in the early 1980's eligibility for Medicaid was decoupled from the cash assistance programs and eligibility was liberalized (Gruber, 2003). Today, the program finances 48% of all childbirths and in combination with the Children's Health Insurance Program (CHIP) provides coverage to 35% (28 million) of all children under the age of 19 (Markus et al. 2013; SHADAC, 2013). On average the program consumes 16% of a state's budget and 8% of all federal spending (KFF, 2012). In 2012, federal Medicaid and CHIP expenditures on children (measured as outlays) were $75 billion, compared to $61 billion for nutrition programs, $60.1 billion for tax expenditures, and $50.4 on income support (Isaacs et al. 2013). Starting in 2014, the Affordable Care Act (ACA) allows states to provide benefits to all people under 138% of the poverty line, regardless of their family structure, and many states have expanded benefits beyond that

138% threshold. As of January 2014, nineteen states cover children up to 300% of poverty.

Eligibility and Benefits

At inception Medicaid provided health insurance coverage to three distinct groups of people: 1) low-income mothers and children, 2) low-income blind and disabled, and 3) low-income elderly (U.S. House Committee on Energy and Commerce, 1993). The essential eligibility and benefit components of Medicaid were carried over from the existing grant-in-aid public assistance programs. The legislation established two classes of eligibility: mandated (required by the federal government for fund matching) and optional. These are described in Table 3.1 (Benard & Feingold, 1970).

The primary eligibility group at Medicaid's introduction was federal cash assistance recipients. Cash-assistance recipients were automatically enrolled in Medicaid. The relevant cash assistance program for children was Aid to Families with Dependent Children (AFDC) which was the primary cash welfare program in the U.S. between 1935 and 1997 when it was replaced by the block-grant program Temporary Assistance for Needy Families (Moffitt, 2003). Medicaid eligibility for children varied across the states mainly as a function of prevailing AFDC eligibility rules (all members of the AFDC benefit unit, including the parent, were enrolled in Medicaid). Eligibility for AFDC was largely confined to single-mother families with dependent children under the age of 18 (or less than 19 in some states). The original AFDC legislation defined parenthood on a biological basis. A child living with a parent and their cohabitating or married partner was not categorically ineligible under federal law. This single-parent definition has often been misunderstood by researchers (Moffitt et al. 1998). Many states also ignored the biological basis of the single-parent requirement and instituted "man-in-the-house" rules that disqualified children if their biological parent lived with an adult partner. However, in 1968 the Supreme Court ruled that such eligibility criteria were not congruent with the federal legislation and subsequently banned them, but allowed the states to count the partners income during the means-test for eligibility (Moffitt et al. 1998). In addition, in 1961, the states were given the option to provide benefits to families with two biological parents if the primary earner was unemployed. The combination of the unemployed parent programs and the 1968 Supreme Court ruling meant that the AFDC population, starting in the late 1960's, was not uniformly composed of children living only with their mothers. Moffitt et al. (1998) estimates that during the late 1980's, between 14 and 30% of women participating in AFDC were married, and an additional 5 to 8% were cohabitating.

A family that met the requisite household structure requirements was subjected to a means test. The test compared gross-income (measured on a monthly basis) minus some disregards to a state's "need standard". Some assets were counted as disqualifying resources. The income cut-offs varied dramatically across the states. For a family of 3 in 1970 the needs standard ranged from \$149/mo in Arkansas (58% of poverty) to \$351/mo in California (136% of poverty) (Green Book, 1996). The AFDC cash benefit was set by the states and varied as a function of need. There was similar variation in benefit standards as there was in needs standards, but generally the cash-grant was small. In 1978 the maximum benefit was less than 100% of the need standard in 32 states (Chief, 1979).

Figure 3.2 shows the monthly average number of children receiving AFDC benefits between 1963 and 1980 and provides a gauge for the number of children that were affected by Medicaid's introduction. In the early 1960's roughly 4% (3 million) of children (under 19) received cash assistance. The share of children on AFDC grew over time. By 1980 just under 10% of children received public assistance (7 million). The graph clearly depicts the "welfare explosion" –a period of rapid growth in the AFDC program (Moffitt, 2003).[6] These figures represent average caseloads at a point in time, but do not speak to the average length of time a recipient remained on the program. Data from the early 1980's suggests that 48 percent of AFDC spells lasted less than 2 years. However, at any given point in time the AFDC population was primarily composed of long-term beneficiaries that stayed on the rolls for at least 8 years (Green Book, 1996).

While the majority of children that were affected by Medicaid's introduction gained access to the program via AFDC participation, the states had options for expanding eligibility to other populations. Twenty-nine states had a medically needy program, 30 states covered income-eligible women pregnant with their first child, and 18 states covered needy children under the age of 21 regardless of their categorical AFDC eligibility (Davis & Schoen, 1978; Foltz, 1975).

[6] The causes of the welfare explosion still remain uncertain. Work in economics has ruled out macro-economic conditions like unemployment and fluctuation in wages, increasing rates of births to unmarried parents, changes in income eligibility and changes in benefits which were actually decreasing (see Decker and Selk, 2011 for a review). Work in sociology suggests that eligibility determination was liberalized at the local level. Piven (1972) hypothesizes that welfare benefits were used to calm civil unrest. The contribution of Medicaid to the welfare explosion is discussed later in this chapter.

Covered health services under Medicaid included a set of essential benefits that were provided with no cost-sharing: inpatient and outpatient hospital services, lab and x-ray, skilled nursing-home services, and private physician services. The states could also cover an extended set of benefits that included cost-sharing (e.g. dental or home health care). The Early and Periodic Screening, Diagnosis, and Treatment program (EPSDT), added in 1967, ensured that children received a comprehensive set of preventative and therapeutic services.

III. The Timing of Medicaid's Introduction across the States

One of the key features of Medicaid's introduction that I use extensively in this project is that the decision of whether or not to adopt was left to the states. However, the federal legislation stipulated that all federal funding through the pre-existing vendor payment systems was to cease on December 31st of 1969. Coupled with increased federal spending under Medicaid, the threat of withdrawal of other funds gave states a powerful incentive to start a Medicaid program. They were allowed to do so starting on January 1st of 1966. While all the states eventually adopted a program, they did so at different times. The nature of Medicaid's introduction provides both time and geographic variation in exposure to Medicaid.

Figure 3.3 describes the timing of Medicaid's roll-out across the states. Twenty-six states formally adopted programs in 1966, 11 in 1967, 11 between 1968 and the end of 1970, and Alaska (1972) and Arizona (1982) were the last to start a program. Within each year, states also varied in the month of adoption. For example, among the 1966 adopters, just over half adopted in July, a quarter adopted in January, and the remaining adopted during other months. In addition to variability in when a state officially started a Medicaid program, there was likely also variability in when recipients were aware of their new benefits. For example, New York's program was implemented in May of 1966 and the state's 575,000 AFDC beneficiaries were automatically enrolled at that time. However, identification cards were not mailed until October 1966, the same month the state began accepting applications from non-welfare eligibles (New York Times 1966a,b). Unfortunately, I do not observe notification dates for the rest of the states and I know of no source that describes public knowledge of the program. Therefore, I rely on implementation dates (as described in Figure 3.3) to measure the presence of Medicaid. Implementation dates are a policy relevant and measurable marker of the presence of Medicaid.

There is no simple or clear answer as to why some states adopted earlier and some adopted later. Federal rule making was chaotic which may have had a larger effect on some states than others (Stevens & Stevens, 1972). The general pattern in Figure 3.3 appears to corroborate reports from the era that states with relatively generous existing welfare medicine programs (primarily concentrated in the Northeast) had incentives to adopt programs quickly so that expenditures could be shifted on to the federal budget. Indeed, some states, such as California and Pennsylvania, were enacting Medicaid legislation at the same time that the federal legislation was being debated (Stevens & Stevens 1974). In states with less generous programs (particularly in the South) Medicaid represented a new expenditure and adoption tended to occur later. This pattern is expressed more clearly in Figure 3.4 which plots average per recipient expenditures from the vendor payment system in 1960 (for participants in AFDC) as function of the relative timing of Medicaid adoption across the states. The figure shows that states with above average spending adopted earlier and states with below average spending adopted later.

Heterogeneous levels of pre-Medicaid health spending on the public assistance population could suggest heterogeneous effects of Medicaid across the states, for both short run outcomes (utilization and health) and the longer term outcomes including adult health and socioeconomic status. This story is complicated by the fact that Medicaid programs were not homogenous, but varied in eligibility and benefits (Stevens and Stevens, 1972). Figure 3.5 plots a measure of per beneficiary Medicaid spending (the ratio of total Medicaid expenditures (1975 dollars) to total 1975 AFDC caseloads) as a function of the level of 1960 vendor payments per AFDC recipient (in 1975 dollars). Caution should be exercised when interpreting this graph because the underling Medicaid expenditures represent spending on every eligibility category, but the vendor payment data is specific to AFDC recipients. However, the graph suggests that states with larger pre-Medicaid programs subsequently had larger Medicaid programs. This suggests that Medicaid may not necessarily have had a larger impact in states with lower pre-Medicaid spending.

It is possible that other pre-Medicaid state level factors influenced a state's decision about when to adopt. The overall budget circumstance of the state, voter demand for public spending on the poor, pressure (either for or against) by special interest groups, and a state's bureaucratic capacity could have all played a role in adoption timing.

Strumpf (2011) reports that a series of pre-adoption state-level factors (i.e. local welfare spending; age distribution, etc.) were not significant predictors of the timing of Medicaid's adoption. Finkelstein (2007) claims that hospital level characteristics, such as admissions and total expenses, predicted the timing of Medicaid's introduction, but she provides no specific information on the magnitude or direction of these correlations.

If unobserved factors correlated with the timing of Medicaid's introduction were also correlated with the outcomes of interest, then estimating Medicaid's impact through variation in adoption timing would not isolate Medicaid's independent causal impact. If these confounding influences reflect persistent differences between the states then they can be accounted for using state fixed effects. However, state fixed effects will not account for the influence of relevant factors that were changing at the same time as Medicaid. Problematic variables include those that changed contemporaneously with Medicaid and those that were changing prior to Medicaid's introduction, but are correlated with adoption timing. For example, Goodman-Bacon (2013) suggests that within state child-mortality rates were declining prior to and after Medicaid's introduction, for both Black and White children. He demonstrates that when regressing child mortality rates on an indicator of Medicaid availability (defined at the year and state level) and year and state fixed effects, Medicaid does not appear to have an impact on the mortality of either Black or White children.

Secular changes in child mortality, and child health more generally, could have been driven by the diffusion of medical technology or other public investments such as improved nutrition. To the extent that these variables can be observed, they can be controlled for. For example, the Bureau of Economic Analysis maintains an historical archive of state and county level spending on public spending for income support other public programs. The National Archives contains data on grant awards to counties that implemented War on Poverty programs such as Head Start and Child and Maternal health programming. The American Hospital Association and the Health Resources and Services Administration have historical data on the number of hospitals and physicians at the state and county level. The analyses described in Chapters 4-6 make use of these data to control for observed confounders that are potentially correlated with the timing of Medicaid's introduction.

While historical data provide the opportunity to directly account for spurious correlations it is reasonable to expect some level of confounding would remain unobserved. Several possible confounding variables, like the diffusion of medical technology, likely affected the entire population and not just those that were eligible for Medicaid. For example, in 1963 Jacqueline Kennedy gave birth to a premature infant who died within 48 hours of delivery. This event catalyzed the spread of regional neonatal intensive care units (Jorgensen, 2010). The spread of such technologies should have improved outcomes for all population groups. Separately estimating the impact of Medicaid for groups that were being targeted by the program (i.e. low income children) and groups that were not (i.e. moderate income children) would reveal if the timing of Medicaid's introduction was correlated with unobserved factors that affected the entire population. This relationship would appear as a significant effect of Medicaid for groups that were not actually eligible for the program. Chapters 4-6 apply this approach.

A related strategy is to rely on the fact that levels of public assistance participation have state based historical antecedents that pre-date Medicaid and have persisted over time (Piven & Cloward, 1972; Quadango, 2005; Stevens & Stevens, 1974). The "dose" of Medicaid, along the categorical eligibility margin, was larger in some states and population groups than it was in others, for reasons that pre-dated Medicaid. In 1963, AFDC rates for children ranged from 1.2% in New Hampshire to 12.5% in West Virginia. Figure 3.6 graphs the share of children receiving AFDC across time. The shares are calculated for each quartile of the 1963 level. While levels were increasing, the distance between each quartile remained mostly unchanged. Goodman-Bacon (2013) reports that these differences were present well before 1963 and likely were present in the mother's pension programs that predated the original AFDC legislation in 1935.

In Figure 3.7 I show that the 1963 AFDC rate (Panel A) and the 1963-1975 growth rate (Panel B) are both poor predictors of Medicaid adoption timing. The rate regression slope is 3.3 (p=0.9) and the growth regression slope is .001 (p=.8). If relative AFDC rates (in both levels and trends) were uncorrelated with other relevant factors that were changing over time, then comparing the impact of Medicaid across populations with varying AFDC probabilities would isolate the causal impact of Medicaid. Goodman-Bacon (2013) suggests that a number of potentially relevant covariates were balanced between relatively high and low AFDC states: child poverty rates, AFDC benefit levels, pre-Medicaid changes in child-mortality, and pre-Medicaid changes in hospital beds per capita. In chapters 4 and 6 I leverage differences in the predicted probability of AFDC participation to help identify the impact of Medicaid's introduction.

The discussion thus far has focused on contemporaneous changes correlated with Medicaid timing and pre-existing trends that vary as a function of relative Medicaid timing. Chapter 6 estimates the long-term reduced form impact of exposure to Medicaid in early childhood. This introduces an additional concern. Changes that occur after Medicaid's introduction that are correlated with the pattern of Medicaid's adoption could bias estimates of long-term impacts. It is important to separate these influences into two groups: those that fall on the causal pathway (and should not be controlled for) and those that are not mediators of Medicaid's impact. For example, it is plausible that the influx of federal funds crowded out previous state spending and that the states redirected funds from medical spending to other productive uses such as public housing or education. Another plausible pathway is that non-profit hospitals experienced an influx of revenue and provided increased community benefits and invested in superior medical technology. In that scenario, the total causal impact of Medicaid includes additional spending in other domains that is caused by Medicaid's more generous federal funding arrangements.

However, it is also plausible that new investments that occurred after Medicaid would have occurred if Medicaid was never introduced. If the level of these variables varies as a function of Medicaid timing (perhaps because timing is a proxy for a state's changing "taste" for public spending on poverty programs) then they could bias estimates of Medicaid's long-run impact.

As the discussion above suggests, archival data can be used to directly control for observable characteristics. However, a complete set of potentially relevant variables is unlikely to be obtained. However, Figure 3.8 suggests that Medicaid adoption timing has a small, positive, but statistically non-significant correlation with changes in per pupil K-12 public education spending (from 1970 to 2006). Public investments in education are one of the most likely unobserved variables that could undermine the study design. To the extent that new and unobserved investments simply reflect persistent state behavior they can be captured by state fixed effects. Similarly, to the extent that these new investments varied over time, but were consistent across the states or within region, they can be controlled for with year fixed effects and region-by-year fixed effects. State specific time trends can also go some way at reducing the threat of state specific changes that occurred subsequent to Medicaid's introduction.

Clearly, the timing of Medicaid's introduction across the states was not a purely random event and attempts to use Medicaid's adoption as a natural experiment must grapple with a number of alternative hypotheses that could explain correlations between the availability of Medicaid and subsequent outcomes. Chapters 4-6 describe results from alternative methodological approaches that attempt to limit the influence of unobserved variables.

IV. Previous Empirical Evaluations of Medicaid's Introduction

A handful of papers that focused on single communities were conducted shortly after Medicaid was introduced. Roghmann et al. 1971 compared crude rates of utilization for Medicaid and the privately insured in Rochester, NY in 1967-1969. They found that Medicaid recipients fared much worse that the privately insured. Olendzki (1974) used a pre-post design and found that Old Age assistance beneficiaries in New York City actually decreased their utilization of medical services after Medicaid. She suggests that the pre-existing public hospital system in New York was adequately meeting the needs of the aged poor. In contrast, Rabbin et al. (1974) found that Medicaid recipients in Baltimore in 1968-1969 were the highest utilizers of out-patient and in-patient services. This trend appeared to be driven by healthy individuals – the sick appeared to use care at the same rates.

Davis and Reynolds (1976) use nationally representative data from the National Health Interview Survey from 1969, when most states had a Medicaid program in place. They find that those on AFDC utilized physician services at the same rate as middle-income persons after adjusting for health status. Importantly, the utilization rate of those on AFDC outpaced low-income persons not on AFDC. Among women of child bearing age, the health-adjusted annual physician visit rate for those on AFDC was 8.8, versus 4.4 for the poor that did not receive cash assistance. Holohan (1975) studied the drivers of Medicaid cost inflation. He found that the utilization rate of Medicaid enrollees on AFDC increased by 1.5 percent per quarter between 1967 and 1972. He observed a decline in the growth rate over time and infers that the growth was attributable to outreach which eventually reached a point of diminishing returns. He also found the level of per-capita physicians in the metropolitan area was positively associated with utilization of physician care, but that smaller physician fees did not appear to dampen the quantity of services delivered.

Later authors have attempted to more fully leverage the state-by-year nature of Medicaid's introduction using difference-in-differences. Decker (2000) examines the impact of Medicaid's introduction on the probability of being a new single-mother (i.e. unmarried and recently gave birth) using data from the 1964 to 1972 Current Population Survey. To do so, she regresses an indicator of new single-motherhood on an indicator of Medicaid availability (that varies by state and year), state and year fixed effects, and series of time-varying state level controls (e.g. AFDC benefit standards). She finds a 14% increase (0.3 percentage points) in single-motherhood among White women and no effect among Black women. Using a similar approach, Decker and Selk (2011) study the contribution of Medicaid to the "welfare explosion" that is depicted in Figure 3.2. They find that the introduction of Medicaid explains 10% of the increase in AFDC caseloads between 1966 and 1972. They also estimate that the introduction of Medicaid had little effect on the labor supply of single mothers. They conclude that introduction of Medicaid increased AFDC participation among families that were already eligible, but it did not induce families to change their eligibility status. Similar work by Strumpf (2011) also found no evidence of Medicaid's impact on labor supply.

A more pertinent paper for my purposes comes from Decker and Gruber (1993). They investigate low birth weight using a difference-in-difference design and find that Medicaid's introduction reduced the incidence of low birth weight in the low-income population (less than $2000, nominal dollars) by a striking 60% (relative to baseline). They demonstrated support for their study design by observing null effects in higher income groups that were not targeted by Medicaid.

Goodman-Bacon (2013) observes that the introduction of Medicaid reduced mortality for non-white children under age 15 by 24 percent, for groups that participated in the program (implied from intent-to-treat estimates that are scaled by participation rates). His study design relies on comparing the impact of Medicaid's introduction in states that had high versus low AFDC participation rates. He also includes "timing-group-by-year" fixed effects which limit comparisons within broad phases of Medicaid's adoption timeline.

Both Decker and Gruber (1993) and Goodman-Bacon (2013) support the idea that introduction of Medicaid improved child health in ways that could be important over the long-term. Goodman-Bacon's results suggest that Medicaid increased the probability of surviving past childhood. The analysis presented in Chapter 6 does not account for survival bias. If children that survived as a result of Medicaid were in worse health than the general population it would suggest that estimates of the longer term impacts of Medicaid are downwardly biased.

V. Summary

The introduction of Medicaid represented a large shift in the provision of health care to the poor. Medicaid's roll-out occurred at different times across the states and I utilize this variation in Chapters 4-6. By relying on Medicaid's introduction to produce intent-to-treat estimates I avoid problems associated with unobserved selection. However, using Medicaid's introduction as a natural experiment is not without limitations. It is possible that the schedule of Medicaid's adoption timing was correlated with other changes that could have also impacted the outcomes of interest. My analyses, described in more detail in the following chapters, include three basic strategies for dealing with spurious correlations: I include a detailed set of contextual controls that vary by geography and time, I include state, year, and geography-by-year fixed effects, and I estimate triple-difference models that will reveal and control for otherwise unobserved secular trends.

CHAPTER 4. THE INTRODUCTION OF MEDICAID AND UTILIZATION OF MEDICAL CARE[7]

I. Introduction

Several studies of have found that eligibility for Medicaid increases children's use of medical care (Howell and Kenney, 2012). Currie and Gruber (1996b) use a simulated eligibility instrument and data from the National Health Interview Survey and find that Medicaid expansions between 1984 and 1992 decreased the probability that a child went without an annual physician visit by 9.6 percentage points and increased the probability of an annual hospital visit by 3.9 percentage points (a doubling of the base rate).

It is reasonable to expect that Medicaid's introduction had a different impact than the expansions of the 1980s and 1990s. Take-up is a necessary condition for a utilization effect and it was fundamentally different prior to the 1980's because Medicaid was mechanically tied to AFDC. Blank (2001) suggests that take up of AFDC in 1984 was between 75 and 95%, depending on the data source. In comparison, take-up of Medicaid in the expansion populations (that were not required to be AFDC-eligible) was much worse. Currie and Gruber (1996b) estimate that eligibility increased by 15.1 percentage points, but Medicaid coverage only increased by 7.4 percentage points.[8] In addition to higher take-up, Medicaid's introduction likely crowded-out a smaller share of private insurance than did Medicaid expansions. I estimate that approximately 20% of low-

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[7] This chapter describes data from the restricted use National Health Interview Survey. The findings and conclusions in this paper are those of the author and do not necessarily represent the views of the Research Data Center, the National Center for Health Statistics, or the Centers for Disease Control and Prevention.
[8] More recent estimates of take-up of Medicaid and the Children's Health Insurance Program, among children, suggest much stronger levels of take-up. Kenney et al. (2010) suggest a take up rate of 81%. It should also be noted that the relevant take-up measure for Currie and Gruber (1996b) is the marginal increase in participation for a given increase in eligibility and not average take-up across all eligibility levels.

income children had private insurance prior to Medicaid's introduction (see Chapter 3). In comparison, Currie and Gruber suggest that 68% of the children that were targeted by expansions had other coverage. Improved take-up and smaller crowd-out suggest that Medicaid's introduction may have had a larger impact on utilization than the expansion experience. However, it is possible that Medicaid did crowd-out charity care. Or that the unwillingness of a large numbers of providers to participate in Medicaid created non-financial barriers to access.

Measuring the impact of Medicaid's adoption on utilization of care is important to this project because it establishes the relevance of the policy for the longer-term impacts estimated in Chapter 6. My conceptual model posits that a critical mechanism linking Medicaid exposure in childhood to health and economic status in adulthood is utilization of health services in the childhood period. No other study, to my knowledge, estimates the utilization effects of Medicaid's introduction, using the full state-by-year roll out of Medicaid adoption. The existing work previously reviewed (see Chapter 3) generally focused on specific communities at a cross-section and came to mixed conclusions.

II. Data and Methods

Data

To measure the impact of Medicaid's introduction on use of services I use restricted-use data from the 1963-1980 National Health Interview Survey (NHIS). The restricted version of the NHIS has fully intact state identifiers that allow me to merge on state specific Medicaid adoption dates in addition to a detailed set of variables describing each state’s health care market and policy environment, as it evolved over time. My primary interest is children under the age of 6, given that the literature suggests that early childhood is a critical period of development in which health producing medical services can have long-term benefits. For completeness, I also estimate models for all children under 18 –the age at which most states no longer considered a child eligible for AFDC. I also consider mothers (defined as women living with related children under 18). Improved utilization by mothers could have positive long-run outcomes for children through three channels: a) improved utilization during pregnancy could improve fetal health; b) improved utilization unrelated to pregnancy could improve the health of mothers in ways that improve pregnancy outcomes; c) improved utilization could have improved health in ways that improved parenting (e.g. by increasing healthy parenting time). I focus on mothers rather than all women of child bearing age, because mothers with children had a far better chance of being eligible for Medicaid, given rules in the AFDC program.

The National Health Interview Survey (NHIS) is the primary source of information on the health of the U.S. population. It collects data about the demographic, socioeconomic characteristics, health status, and health care utilization of families through in person interviews. It has been fielded every year since 1957 and is based on a large, nationally representative sample of families. The sample frame is based on the preceding decennial census, updated for new construction, and housing units are selected through a multi-stage area probability design. The sampling plan is updated on a 10-year cycle. Information about each person residing in the household is obtained from a knowledgeable adult (DHEW, 1975).

This analysis focuses on data for approximately 760,000 children and 263,000 mothers that were collected between 1963 and 1980.[9] I rely on a harmonized version of the NHIS, called the Integrated Health Interview Series (IHIS). The IHIS has been standardized to have consistent variable names and response categories over time (MPC, 2012).[10]

Measures of Health Care Utilization

A natural measure of medical utilization for children is annual physician visits. Pediatric guidelines generally recommend an annual preventative care visit for children under the age of 21 (MHQP, 2014; American Academy of Pediatrics, 2014). Therefore, an increase in the probability of at least one physician visit over a 12 month period would provide a clear signal of improved access to care. Unfortunately, the NHIS did not consistently collect physician visit information during the 1963-1980 period. The Fiscal Year 1964 questionnaire (covering July 1963 to June 1964) asked, "About how long has it been since you have seen or talked to a doctor?" A similar question was not asked again until the Fiscal Year 1968 interview.[11] The absence of visit data between July of

[9] Very similar results, to those presented here, are obtained from using a shorter observation window that ends in 1975.

[10] The analytical data contains the harmonized data merged with original NHIS variables to ensure that the harmonization procedures met the needs of this study and to add variables from the NHIS that are not kept by IHIS. The requisite linking keys are not available in the IHIS in early years of the time series. I created them using internal IHIS files. I am grateful to Julia Drew of the Minnesota Population Center for assisting me in gaining access to these data.

[11] The NHIS operated on a fiscal year calendar until 1968 when it switched to calendar year schedule. This change coincided with a major redesign of the instrument from a "condition approach" to a "person approach". This change altered the questionnaire from a design that gathered information about a list of health conditions and then asked about the functional impairment caused by present conditions to a design

1964 and June of 1967 prevents me from establishing a reliable trend in annual physician visits prior to the introduction of Medicaid in 1966.

Due to the limitations of the physician visit series in the NHIS, my primary outcome of interest is the probability of an overnight hospital stay in the 12 months preceding interview. Hospital visit data has been consistently collected by the NHIS since its inception. The question asks, “Have you been in a hospital at any time since ____, a year ago?” NHIS interviewers were instructed only to count visits to short-term hospitals that included an over-night stay. Visits for mental illness, tuberculosis, orthopedic, contagious, and chronic diseases were not counted. The hospital visit data represent only services for acute conditions and will miss Medicaid’s impact on utilization of care for the excluded conditions. Another major limitation inherent in hospital visit data is that hospital utilization is confounded with morbidity. It is possible that Medicaid improved access to out-patient services and that these services reduced the need for hospital care. The effects I estimate capture both any decrease in the probability of hospital utilization that is driven by improved efficiency of care and any increase in the probability of hospitalization that is driven by improved access to hospital services.

Exposure to Medicaid (Treatment Variable)

After merging state specific Medicaid adoption dates (described in Chapter 3) with the NHIS (according to each household’s state of residence), I generate a binary

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that began with questions about functional impairment and then asked what conditions caused those impairments (DHEW, 1975). There have been numerous smaller changes in the questionnaire over time. For example, some useful variables, such as private health insurance coverage, we added and then removed from the instrument every few years.

treatment variable that equals 1 if Medicaid exists in the beginning of the hospital visit reference period. If a respondent was interviewed in April of 1970 their hospital visit data pertains to visits that occurred from April 1969 through March of 1970. I consider such a person to be exposed to Medicaid if Medicaid was adopted in their state of residence by April of 1969. This process is facilitated by year and quarter of interview variables included in the NHIS.[12] Figure 4.1 plots the weighted fraction of children (age 0-18) in the NHIS that are exposed to a Medicaid program. Alaska and Hawaii are excluded from the sample because they have incomplete contextual data (described below). Arizona has been excluded because its adoption date is a clear outlier (1982) and I do not have firm understanding about why it adopted so late. The removal of 3 states excludes 33,746 people from the data (1.5%). The chart shows that about 35% of children were exposed starting in 1966, 65% in 1967, and by 1970 every child in the sample lived in a state with a Medicaid program. The graph does not demonstrate within year variation that is driven by the month of adoption.

Income and Predicted AFDC Participation

Income data are particularly import for my purposes because Medicaid was targeted at very low-income families and without reliable income measures it would be difficult to identify the target population. The income-eligible represented a very small fraction of the population and it is likely that even if Medicaid drastically improved

[12] The results presented here are robust to alternative definitions of the treatment variable. For example, I get similar results with a variable that has three levels which include pre (Medicaid starts after the hospital reference period), implementation (Medicaid starts in the year of the reference period), and post (Medicaid starts before the beginning of the reference period).

access to care for the income-eligible, the impact would not be detectable in the total population. Furthermore, I can use the fact that exposure to Medicaid varied across income groups as another source of identifying variation.

The NHIS collects family income in categories and the intervals were not sufficiently adjusted over time to keep pace with a shifting income distribution (both real and inflationary). Over time, this caused the sample to increasingly heap in the highest interval until the categories were expanded to capture the upper tail of the distribution in more detail (this was done once in 1970). The major limitation of categorical family income is the difficulty of converting the nominal values into real terms so that a consistent low-income population can be tracked over time. The income variables are also subject to item-missing values and the rate of item non-response grew over time: from 5.1% in 1963 to 7.5% in 1980.

To address the limitations of the income data I follow procedures described by Currie and Gruber (1996b) and Currie et al (2008). First, I impute missing data using a single variable hot deck routine. Values are imputed for family heads and then applied to the rest of the family unit. The stratification variables in the hot deck included age (3 levels), the number of workers in the family (3 levels), education (2 levels), race (2 levels), and the median income in the geographic sample segment (a neighborhood level geographic cluster). Second, I assign precise dollar amounts by appending data from the NHIS to data from the March Supplement of the Current Population Survey (CPS) and then use a hot deck procedure to impute specific dollar amounts from the CPS to the NHIS, within each income bracket. The only other variable included in the hot deck, besides the income bracket, was the maximum educational attainment in the household (more detailed strata were not possible due to sample size). The resulting means and quantiles from the NHIS track closely with results from the CPS (see the appendix for a more detailed discussion of the income imputation procedures).

Using the continuous income variable I convert the nominal dollar amounts into real 1970 dollars using the CPI-U. Real, continuous family income allows me to easily separate the sample into low and moderate income groups. The low income group includes family income up to $3,000 (in 1970 dollars). The median annualized AFDC needs standard for a family of 3 in 1970 was $2,784 (Green Book, 1996) and so $3,000 is a reasonable if imperfect approximation of the income-eligible population. This low-income definition captures about 10% of children as estimated by the NHIS (see Table 4.1). The moderate income group includes children with $5,000-$10,000 of family income. The probability that a moderate income family was covered by AFDC and thus Medicaid was very low and Medicaid should have no impact on this group. As described below, I use the sample from moderate-income families as a control group.[13] I chose not to include families in the moderate income group that had slightly more than $3,000 because there is no firm threshold above which a family was not eligible (due to income disregards and family size dependent thresholds). Excluding families that just exceed the

[13] I also attempted to use the continuous income variable to create income-to-poverty ratios. However, because of substantial differences between the Census definition of a family and the NHIS definition of the family, the estimated NHIS poverty rates did not align with rates the CPS. For example, in 1970 the estimated poverty rate in the NHIS is 18% compared to 11% in the CPS. Due to these differences I chose to use the income data, which compares well across the two data sets (see the appendix). However, using an income-to-poverty threshold returns similar results to those presented here. However, the effects by poverty level fade out quickly as the poverty threshold is broadened from 75 to 150% of poverty. This aligns with the higher than expected poverty rates in the NHIS and suggests that the poverty variable is over-counting the population that is income eligible for Medicaid.

upper bound of the low-income threshold ensures that the two samples have distinct probabilities of exposure. I exclude the upper end of the income distribution in order to make the samples as comparable as possible.

Another approach to isolating Medicaid's target population is to look directly at families that report AFDC income. Unfortunately, the NHIS has not consistently collected information on AFDC participation over time. Additionally, as suggested by Decker and Selk (2011), the decision to enroll in AFDC was affected by the introduction of Medicaid. Their findings suggest that there could have been important compositional shifts in the AFDC population as a result of Medicaid's introduction and these shifts could obfuscate the causal impact of Medicaid, if I were to focus directly on the subset of families that reported AFDC income. For example, it could be that those that participated in AFDC only to gain access to Medicaid were in relatively poor health and had greater demand for hospital services. If this group had different pre-Medicaid utilization patterns than the general AFDC population then the effect of Medicaid would be biased. An alternative approach is to rely on the fact the while levels of AFDC participation were generally increasing, the relative probability of participating in AFDC, across states and demographic groups, has remained stable since the introduction of the program in 1935 (see a fuller discussion in Chapter 3). Estimating the relative impact of Medicaid's introduction for groups of people that had a higher versus lower predicted probability of AFDC participation will allow me to both focus on Medicaid's target population and subtract out any unobserved secular trends that affect the entire population, are unobserved in the model, and are related to the outcome of interest. Relying on predicted probabilities, rather than realized indicators of participation, helps to average out potential selection bias.

I create two variables that reflect the predicted probability of participation in AFDC; each of the variables has strengths and limitations.[14] The basic approach is to estimate AFDC participation rates by sub-group in the 1964-1981 March CPS (reflecting AFDC participation in 1963-1980) and then merge the probabilities with the NHIS. The first method calculates year specific AFDC participation at the national level within 16 demographics groups. The groups come from cross-classifying information on the household head: gender, race, education, and the number of workers in the family. The second method calculates year specific AFDC rates by state and the gender of the household head. State is a powerful predictor of AFDC participation because AFDC rules and take-up behavior varied across the states. Unfronately, due to relatively small cell sizes, condition the rates on state prohibits the inclusion of a large set of additional variables. Gender was chosen because survey rules define a household head as a married man when one is present, gender and marital status are nearly synonymous. Marital status is the best predictor of AFDC participation besides income.

The advantage of the first method is that the strata are reasonably predictive of AFDC participation and there is a high degree of variability in the means. Table 4.2 suggests that after merging the means to the NHIS the average predicted probability from method 1 is 0.03 and it ranges between .003 and 0.65. The disadvantage is that sub-groupings are composed of potentially endogenous variables such as work status. Method 2 overcomes this limitation by relying only on variation through the gender of the

[14] The appendix describes, in detail, the methods used to generate these probabilities.

household head and state of residence. As demonstrated in Chapter 3, relative state level participation rates were stable over-time. Neither pre-Medicaid levels nor changes over time were correlated the timing of Medicaid's adoption. The limitation of this approach, evident in Table 4.2, is that there is less variation in the measure. The mean is 0.02 and the standard deviation is a half the size as predicted probabilities derived by method 1. In the tables that follow I refer to the alternative AFDC predicted probabilities as AFDC Moderator 1 (the demographic sub-group method) and AFDC Moderator 2 (the state by gender method).

Covariates

All models include a set of simple demographic controls. Race is measured as white versus non-white (more detailed racial categories are not possible because of changing racial categories in the survey and small non-white cell sizes). Education of the household head is measured as at least some-high school versus less. Categorizing education in reference to some high school attendance versus high school graduation was dictated by available data. In the 1960's the NHIS grouped high school graduates with those who attended, but did not complete high school. I measure age using single year dummies and in models where the sample is composed of children I include the age category of the household head.

In addition to demographic controls, I merge the NHIS with a detailed set of contextual controls that vary by state and year. The contextual variables help control for other changes in the health care market and public policy environment. I observe per capita doctors and hospitals from the American Hospital Association and the Area Health Resource File. From the Bureau of Economic Analysis I have measures of public assistance expenditures for AFDC, Supplemental Security Income (cash transfers for the disabled), the Food Stamp Program, and General Assistance (local cash welfare). From the Administration of Children and Families I include per capita AFDC caseloads and from Berry et al. (1999) I observe the maximum AFDC benefit standard for a family of 3. The appendix provides additional information on these data sources.

Summary Statistics

The basic characteristics of the sample are described in detail in Table 4.1. The table is divided into the three subgroups of interest, children under 6, children under 18, and mothers of child bearing age (18-45). Approximately 66-72 percent of the sample was exposed to Medicaid starting at the beginning of the hospital visit reference period. Seven and half percent of children under 6, 5.5% of all children, and 21% of mothers stayed over-night at the hospital in the year prior to interview. The relative high rate for mothers is likely driven by child births. Eighty-six percent of young children, 72% of all children, and 83 percent of mothers had at least one physician visit in the last year (for the years in which physician visits were measured).

Table 4.2 describes means, standard deviations, and ranges for the predicted AFDC rates and the contextual variables. For 0-17 year olds, on average, there were 0.28 short stay general hospitals per 10,000 people, 0.14 physicians per 100 people, and $210 in public assistance spending per person (2000 year dollars). The maximum AFDC benefit payment for a family of 4 was $859 per month (2000 year dollars), and there were 37.8 AFDC participants per 1000 people. As expected there was little variation in the contextual variables across the three population groups.

Empirical Strategy

To identify the effect of Medicaid on the probability of having at least 1 annual over-night hospital stay I regress the binary outcome on the Medicaid indicator described above. The model takes the form described in equation 4.1:

$$(4.1) \qquad P(Visit_{ist}) = \Lambda(\lambda MCAID_{st} + \beta X_{ist} + \rho Z_{st} + \delta_t + \gamma_s + \phi_{rt}),$$

where $Visit_{ist}$ is the visit outcome for person *i* residing in state *s* and interviewed in year *t*. MCAID is the 0/1 indicator of Medicaid availability and λ measures the effect of exposure to Medicaid. X_{ist} is a set of individual level controls, including gender, race (white vs. other), education of the household head (some high school versus less), age category of the household head (<25, 25-44, 45-64 >64), and a full set of single-age dummies. The major purpose of these individual controls is to improve the precision of the model. Z_{st} is the set of contextual controls described above. The model includes interview year fixed effects (δ_t) and state fixed effects (γ_s). These hold constant any stable, but unobserved period or state effect. ϕ_{rt} are region by year fixed effects which control for unobserved region specific changes over time.[15] $\Lambda(\cdot)$ represents the logistic cumulative distribution function.

Equation (4.1) represents a generalized difference-in-difference estimator (Imbens & Wooldridge 2007; Bertrand, Duflo, and Mullainathan 2004) that captures the effect of Medicaid policy, rather than Medicaid participation specifically (i.e. intent-to-treat). Because Medicaid was only made available to very low-income families I estimate (4.2) for children and mothers that have less than $3,000 in annual family income. While the rich set of controls and fixed effects will go a long way in controlling for spurious correlations it is possible that they do not go far enough. To examine that potential that spurious correlations account for an observed effect, I also estimate (4.1) for children and mother's that report $5,000-$10,000 in family income. This placebo group of moderate income families was largely not eligible for AFDC, and thus Medicaid, and should not have benefited from Medicaid's introduction. Thus, the impact of Medicaid in the moderate income sample, captured by λ, provides a check on the study design. If the coefficient is similar to the coefficient estimated in the low-income sample it would suggest that λ is not isolating the impact of Medicaid.

A similar approach is to use the predicted AFDC probabilities described above to examine if the impact of Medicaid was different for groups with higher versus lower probabilities of participating in Medicaid. These probabilities are not directly a function of income and thus provide a different source of variation to identify the target population. I accomplish that by estimating equation 4.2, which takes the form:

[15] The models in chapter 6 use state specific trends rather than region by year fixed effects due to sample size constraints in the Panel Study of Income Dynamics. The results presented in chapter 4 are robust to replacing the region-by-year fixed effects with state specific trends in survey year.

$$(4.2)\ \ P(Visit_{ist}) = \Lambda(\lambda MCAID_{st} + \pi AFDCD_{gt} + \ \psi MCAID_{st} * AFDC_{gt} + \beta X_{ist} + \rho Z_{st} + \ \ \delta_t + \gamma_s + \phi_{rt}).$$

Now, λ captures the effect of Medicaid for groups that had a low probability of participating in the program. ψ is the coefficient on the interaction of the Medicaid indicator and one of the AFDC predicted probability measures that varies by sub-group and year. ψ will capture the relative impact of Medicaid for groups that had a high probability of participating in Medicaid.

I transform the logistic model coefficient on the Medicaid indicator in equation 4.1 using average-marginal-effects (Karaca-Mandic et al. 2012). The average-marginal-effects (AME) for the regression described by 4.1 represents the discrete change in the predicted probability of hospital use for people exposed versus not exposed to Medicaid. All covariates values are set to the value they take in the data (x=x*) and the predicted probabilities are averaged within the treatment groups, as shown in Equation 4.3.

$$(4.3)\qquad AME = \frac{1}{n}\sum_{i=1}^{n}\widehat{p_\iota}(y = 1|\ x = x^*, MCAID = 1) - \ \frac{1}{n}\sum_{i=1}^{n}\widehat{p_\iota}(y = 1|\ x = x^*, MCAID = 0),$$

where $\widehat{p_\iota} = \frac{e^{x_i'\beta}}{1+e^{x_i'\beta}}$.

For regressions described by (4.2), I calculate the AME of the predicted AFDC probability when Medicaid equals 0 and then for Medicaid equals 1. I interpret the difference of these two effects as the relative impact of Medicaid.[16]

In all analyses the NHIS sample weights are used to adjust for unequal probabilities of selection, non-response, and non-coverage error. Standard errors are estimated using Taylor series linearization which accounts for the NHIS sample clusters and stratification.[17] Following guidance from the National Center for Health Statistics, sample clusters and strata are assumed to be dependent within a sample design period and independent across periods.

III. Results

Table 4.3 presents the average marginal effects of exposure to Medicaid, estimated from the regression described in equation 4.1, for young children (under 6), all children (under 18), and mothers of child bearing age. The complete set of logistic regression coefficients is available in the appendix (Tables C4-C8).

For young, low-income children, the results suggest that Medicaid increased any annual overnight hospital stay by 3 percentage points ($p<0.01$). This translates into a 51% increase from the base rate. The effect was about half that size for the population of low-income children under 18. The point estimate suggests a 28% increase in hospital

[16] An alternative approach would be to calculate the discrete change in the predicted probability of hospital utilization, over the Medicaid indicator, at specific values of the predicted AFDC participation probability. However, either method should capture the same variation of interest and address the question: does turning Medicaid on increase hospitalization more for likely AFDC participants than likely non-participants?

[17] The appendix shows that this approach to variance estimation returns very similar results to clustering at the state level.

utilization for all children ($p<0.05$). The second column describes results for the moderate income sample which was not being targeted by the program. For both young children and all children, the coefficient is nearly zero and not significant. This suggests that the timing of Medicaid's introduction was not correlated with unmeasured trends that were affecting both low and moderate income children. However, I can only reject the equality of the coefficients for the young child group.

The bottom panel of Table 4.3 presents results for mothers of child bearing age. For both income groups, the estimates were small and non-significant. A large share of hospitalizations among women of this age range was likely related to child birth. By 1960, 97% of births occurred in a hospital (Midwifery Today, 2014); therefore, there was very little room on the extensive margin for Medicaid to improve the dominant form of hospitalization for mothers. It is possible that Medicaid increased the intensity or quality of hospital-based child birth services, but that would not be reflected in these estimates. In a robustness exercise, not presented here, I found very similar effects after excluding mothers living with children under 1 year of age.[18] These results suggest that that there is no evidence in these data that Medicaid improved access to hospital services for mothers of child bearing age.

Figure 4.2 describes results from an event study specification of equation 4.1, estimated for low and moderate income children age 0-5. The event study specification replaces the Medicaid indicator with a series of dummies that indicate the number of years until Medicaid's adoption in a child's state of residence. The time scale is bottom coded at -3 because children in early adoption states had, at most, 3 years of pre-data.

[18] Another avenue for future research is to examine the average number hospital nights for new mothers.

The marginal effects graphed in the figure are in reference to the year prior to Medicaid's adoption. The figure facilitates graphical inspection of pre-trending in hospital utilization. The event-study specification also allows the effect to vary over-time if, for example, there was a "ramping up" period as suggested by Hollohan (1972). The graph does show some minimal level of pre-trending, but it is mirrored in both low and moderate income children, suggesting that placebo comparisons in table 4.3 are reasonable tests of the study design. Starting in the first year of Medicaid's adoption (where the x-axis is at 0), the rate of growth in hospital utilization increases for low-income children, but remains flat for moderate income children. After 3 years from Medicaid's adoption, the effect dissipates for low-income children and there appears to be uptick for moderate-income children. The figure generally corroborates the results described in Table 4.3.

Table 4.4 presents the results from the AFDC specifications described in equation 4.2 and estimated on the entire sample (all income groups). For each method of calculating predicted probability of participating in AFDC I present the marginal effect of the predicted AFDC probability given Medicaid does not exist ("No Medicaid") and then given Medicaid does exist ("Medicaid"). Because the AFDC probabilities range from 0 to 1, the marginal effect represents the effect of going from 0 to 1. The parameter of interest is the difference in these two quantities which is listed in third row of each panel ("Difference").

The first AFDC moderator is the predicted probability of AFDC participation defined within year and 16 demographic groups. The results for young children suggest that the introduction of Medicaid increased hospital utilization by 5 percentage points in likely AFDC participants (and therefore likely Medicaid participants) relative to likely non-participants ($p<0.05$). A similar result was found for children under 18 and the result for mothers was small and non-significant.

A potential limitation of the first moderator is that probabilities were calculated across variables which families had direct and immediate control, e.g. work status. The second AFDC moderator represents probabilities calculated over year, gender and state of residence. The results from this analysis suggest that impact of Medicaid for likely AFDC participants was a 13 percentage point increase in hospitalizations for the youngest children ($p<0.01$) and a 10 percentage point increase for all children ($p<0.001$). The result for mothers was a 12 percentage point increase, much higher than any other regression presented thus far, but still not statistically significant.

The direction and significance of the coefficients on the demographic controls generally met expectations (see Tables C4-C8 in the appendix). White children were more likely to use hospital services compared to Black children. Older children were less likely to visit the hospital compared to younger children. The contextual variables also generally pointed in the expected direction, but none were significant.

For completeness, I also examined regressions where the outcome of interest was an indicator of any annual physician visit. These results are presented in the appendix (Tables C1-C3). As previously described this variable is highly problematic because it is missing for basically all of the period prior to Medicaid. The results suggest positive, but non-significant effects for children and mothers.

IV. Discussion

This chapter presented new evidence on the impact of Medicaid's introduction on hospitalization. Previous work from the 1970's was largely based on specific communities that may not have been representative of the country as a whole and no study has taken full advantage of the staggered state-by-year roll out of Medicaid's introduction. My models accounted for a number of relevant state-by-year controls, state fixed effects, year fixed effects, and region-by-year effects. The latter of these represent a relatively stringent test of the hypothesis as most of the plausible unobserved variation that could threaten a causal interpretation of my estimates arise from changes over time that differed across regions, but were fairly homogenous within regions (e.g. hospital desegregation). The lack of significant findings for placebo groups that were not in fact eligible for Medicaid further supports my findings. The results were consistent for both a binary treatment indicator and for an event study specification that did suggest some pre-trending, but it was constant for both the target and placebo groups.

The results from the income-group specific models suggest that Medicaid increased hospital utilization for the youngest low-income children by 3 percentage points and for all low-income children by 1.3 percentage points. The coefficient estimates suggest that the introduction of Medicaid erased the disparity between low and moderate income children. The AFDC regressions suggested larger utilization effects. This could be because the AFDC regressions predicted AFDC probabilities did a better job of isolating the treated population and thus the coefficients are a closer approximation of the effect of the treatment-on-the-treated.

The results from the income based stratification are similar to results presented by Currie and Gruber (1996b) who estimate a 3.9 percentage point increase in hospitalization (all aged children) as a result of Medicaid expansions that occurred in the 1980’s. My results for all age children were smaller, which is surprising because the population targeted by Medicaid’s introduction had fewer insurance alternatives compared to the expansion population. The target population at Medicaid’s introduction presumably had more to gain from public health insurance coverage. It is possible that they faced greater non-insurance related barriers to care.

I did not find any evidence in these data that mothers of child bearing age increased their hospital utilization as a result of Medicaid. One explanation for this finding is that the majority of hospital stays for women in this age range are for child birth and hospitalized child birth had already reached a saturation point by 1960. However, the fact that I did not find any evidence for an impact after removing mothers with children under 1 suggests there was no increase in the probability of non-child birth hospital stays.

Currie and Gruber (1996a) suggest that the 1980’s expansion led to increased access to intensive medical procedures and it is possible that Medicaid’s introduction improved access to neonatal intensive care units, which were rolling out in the 1960’s and 1970’s. It would be possible, in future work, to use additional NHIS data that describes the length of hospital stays and the conditions that were treated, to more fully examine utilization patterns for mothers and children.

In the context of the larger aims of this study, this chapter finds support for the hypothesis that Medicaid increased utilization of care, one of the proximate mechanisms that I suggest link Medicaid to longer run outcomes. One of the primary limitations of the 1963-1980 NHIS is that did not consistently track relevant measures of child health. I turn to that topic in Chapter 5.

CHAPTER 5. THE INTRODUCTION OF MEDICAID AND INFANT HEALTH

I. Introduction

My conceptual model suggests that Medicaid's long-term impact is partly mediated by short-run health improvements that persist over-time. There is now considerable evidence that childhood health, beginning *in utero,* influences adult health and economic outcomes (Currie & Almond, 2011). This chapter examines impact of Medicaid's introduction on infant health outcomes.

In the1980's Medicaid was decoupled from the Aid to Families with Dependent Children program (AFDC) and eligibility for pregnant women and children was slowly liberalized. One of the primary goals of the expansion was to increase prenatal care utilization among poor expectant mothers in the hope that it would improve infant health. Evaluations of Medicaid's expansion find only a modest impact of Medicaid eligibility on infant health. Results are not generally perceived to have met aspirations (Howell, 2001).

One plausible explanation for less than robust returns is that burdensome application requirements, borne by potential enrollees after they became pregnant, caused many women in the expansion population to delay obtaining coverage until late in pregnancy when prenatal care is thought to be less effective (Currie & Grogger, 2012). Indeed, Currie and Gruber (1996a) suggest that about half of the expansion population did not take up coverage during critical periods in early pregnancy. Results from correlational studies suggest that among deliveries ultimately financed by Medicaid, women that had Medicaid prior to pregnancy were 3 times more likely to initiate early prenatal care compared to women that obtained Medicaid during pregnancy (Rosenberg et al, 2002). In addition to the burdensome application costs that occurred at clinically sensitive periods, relatively high levels of crowd-out could have caused Medicaid to substitute for private insurance such that that there was no net increase in access to preventative care. Currie and Gruber (1996b) suggest that 68% of the children that were targeted by the expansions had other coverage.

This chapter presents new evidence on the effect of Medicaid on infant health. To do so, I leverage Medicaid's introduction which occurred at different times across the states, largely between 1966 and 1972. At Medicaid's introduction the majority of non-elderly enrollees were AFDC participants (Holahan, 1975). The target population faced no additional Medicaid application costs at the time of pregnancy. Furthermore, Medicaid's original target population consisted of very low-income women that likely had few health care alternatives and were from a highly disadvantaged socioeconomic stratum –a group that the medical literature suggests has the most to gain from prenatal care. The factors that separate the character of Medicaid's introduction from its expansion suggest that the introduction of Medicaid may have had a more pronounced effect on infant health than did the expansions of the 1980's.

The results of the analysis presented in this chapter contribute to our understanding of the impact of Medicaid on infant health, generally, and provide specific evidence on the conceptual model described in Chapter 2. The conceptual model suggests that the long-run effects of Medicaid run through short-term impacts on the utilization of medical services, health and financial security. In Chapter 4, I presented evidence from the National Health Interview Survey that suggests that introduction of Medicaid increased hospital use among children. In this chapter, I estimate the impact of Medicaid's adoption on birth weight, a well-studied measure of infant health.

II. Background

Prenatal Care

Historically there has been consensus in the medical community that prenatal care is an effective tool for promoting infant health. Prenatal care is thought to have a direct effect by identifying and treating maternal medical conditions, such as genital tract infections, gestational hypertension and gestational diabetes, that could jeopardize pregnancy (Kramer, 1987). Perhaps more significantly, prenatal care is thought to have a potentially large indirect effect by modifying known behavioral risk factors such as smoking, alcohol consumption, and nutrition. The behavior risk channel could be particularly important in the context of Medicaid's introduction because Medicaid was introduced shortly after the Surgeon General's landmark report on smoking in 1964. Access to prenatal care, driven by Medicaid, could have helped diffuse information on smoking's deleterious effects on infant health.

These views were summarized by a 1985 Institute of Medicine report (IOM) that motivated Medicaid expansion proposals (Howell, 2001). The report concluded, "…the overwhelming weight of the evidence is that prenatal care reduces low birth weight. This finding is strong enough to support a broad, national commitment to ensuring that all pregnant women… receive high quality-care" (IOM, 1985; p.146). The IOM suggested that the benefits of prenatal care were greatest for high-risk women, in terms of both medical history and socioeconomic position, and that early and sustained care was better than late or interment care.

The IOM's conclusions were supported by a review of the extant literature that found a consistent if not universal association between prenatal care and birth weight. Birth weight is not an ideal measure of infant health, but low birth weight (<2,500 grams) is strongly related to delivery costs, infant mortality, childhood disease, and long term health and economic outcomes (Almond et al, 2005; Almond & Currie, 2011).

The IOM report acknowledged that many studies that supported their claim inadequately controlled for selection bias. However, some studies they considered did rely on plausibly exogenous variation in prenatal care. For example, Rosenweig and Schultz (1983) use a 2SLS procedure that simultaneously models prenatal care use and outcomes. They find that delaying prenatal care is associated with a 7% decline in birth weight. Later work by Corman et al. (1987) using methods analogous to Rosenweig and Schultz (1983) suggests that expanded prenatal care was the driving cause for declining low birth weight and neonatal mortality between 1964 and 1977, second only to abortion.

Similar to Roswenweig and Schultz (1983), Corman et al. (1987) found that accounting for endogeneity increased the effect of prenatal care compared to naive OLS estimates. This suggests that selection bias into prenatal care is adverse—women at greater risks for complicated pregnancy are more likely to receive prenatal services. Admittedly, their identifying restrictions are not ideal and their results are not conclusive.

Others have speculated that selection runs in the other direction –that the absolute value of OLS estimates are biased upwards due to the selection of more health conscious (and healthy) mothers (Alexander & Korenbrot, 1995). Such reviewers have been more skeptical about the value of prenatal services (cf. Alexander & Korenbrot, 1995; Kramer, 1987). In a thorough review of the public health and clinical literatures on the determinants of birth weight, Kramer (1987) suggests that early prenatal care initiation is unlikely to have a causal impact. This view is often repeated in the economics literature (cf. Currie & Grogger, 2002; Almond et al, 2005). However, none of the studies in Kramer (1987) controlled for selection and it is plausible that they underestimated the true effects of prenatal care if selection is indeed adverse.

Two recent studies have found that properly modeling selection and heterogeneous treatments effects suggest results in a positive association of prenatal care and birth weight. Bell & Zimmerman (2005) find that naïve estimates tend to underestimate the effect of prenatal care. Conway & Deb (2005) find that prenatal care increases birth weights in otherwise ‘normal’ pregnancies, but not in complicated pregnancies. Their results suggest that failing to account for this heterogeneity will mask the effectiveness of early prenatal care.

Existing Evidence on Medicaid and Infant Health

Medicaid is predicted to improve infant health by reducing the cost of prenatal care services which in turn leads to utilization of prenatal services that would otherwise not be consumed. The program may also improve birth outcomes by increasing hospital revenues which facilitates the diffusion of superior technologies. The technology channel is more likely to improve measures such as fetal death or neonatal mortality by increasing the likelihood of survival regardless of health, not by improving underlying morbidity *per se*. It is also possible that Medicaid has an indirect effect by freeing up income that would be spent on prenatal services in the absence of Medicaid. Such resources could be diverted to other health promoting goods such as nutrition. Medicaid could also reduce stress during pregnancy by providing a mechanism for accessing desired but unaffordable services or by reducing the financial strain of consumed services. Stress during pregnancy is known to be associated with birth outcomes (Wadwa et al, 1993).

Much of what is known about the effect of Medicaid on infant health comes from evaluations of Medicaid's expansion experience. Traditionally, Medicaid eligibility was strongly tied to the Aid to Families with Dependent Children (AFDC) program. Beginning in 1984, Medicaid was gradually decoupled from AFDC and its means-test was liberalized for pregnant women. Between 1979 and 1991, eligibility for Medicaid among women who would have been eligible given a pregnancy, rose from 12.4% to 43.3%. Rising eligibility occurred at different rates across the states, providing a source of Medicaid variation across states and time.

Despite large increases in eligibility, the expansions had modest impacts on infant health. Currie and Gruber (1996a) suggest that a 30% increase in Medicaid eligibility reduced the incidence of low birth weight by 1.2%, in the general population. However, they find larger impact for earlier expansions that targeted lower income women: a 7.8% decline in the incidence of low birth weight. They suggest that the discrepancy is due to low take-up during the later expansions. However, it is also possible that crowd-out or the heterogeneous effect of prenatal care by income, played a role.

Other studies of that look more directly at Medicaid's target population have found larger effects. Long and Marquis (1998) find a significant reduction in the incidence of low birth weight by about half a percentage point among women in low-income Florida zip codes. However, most of the literature has found small and non-significant effects (Howell, 2001). Infant mortality, however, appears more sensitivity to Medicaid eligibility (Howell, 2001). Currie and Gruber (1996a) find that a 30% increase in eligibility 8.5% decline in infant mortality. This suggests that Medicaid expansions may not have improved health at birth, but improved survival of the sickest infants.

There are several potential reasons why expanding Medicaid eligibility to pregnant women would not improve infant health. Low provider fees discourage provider participation. Lack of accessible providers could reduce access to care. Medicaid beneficiaries could have little demand for prenatal care even when it is free. Or prenatal care could be limited in its ability to promote infant health.

Currie and Grogger (2002) offer an alternative explanation. Pregnant women made eligible for Medicaid through the expansions (i.e. non-AFDC enrollees) faced high application costs and may have delayed obtaining coverage until late in their pregnancies. For such women, the tangible costs of application, stigma, and other non-monetary barriers may exceed the perceived benefits of coverage. However, hospitals are mandated to serve women in labor regardless of their ability to pay and have developed large infrastructures to enroll women who show up for delivery (Currie and Grogger, 2002;

Cutler and Gruber, 1997). This suggests that Medicaid may finance a large share of births for which it has not financed prenatal services. Currie and Grogger's (2002) analysis suggested that reducing individual application barriers and presumptive eligibility policies seem to have little effect on increasing Medicaid caseloads or early prenatal care utilization. This suggests that these reforms were not sufficient enough to incentivize timely take-up and use of medical services.

Some studies find that women enrolled in Medicaid tend to delay prenatal care at greater rates than privately insured women (cf. Epstein & Newhouse, 1998). Certainly, low provider participation and the demographic profile of Medicaid enrollees suggests that they face non-insurance barriers. However, these studies define Medicaid status as enrollment on the day of delivery. Thus, it is plausible that the observed delay of prenatal care is partially related to delay in Medicaid enrollment.

Previous Evaluations of Medicaid's Introduction

The analysis in this chapter departs from most of the previous work on Medicaid and infant health in that I do not rely on the federal expansions of the 1980's. I estimate the impact of Medicaid on birth weight using the staggered timing of Medicaid's introduction across the states as a natural experiment (see Chapter 3 for a discussion of Medicaid's introduction).

There have been few other rigorous studies of Medicaid's introduction. Goodman-Bacon (2013) observes that the introduction of Medicaid reduced mortality for non-white children under age 15 by 24 percent, for groups that participated in the program (implied from intent-to-treat estimates that are scaled by participation rates). His study design relies on comparing the impact of Medicaid's introduction in states that had high versus low AFDC participation rates. He also includes "timing-group-by-year" fixed effects which limit comparisons within broad phases of Medicaid's adoption timeline.

The current analysis is a direct extension of Decker and Gruber (1993). Using a triple-difference strategy and data on individual births from the 1964-1967 National Natality Surveys (NNS) they find that the introduction of Medicaid was associated with large reductions to the incidence of low birth weight. They suggests that the introduction of Medicaid was associated with a striking 60 percent decline in low birth weight in the low-income population (<$3,000). They find no effect in higher income placebo groups, providing support for their study design.

I make several improvements on Decker and Gruber (1993). I follow the same basic study design, but in addition to the 1964-1967 NNS, I include the 1968-1969 and 1972 surveys. This allows me to track the effect of Medicaid over the full course of its implementation. I also include a fuller set of state by year controls that help me rule out a number of competing hypotheses that could explain the association of Medicaid's introduction and infant health. Unlike Decker, I also include region-by-year fixed effects which control for unobserved changes over-time within geographic region (North East, Midwest, South, and West).

III. Data and Methods

Data

The best source of information on birth weight is individual vital statistic records. However, micro-data from birth certificates do not exist prior to 1968. Most researchers investigating fertility and infant health in the pre-1968 period rely on aggregated data from vital statistics reports (cf Almond et al 2011; Bailey, 2012). However, these data lack the key variables in my analysis, namely income groups or detailed demographics with-in state. Instead, I use individual level data from 7 waves of National Natality Surveys (1964-69 and 1972). During this period all but one state adopted a Medicaid program.

The NNS were surveys of new mothers conducted by the National Center for Health Statistics (NCHS, 1966). The samples are drawn from birth certificates and the sampling strategy ensured sample coverage in each state. The files contain most of the data available on the certificate, including state of residence, birth weight and simple mother characteristics. In addition they contain a rich set of socio-demographic variables gathered from follow-up mail surveys. Depending on the year, the surveys obtain information on race, education, family income, fertility expectations, health insurance coverage, pre/postnatal care, and welfare income receipt. Unfortunately, many variables, such as health insurance coverage (available in 1964-1966), welfare receipt, and the timing of prenatal care initiation (both available in 1967), were not consistently measured in each wave. Even though these variables are unusable in the models described below, they are still somewhat instructive. For example, in 1964-1966 78% of low income mothers (<$3000) lacked private health insurance versus 17% of high income mothers (>$7000). Thirty-seven percent of low income mothers initiated prenatal care late or not at all compared to 12% of high income mothers. These data suggest that the introduction of Medicaid could have had a sizable effect on the availability of medical care in the low income population.

Birth Weight

I operationalize infant health using birth weight. I consider both the log of continuous birth weight measured in grams (I use logs in order to deal with a long left-tail and to be comparable to most other studies of continuous birth weight) and an indicator of weight less than 2500g (low birth weight). Due to limited sample size I am unable to measure lower thresholds of birth weight. I am also unable to consider other frequently used markers of health such as fetal death, or infant and neonatal mortality.

Income

I make particular use of family income which is gathered consistently in each year of data collection. The income data allow me to partition the sample into groups with higher and lower propensities to participate in Medicaid. I consider nominal income below $3000 as the high impact group and incomes between 5 and 10 thousand dollars as the low impact group. The low income group includes people that would be eligible for AFDC (and thus Medicaid) on the basis of income in the most generous states. Those in the moderate income group have a much smaller chance of being eligible for Medicaid. Due to the low probability of Medicaid participation in the moderate income group there is no theoretical reason to expect Medicaid's introduction to have an effect. Therefore, this group will reflect any secular changes that effected the population and occurred coincident to Medicaid's introduction.

The primary limitation of the income data in the NNS is that is collected in broad categories that make it difficult to inflate nominal dollars into real terms. In Chapter 4, I addressed this problem in the National Health Interview Survey by assigning specific income amounts through an imputation procedure that relied on income distributions observed in the March Current Population Survey. Unfortunately, a similar procedure is not possible in the NNS because the NNS is a survey of specific population (new mothers) that is not identifiable in the CPS. Prior to 1968, the CPS only included household members that were at least 14 years of age and it is impossible to identify women living with children under 1. The effect of using nominal dollars in this analysis is that over time, the low-income group represents a shrinking fraction of the population. In robustness tests I examined different methods for inflating incomes, such assigning each person the median value of their income bracket or assigning specific dollar amounts from women of child bearing age identified in the CPS. In each case I came to the same basic conclusions as those presented here, but because of the limitations of income assignment in the NNS, I prefer relying on nominal dollar amounts.

Medicaid Adoption (Treatment Variable)

Using state of residence, I merge the NNS with Medicaid adoption dates. Once the adoption dates are merged with the NNS I create a treatment indicator that is equal to

1 if Medicaid started in a child's birth state prior the child's month of birth. This variable varies by month, year, and state. It includes children born to women that were exposed to Medicaid for all or part of their pregnancies.[19]

Covariates

From the survey data I include a series of covariates that are known to be correlated with birth weight. Race is measured as White versus Non-White. Education is measured as high-school graduate or less. I also include a quadratic in mother's age and the mother's geographic region of residence (North East, Midwest, South, and West).

The state of residence identifiers also allow me to merge the NNS with a rich set of contextual data that describe the health care market and policy environment at the state by year level (see the appendix for a complete description of data sources). I observe the number of AFDC beneficiaries per 1000 population and the maximum AFDC benefit payment for a family of 3 expressed in real dollar terms. I include a measure of transfer spending through income-maintenance programs (public assistance, SSI, etc.) from BEA's Regional Economic Information System (REIS). The number of active physicians per 100,000 population comes from the Area Health Resource File and the number of short-term general hospitals per 100,000 population comes from American Hospital Association reports. I also include a series of variables that describe the availability of

[19] Early versions of this analysis attempted to measure the timing and duration of pregnancy using gestational age variables in the NNS. Gestational age would allow me to accurately model the duration and timing of Medicaid exposure over the course of pregnancy. However, this approach was abandoned because there is a large time-series break in the gestational age data. Prior to 1968 roughly 65% of the sample has 40 week pregnancy, after 1968 35% has a 40 week pregnancy. More recent data from the CDC suggests that between 30-35% of pregnancies last 40 weeks.

other public programs associated with the War on Poverty. Since these programs operated at the county level and I only consistently observe state, I parameterize them as the fraction of the state's population that lives in a county with an existing program. These variables describe the Food Stamp Program (from the Department of Agriculture), Maternal and Child health grants, job training grants, Community Health Centers or other heath programing, family planning, and Head Start grants (from the National Archives). Because the underlying data from the National Archives only includes the creation of a program (and not its cessation), I assume that once a program starts it never dies. The appendix includes a fuller description of the contextual controls.[20]

Sample Limitations

The major limitation of the NNS is that the sample was ostensibly restricted to "legitimate births". Given that the largest segment of the Medicaid population were AFDC participants, most of which were single-mother families, the NNS would appear to be the wrong choice. In states that collected marital status as part of the birth registration, NCHS used the administrative flag from the certificate to select legitimate births. In the remaining states illegitimacy was inferred by the absence of information about the father on the birth certificate (this occurred for 33% of sample births).

In practice the legitimacy screeners appear to have not fully excluded all illegitimate births. It is likely that when illegitimacy was reported on the birth certificate it was under reported. In states where inferences about legitimacy were made, the

[20] I am indebted to Doug Almond, Hillary Hoynes, Martha Baily, Andrew Goodman-Bacon, and Amy Finkelstein for providing these data.

decision rules likely lacked a high degree of sensitivity. Family composition variables in the NNS show that between 3.7% and 5% of births (depending on the year) were to woman-headed families. National Vital Statistics Reports suggest that between 6% and 9% of births during the same period were illegitimate. This suggests that the NNS is composed of an over-sample of married mothers, versus a selected sample of married mothers.

Table 5.1 presents additional evidence to support this claim. Table 5.1 compares the characteristics of mothers in the 1967 NNS to mothers from the 1970 Decennial Census long form, which collects more data than the regular Census form. I chose to focus on the 1967 file because it has a welfare indicator. Statistics from the Census data are reported for mothers with children age 1 or under, by marital status. Measurement differences notwithstanding, the table demonstrates that the weighted NNS sample looks more like the population of all mothers than the subpopulation of married mothers. The NNS means suggest that that sample closely approximates the total population of mothers in terms of race, education, family income, and receipt of cash-assistance welfare and is less representative of the subset of married women. In the NNS, 5% of all mothers report welfare income compared to 4.6% of all mothers in the Census and 1.6% of married mothers. Among low-income women in the NNS, 18% report welfare income compared to 24% of all low-income women in the Census and 5% of married mothers.

The NNS is not the ideal data set for studying the effect of Medicaid on birth outcomes—the composition of the sample works against finding an effect. However, Table 1 suggests that the NNS does not appear to fully exclude mothers that were the largest target of the Medicaid program. Furthermore, several states had AFDC Unemployed Parent programs which covered 2-parent families and a court decision in 1968 banned the states' use of "man-of-the-house" rules which prohibited AFDC benefits to women and their biological children when the mother was cohabitating or married to a man not biologically related to the children (Moffitt, 1998). While it is certainly true that AFDC participation was far higher single-woman households, the 1970 Census suggests that 33% of mothers reporting AFDC income were married.

Summary Statistics

Table 5.2 reports weighted summary statistics. I include only singleton births and I discard observations from Alaska, Hawaii, and Virginia because I do not have complete state-by-year contextual data for them. Nevada is dropped because there are no NNS observations in Nevada in 1965. Arizona is dropped because its Medicaid adoption date is clear outlier, however, including it has no material effect on the results presented here. Several state-by-year cells have small samples and either no low birth weight infants or all low birth weight infants, which causes perfect prediction in the model described below. To deal with that I group low-sample states that are geographically proximate and share similar adoption timing. I also present results that exclude these states.

The first column of the table includes information on the full sample of mothers observed in 1964-1969 and 1972 and the second column includes information on mothers that were either in the low (<$3000) or moderate income ($5-10k) groups. The later column represents that sample that is of primary interest—the target and placebo groups.

I observe a total 24,203 births. Mean birth weight is 3291 grams and 7% of the sample is low-birth weight. The low and moderate income groups have a slightly higher incidence of low birth weight. The low income group comprises the bottom 15% of the income distribution and the moderate income group comprises about 46%. Roughly 12% of births were to non-white mothers and 68% to mothers that completed high school. Roughly 48% of infants were exposed to Medicaid *in utero*.

Empirical Strategy

The staggered introduction of Medicaid provides variation in the availability of Medicaid across states at a point-in-time, and within states across time. The data also includes categorical family income measures that I use to proxy the propensity to participate in Medicaid. This provides a third source of identifying variation. I use this variation using a triple-difference framework (Imbens &Wooldridge 2007; Bertrand et al. 2004). The linear form of the model is described in equation 5.1.

$$(5.1) \qquad y_{ist} = \alpha + \beta_1 TREAT_{st} + \beta_2 P_{ist} + \beta_{12} TREAT_{st} * P_{ist} + \eta M_{ist} + \delta S_{st} + \lambda_{sp} + \phi_{tp} + \Psi_{rt} + \epsilon_{icst},$$

where y is a measure of birth weight (its log or an indicator of less than 2500g) for child *i* in born in state *s* on date *t*. TREAT is the measure of Medicaid availability described above. P is my income based proxy for propensity to participate in Medicaid. The sample is restricted to low or moderate income individuals and *P* is set to equal one for the low income sample. β_{12} captures the difference in the effect of TREAT across levels of P. This triple-difference strategy subtracts out any unobserved secular trend that is correlated with Medicaid adoption timing and is affecting both low and moderate income births. M is the set of mother characteristics including race, a quadratic in age, and an indicator of high school completion (high school or more). The purpose of these covariates is mainly to improve the precision of the model. The state fixed effects, λ_{sp}, and year fixed effects, ϕ_{tp}, control for any stable state characteristic or any stable period characteristic. They are interacted with P and this is written in the model using a p subscript.[21] S denotes a vector state by year characteristics described above that control for observable changes within state, over time. While this rich set of contextual data includes a large amount of potentially problematic coincident policy variation, I also include region by year fixed effects (Ψ_{rt}) which control for unobserved changes within region and over-time.[22]

NCHS cleaned and imputed each variable and provides sampling weights to correct for unequal probabilities of selection (NCHS, 1967). The log of continuous birth weight is modeled with OLS and the low birth weight indicator is modeled using logistic regression. I report the results of the logistic regression as average marginal effects (see Chapter 4 for a description of this method). Standard errors are clustered on state of birth.

[21] Estimating a version of equation 5.1 on just the low (moderate) income births, such that all the coefficients are allowed to vary by income, returns nearly the exact results as those presented here.
[22] The models in chapter 6 are estimates with state specific trends rather than region-by-year effects due to sample size constrains in the Panel Study of Income Dynamics. The results presented in this chapter are robust to replacing the region by year effects with a state specific trend in birth year.

IV. Results

Table 5.3 presents results from estimating equation 1. The Medicaid indicator is 1 if Medicaid was available before the month of birth and thus could have affected all or part of the pregnancy. The full set of model results are presented in the appendix (Table D1), but here I focus on the coefficients of direct interest. In the top panel (Panel A), the first column reports the effect of Medicaid on low-birth weight (<2,500g). In the moderate income sample the effect was small and non-significant. This supports the study design as very few moderate income families were eligible for Medicaid and any observed impact would likely reflect population wide trends correlated with the timing of Medicaid. The impact in the low-income sample is a 4 percentage point reduction in the incidence of low birth weight ($p<0.01$). The interaction term suggests that the relative impact of Medicaid for low-income infants compared to moderate income infants was a 4.4 percentage point decline ($p<0.01$). This represents a 45% decline in the incidence of low-birth weight from the base rate in the low-income sample of 9.8%.

The second column of the top panel summarizes results for the log of birth weight. For low and moderate income groups there was no evidence of an effect. Similar to other studies in the birth weight literature I found negative effects to the incidence of low birth weight, but much smaller and non-significant effects to continuous birth weight. This could result if the introduction of Medicaid prevented fetal deaths that ended up as relatively low weight births. Unfortunately, the data lacked information on fetal deaths and the sample size was not sufficient enough to examine distributional effects or lower birth weight thresholds.

The second two panels (B and C) summarize low-birth weight results from similar regressions on different samples. Panel B shows that removing the low-sample states has no meaningful impact on the effect, but as expected it is not estimated as precisely as the results in panel A. Panel B shows results from the restricting the sample to the 1964-1966 NNS. In the full data (Panel A) all states eventually adopt a program, in Panel C only half the states adopt (all in 1966). The difference-in-difference comparison is perhaps more intuitive given that half the states serve as controls. The results in panel C are larger, but qualitatively similar to those reported in Panel A.

The direction and statistical significance of the coefficients on the demographic covariates generally met expectations (See Table D1 in the appendix). Non-White infants were more likely to be born low-birth weight compared to White infants, the effect of mother's age was convex, and children born to higher educated mothers were less likely to be low birth weight compared to less educated mothers. None of the coefficients on the contextual controls were significant at traditional thresholds.

V. Discussion

The results in this chapter demonstrate that that the introduction of Medicaid was associated with substantively meaningful reductions in the incidence of low birth weight in the low income population. My study design relied on the gradual introduction of Medicaid across the states. The approach was strengthened by the inclusion of state-by-year contextual variables that captured changes in the health care market and public policy environment and by region-by-year fixed effects that captured all unobserved region specific factors changing over time. I demonstrated further support for the study design by showing that the pattern of results for low income infants had that had a relatively high propensity to participate in Medicaid did not hold for higher income infants that had a relatively low propensity to participate.

The data had important limitations. The sample was ostensibly restricted to legitimate births, a group that was largely excluded from Medicaid. However, Table 5.1 suggests that in practice the sample appears to have been an oversample of married women rather than an exclusive sample. Nonetheless, the composition of the data worked against finding an effect and our results likely represent a lower-bound. The NNS did not consistently collect information on the timing of prenatal care initiation so I was unable to examine the first stage utilization component of my conceptual model.

My results are smaller, but in line with those presented by Decker and Gruber (1993). I used more years of data and a more saturated model that provides a more robust test of Medicaid's impact. However, my results are much larger than estimates of Medicaid's expansion impact. There are at least 4 plausible reasons this. Our data allowed us to condition the sample on family income which is a relatively strong proxy for participation in Medicaid. It is possible that if authors examining Medicaid's expansion had access to individual data on income or other eligibility proxies they would have found similar results. Some authors have conditioned on other measures of SES such as education. However, education is a worse proxy of eligibility compared to income. Conditioning the sample on income could introduce endogeneity if women modified their incomes to gain access to Medicaid. However, Decker and Selk (2011) show that increases in AFDC caseloads during Medicaid's introduction were driven by take-up among the eligible-but-not-enrolled rather than by those changing their behavior to become eligible for Medicaid.

The second possibility is that the AFDC population was in a worse socioeconomic position compared to the expansion population. They could have had more to gain from public health insurance. The clinical literature suggests that the effect of prenatal care is related to socioeconomic status (IOM, 1985), perhaps due to underlying morbidity or health behaviors. The third possibility is that the poor of the late 1960's do not generalize to the poor of the 1980's. For example, Aizer and Stroud (2011), suggest that the diffusion of information about smoking's deleterious effects, which began prominently with a Surgeon General's report in 1964, varied by education. It is possible that some portion of the effect we observed was through smoking cessation initiated in prenatal care. Smoking is thought to be one of leading risk factors for prematurity and birth weight (Kramer, 1987). If smoking information had been widely diffused by the 1980's it is possible that the poor of the 1980's had less to gain from prenatal care compared to the poor of the late 1960's.

The final possibility, which holds the most policy relevance for current insurance reforms, is that women impacted by Medicaid's introduction initiated prenatal care earlier than women impacted by Medicaid's expansion. Women receiving Medicaid via AFDC participation did face application costs; however, the calculus for AFDC take-up was arguably different than Medicaid take-up. AFDC provides cash-in- hand—a tangible benefit that could have offset stigma and application costs. Furthermore, application costs were not necessarily incurred during pregnancy. The benefits of Medicaid (free prenatal care) could by itself appear too small and abstract to offset application costs. Such costs, in the expansion population, were incurred only after the pregnancy was discovered. Pregnancy is a time of increased stress when the marginal cost of application could be especially burdensome.

Prior to the Affordable Care Act, it was possible that Medicaid financed a portion of births for women that enroll near the end of their pregnancies or at the point of delivery. To that end, relatively low levels of Medicaid take up compared to the high share of deliveries financed by Medicaid (facilitated by robust hospital systems intended to enroll women that would otherwise receive self-funded or uncompensated delivery care) are suggestive. In such circumstances Medicaid finances deliveries for which it has not financed health promoting and cost reducing prenatal care. The Affordable Care Act (ACA) may disrupt this pattern. The ACA will extend Medicaid coverage to childless adults, reduce application complexity, and penalize uninsurance. This will increase Medicaid participation generally and could have a large effect on take-up among the previously eligible-but-not-enrolled. Evidence from similar reforms in Massachusetts suggest that take up among low income parents that were eligible prior to the reform increased by nearly 20% as a result of reform (Sonier, Boudreaux, & Blewett, 2013).

In the context of this project, the results presented in this chapter suggest that introduction of Medicaid had substantial short term health effects that could have plausibly translated into longer run impacts. Low birth weight has previously been linked to a range of long-run health and economic outcomes. Evidence in Chapter 4 suggests that Medicaid increased children’s use of medical care and thus may have improved child health, conditional on birth weight. Chapter 6 estimates the long-run impacts of exposure to Medicaid in childhood on health and economic outcomes.

CHAPTER 6. THE LONG-TERM IMPACTS OF EXPOSURE TO MEDICAID IN EARLY CHILDHOOD

I. Introduction

Poor health early in life can have long-term consequences, including reduced health and socioeconomic status in adulthood (Currie & Almond, 2011). Accordingly, recent research highlights the possibility that health investments in the early childhood period may yield long-run returns (Hoynes, Schanzenbach, and Almond 2012; Bharadwaj, Løken, and Neilson 2013). The primary goal of this project is to examine the long-term impacts of Medicaid, which provides health insurance coverage to low-income populations.

In theory, Medicaid could influence the long term outcomes of children by increasing access to effective medical interventions that improve childhood health in ways that persist into adulthood. Take-up of Medicaid also could reduce a family's inframarginal medical spending, freeing up resources that are subsequently directed towards other investments in children. Evaluations of Medicaid's short and medium-term effects suggest that for very low-income children Medicaid does in fact encourage consumption of medical services, improves health, and potentially reduces the risk of catastrophic out-of-pocket spending (Howell & Kenny 2012; Currie & Gruber 1996a,b; Currie, Decker, and Lin 2008; Sommers & Oellerich 2013). These outcomes are known to shape the evolution of health and socioeconomic status.

I add to this literature by studying Medicaid's long-term impact, leveraging the program's staggered adoption across the states in a generalized difference-in-differences design. Medicaid's adoption, which occurred mainly between 1966 and 1970, created meaningful and plausibly exogenous variation in early life exposure to the program for birth cohorts that are now in midlife. In Chapter 4 I found evidence in the National Health Interview Survey that Medicaid's introduction increased use of hospital services by low-income children. In Chapter 5 I found evidence in the National Natality Survey that Medicaid decreased the incidence of low-birth weight—a key measure of infant health that has previously been linked to long-run health and economic outcomes. Chapters 4 and 5 establish that Medicaid's introduction had an immediate and substantial impact on low-income children which could have plausibly translated into improvements in health and economic well-being across the life-course.

In this chapter I use the Panel Study of Income Dynamics to track the 1955-1980 birth cohorts from conception into adulthood (age 18-54). The longitudinal nature of the PSID allows me to focus on subgroups that vary in their propensity to have participated in Medicaid during early childhood. Using geographic identifiers I merge data from the PSID with Medicaid adoption dates and a detailed set of contextual controls that describe characteristics of the health care market and the availability of other public programs.

II. Methods

Data

Micro data come from the 1968-2009 Panel Study of Income Dynamics (PSID) (Panel Study of Income Dynamics 2012). The PSID is a nationally representative household panel survey that began in 1968 and annually follows participants and their descendants after they leave home and create their own households (data collection occurs biennially as of 1997). The original sample consists of about 5,000 households, including an oversample of low-income families that allows me to focus specifically on groups that had a high probability of participating in Medicaid. Interviewing was conducted in person using paper and pencil questionnaires from 1968-1973. Since 1973, the majority of interviews have been conducted over the phone (PSID, 2013).

The PSID maintains extraordinarily high wave-to-wave response rates (~98%), but given the length of the panel these small losses result in appreciable attrition over time. However, the PSID provides sample weights that adjust for initial selection and attrition and attrition bias to earnings, education, marriage and welfare receipt appears minimal (Gouskova et al. 2008. Fitzgerald et al. 1998).

I restrict the analytic sample to cohorts born between 1955 and 1980. This provides 6 cohorts that had no exposure to Medicaid prior to age 6, 10 cohorts that were exposed starting at conception, and 10 that were exposed starting at some point in the early childhood period. I include only observations that currently are heads or spouses (called ‘wives’ in PSID parlance) age 18 and over when assessing health outcomes. I restrict the sample to age 25 and over when assessing socioeconomic status (SES) to ensure completed education. The sample of interest includes only adults, but by utilizing the longitudinal nature of the PSID I observe their childhood characteristics. I drop observations whose first PSID interview occurred after age 13 because I cannot determine their early life covariates with confidence. I also remove children born in Arizona because its Medicaid start time is a clear outlier.[23]

Medicaid Exposure (Treatment Variable)

State of residence is available on the public use PSID. This allows me to identify the state of birth for each observation. For cohorts born prior to 1968 I infer state of birth from the family's residence in 1968 (the first year of interview). To reduce measurement error in state of birth I restrict the sample born prior to 1968 to those that did not move between birth and the 1968 interview, identified using retrospective mobility questions.[24] Using state of birth I merge on Medicaid adoption dates (measured to the year and month). The PSID also ascertains month of birth. Similar to Hoynes et al. 2013 and in the spirit of other work (e.g. Bleakley 2007), I measure Medicaid availability as the fraction of months exposed during early childhood. The early childhood period spans from the month of approximate conception to the month of the 6th birth day. To account for potential non-random migration the exposure variable is calculated based on state of birth and not the temporal dependent state of current residence.[25] The treatment measure varies as a function of the date and place of birth in reference to the state-specific implementation date of Medicaid. Because all states eventually adopted a program and

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[23] Less than 1% of the analytical sample was born in Arizona and this exclusion has a negligible effect on the results.

[24] In practice this step removes a very small fraction of the sample: 179 unique persons or 1.4% of people born prior to the 1968 interview.

[25] A weighted 6.1% of the analytical sample moved states during the early childhood period. This is less than Census Bureau's estimate of interstate migration for the population 5 years and older between 1965-1970 (8.6%). All results presented here are qualitatively robust to the removal of subjects that migrated states in the early childhood period. However, I prefer including these cases and calculating their exposure from their birth state of residence because it gives an intent-to-treatment estimate that is more realistic of the alternative policy choices. See below for the migration robustness results.

none repealed after implementation, a higher value of the Medicaid exposure measure represents having been exposed to Medicaid at a relatively earlier point in life.

Figure 6.1 plots the number of months exposed to Medicaid by birth cohort, as observed in the analytical sample. Starting in 1960, the average level of exposure increases by 5-10 months a year. By the 1972 cohort all sample members were exposed to a Medicaid program starting at conception. The error bars mark out 1 standard deviation above and below the mean to provide a sense of the intra-cohort variability in the data that is driven by state and month of birth. The graph demonstrates substantial variability in months of exposure during the early childhood period. In the 1962 birth cohort exposure to Medicaid averaged roughly 10 months and in the 1970 cohort averaged just less than 80 months. The staggered introduction of Medicaid across states also created within cohort variation. For example, roughly 20% of the 1965 cohort was exposed to Medicaid for less than 20 months in early childhood, 47% had was exposed for 20 to 60 months and 33% were exposed for longer than 60 months.

Adult Economic Outcomes

Since its inception, the PSID has data on socioeconomic and demographic characteristics. I use the socioeconomic data, measured during adulthood (age 25-54), to observe a set of economic outcomes including years of completed education (top coded at 17 years), the continuous ratio of family income to the federal poverty line and family wealth measured in 2000 year dollars.[26] To handle the skewed distribution of family wealth which includes zero and negative values I measure family wealth by categorizing

[26] Poverty is defined using the federal poverty levels suggested by Grieger et al. 2007.

the distribution into deciles. The average level of wealth in the lowest decile is $-33,172, $17,454 in the fifth decile and $1,074,344 in the tenth decile. Family wealth is consistently measured in the PSID starting in 1999 and therefore all models including family wealth pertain to the 1999-2009 data.

Adult Health Outcomes

Beginning in 1984 the PSID began asking family heads and their spouses to indicate their general health status (excellent, very good, good, fair, poor). In 1999, respondents began providing more detailed information on specific health conditions including hypertension, heart disease, heart attack, and diabetes. The PSID questionnaire ascertains whether the head or spouse was ever diagnosed with a condition and the age at first diagnosis. The survey also collects measures of self-reported height and weight that I use to construct body mass index (BMI) and an indicator of obesity (BMI≥30). Previous work suggests that these health outcomes are sensitive to early life conditions. (Gluckman, Hanson, & Beedle 2007; Currie & Almond 2011; Montez & Hayward 2011). Specifically, life-long disease risk is partially determined by exposures in the prenatal environment and/or through childhood health conditions that can lead to chronic inflammation responses. Medicaid could intervene in these processes by improving the prenatal environment through the provision of prenatal care, by preventing childhood illness through preventative care (e.g. vaccinations), and by effectively treating childhood disease when it occurs, thereby reducing the severity of long-run sequelae. The health data are coded to indicate undesirable outcomes (i.e. fair health or worse; ever being diagnosed with heart disease, etc.). Heart disease and heart attack are combined to accommodate their low prevalence. The diabetes indicator is set such that only cases that report an age of onset greater than or equal to 18 are considered to have the condition. This is done because diabetes is a relatively common childhood condition and it is impossible to determine if an association between exposure to Medicaid in childhood and childhood diabetes reflects improved detection or an effect to underlying disease.

Indexing

I summarize the individual health condition and economic outcomes by creating a chronic condition index and an economic index. By summarizing the individual outcome variables I gain statistical precision and reduce problems associated with multiple-comparisons (Andersen 2008). Following previous authors (Andersen 2008; Hoynes, Schanzenbach, & Almond 2013), the indexes are constructed as the equally-weighted average across each variable's z-score. The condition index includes only the chronic condition variables (high blood pressure, heart disease/heart attack, adult onset diabetes, and obesity) and is constructed such that increasing values indicate worse health (i.e. increasing prevalence of conditions). The economic index, including years of education, poverty level, and decile of family wealth, is constructed such that increasing values indicate a greater level of economic resources. The distribution of chronic condition index values by the number of chronic conditions gives some intuitive meaning to the health index: the average health index value for those without any condition is -0.4 and ranges to 3.1 for those with 4 conditions. A 1 standard deviation increase in the health index is associated with an increase in the number of chronic conditions by 1.4, on average.

Covariates

The models described below include a set of demographic controls pertaining to the adult period. I include race (White vs Non-White), age, gender, and current marital status. The longitudinal design of the PSID allows me to observe a set of childhood characteristics for each adult in the sample. These characteristics include individual, family, and community level factors (defined at the state or county level). I describe these data and their function in more detail below.

Summary Statistics

Table 6.1 presents descriptive statistics from the analytical sample consisting of adults age 18 and above (unless otherwise noted). The data are organized into person-year observations. There are 18,243 person-year observations with non-missing condition index values, representing 3,863 unique individuals. On average each person has a non-missing condition index for 3.6 years (out of a possible of 6; recall that the health condition data begin in 1999).[27] The average exposure to Medicaid, measured as the fraction of time exposed in early childhood, is 0.37. 46% of the sample is male and 83% is white. Roughly 22% had family incomes below 150% of the poverty line during early

[27] The average condition index is statistically indistinguishable for cases that have complete data versus cases that are have at least one year of data, but are missing 1 or more years (difference= .01; $p \leq 0.38$). The same holds for the Medicaid exposure variable and the economic index. Therefore, I assume that item missingness occurs completely at random.

childhood and 35% lived in families where the childhood head had less than a high school education. In adulthood, 7.4% of the sample reported being in fair health or worse; 14% reported ever being diagnosed with hypertension; 2.5% with heart disease or heart attack; and 24% met the threshold for obesity. The mean of the condition index was -0.13. At age 25 and later, the average number of years of education was 13.4, the average income to poverty ratio was 4.6 and the average wealth decile value was 6.1. The mean of the economic index was 0.19.

Empirical Strategy

To identify the effect of Medicaid's long term impact I regress a given health or economic outcome on the fraction of months a person was exposed to the Medicaid program during early childhood. The model takes the form described in equation 6.1:

$$(6.1) \quad y_{insct} = \lambda MCAIDSHARE_{st} + \beta X_{inst} + \theta Z_{sct}\,\rho_n + \delta_t + \gamma_s + (\gamma_s * t) + e_{insct} ,$$

where y_{insct} is a health or economic outcome, for person *i* at adult interview year *n*, born in state *s* and county *c* in year *t*. MCAIDSHARE is the continuous measure of Medicaid availability in the early childhood period and λ measures the effect of moving from no exposure to full exposure (i.e. MCAIDSHARE increasing from 0 to 1). Because the Medicaid programs were never repealed, every person in the 1955-1980 cohorts was exposed to Medicaid at some point in their lives (albeit perhaps not until after the early childhood period). As a result of this feature of the policy experiment, the coefficient on the exposure measure captures the effect of a marginal increase in exposure earlier in life.[28] Therefore, it includes the effects of both duration and timing.

X_{insct} is a set of individual level controls, including gender, race (white vs. other), a quadratic in age, and marital status.[29] These variables control for compositional differences correlated with Medicaid exposure and will improve the precision of the model. The model includes interview year fixed effects (ρ_n), year of birth fixed effects (δ_t) and state of birth fixed effects (γ_s). These hold constant any stable, but unobserved period or state effect. ($\gamma_s * t$) is a state specific linear trend in birth cohort that captures changes over time within state of birth (linear trends are used rather than unrestricted state-by-year or region-by-year effects because of sample size constraints). Equation (5.1) represents a generalized difference-in-difference estimator (Imbens & Wooldridge 2007; Bertrand et al. 2004) that captures the effect of Medicaid policy, rather than Medicaid participation specifically. In all analyses the PSID weights are used to adjust for the initial probability of selection and attrition over time (Gouskova et al. 2008). Standard errors are clustered on state of birth (Bertrand et al, 2004). Models using continuous outcomes are estimated with OLS and regressions of binary outcomes use linear probability models.

The eligibility criteria of the Medicaid program provide an additional dimension of variation to help identify the effect of exposure to Medicaid in early childhood. I

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[28] Take, for example, a person with 45 months of exposure versus a person with 81 months of exposure. The former person's exposure to Medicaid began on their third birthday (assuming 9 months of gestation) whereas the later was exposed since conception.

[29] I was concerned about the collinearity of age given the presence of interview year and birth year fixed effects. However, results are robust to measuring age in categories or removing age entirely from the model. Results are also robust to excluding current marital status which is potentially endogenous.

estimate equation (6.1) on sub-groups that vary in their propensity to participate during early childhood. Statistically significant effects in groups with high participation and null effects in placebo groups with low participation will support the claim that adoption timing was not correlated with other secular trends, such as medical technology diffusion or changes in social and economic environments that affected the entire population. I present results for 2 high impact groups: 1) a low income group defined as adults that had family incomes less than 150% of the poverty level, on average, during early childhood[30]; and 2) a low education group defined as adults that grew up in families where the head had less than a high school education. I examine multiple subgroups because there are several trade-offs in choosing the appropriate target population. The low income group is likely a better approximation of the group eligible for new Medicaid benefits due to AFDC—the PSID suggests that about 40% of this group participated in AFDC at some point in their childhoods. However, it has a smaller estimation sample which may limit the precision of λ, holding model fit constant. The income group is also more prone to selection bias as income is easier to manipulate over the short-run than is education. The disadvantage of the low education group is that it does not home in on the affected group as precisely, which can result in a weaker approximation of the effect of Medicaid on those who actually participated. Approximately 22% of this group participated in AFDC at some point in childhood. The PSID has also tracked AFDC

[30] 150% of the federal poverty line roughly translates to the un-weighted average of "gross" income eligibility limits for a 3 person family that prevailed across the states in 1970. Thus, this group approximates the group of income eligible. The early childhood period for the purpose of defining the low income group runs from conception through age 5 for those conceived in 1968 or later and 1968-1972 for those conceived prior to 1968. Forcing all cohorts to have the 1968-1972 childhood period has no qualitative effect on the results.

participation since 1968, but I chose not to examine AFDC participants directly because AFDC participation decisions are clearly endogenous and because AFDC participation increased after the introduction of Medicaid (Decker & Selk, 2011) and related compositional biases would emerge from using this subgroup. Below I also estimate triple-difference specification that approximates the effect of the treatment-on-the-treated without relying directly on indicators of AFDC participation.

I run placebo tests on two subgroups that have a smaller implicit probability of being effected by Medicaid: 1) observations whose average childhood poverty level fell between 175 and 300% of the poverty line (referred to as moderate income); and 2) observations born to household heads with greater than a high school education (referred to as high education). The lack of appreciably sized and significant coefficients in the placebo samples will provide evidence for the lack of omitted variables that affected the entire population.

It is possible that some unobserved factor that improved the life chances of low SES children, but not higher SES children, is correlated with Medicaid adoption time. The period of Medicaid's implementation was characterized by an explosion in public assistance programs. The Food Stamp Program, Head Start, and Community Health Centers were all rolling out around the same time as Medicaid's introduction. If these programs have long-term impacts that are concentrated in the high impact subgroups then the effect of Medicaid could be overstated and null results in the high SES groups would not illuminate the problem.

To account for that possibility I merge to the sample a detailed set of contextual controls that are linked by state or county of birth.[31] At the county level I observe the number of per capita doctors and short-term general hospitals, real per capita spending on public assistance (e.g. AFDC, general assistance, food stamps, and SSI), and whether the county of birth had one of several War on Poverty era programs that was implemented at the county level (Food Stamps, Head Start, Community Health Centers, Family Planning, Maternal and Child Health grants, other health related programming, and job training grants).[32] The health market and public expenditure data are measured as the average level in the early childhood period and the program variables are specified as the fraction of months exposed during early childhood (the exception is family planning which is specified as an indicator if a program existed at the time of conception). At the state level I observe unemployment, real AFDC benefit standards and per capita AFDC caseloads, all measured as the average in the early childhood period. I also include an indicator if the adult was conceived in a state and year with legalized abortion. County-level covariate data were not available for Alaska, Hawaii, the District of Columbia, and Virginia, and that portion of the sample was dropped (n=3,068; 4.4%).

All models control for the full set of contextual controls (called Z_{sct} in equation (1)). These controls give additional leverage, beyond the inclusion of state specific linear cohort trends, for controlling for policy changes that were coincident with Medicaid's introduction.

[31] County of residence is a restricted use variable which I obtained through special arrangement with the University of Michigan.

[32] I am indebted to Doug Almond, Hillary Hoynes, Amy Finkelstein, Martha Bailey and Andrew Goodman-Bacon for providing and assisting us with these contextual data. The online appendix provides a fuller description of each source.

III. Results

In Table 6.2 I report the coefficients on MCAIDSHARE in the health outcome models for each of the 4 subgroups described above.[33] Each set of results come from models that control for the full set of demographic controls, contextual controls, and fixed effects. The top panel includes results for the condition index and the bottom panel describes results for self-reported health status and the 4 chronic condition indicators that constitute the index (ever being told that you have high blood pressure, heart disease or heart attack, diabetes after the age of 18, and meeting the BMI threshold for obesity).

In the low income group, the model implies that the effect of moving from no exposure to full exposure is a 0.36 standard deviation reduction in the condition index (indicating improved health). The point estimate is statistically significant at the 5% level. In the low-education group, the effect is about half the size (-0.18), but not statistically significant. Scaling the coefficients in the low-income and low-education by the relative AFDC participation rates groups suggest nearly the same estimate of the treatment on the treated (approximately -0.8). Looking at the specific health measures, in both high-impact groups the results for the indicator of fair health or worse are small and non-significant. The specific condition indicators are all negative as expected. However, I am only able to detect a significant effect to high blood pressure. The coefficient in the low income group implies that that full exposure to Medicaid in early childhood, versus its absence, is associated with a 23 percentage point decline in prevalence, significant at the 5% level.

[33] The full set of results is available in the appendix.

The right hand panel of Table 6.2 describes model results in the low impact (placebo) samples. In both the moderate income and high education group, for all outcomes considered, the coefficients are small and not statistically different from zero. For example, the coefficient for the condition index in the moderate income group is a 0.05, which is different from the impact in the low-income group at the .05 confidence level. The pattern of results in the placebo groups suggest that the health impacts observed in the high impact groups are not being driven by unmeasured underlying trends correlated with adoption timing.

Table 6.3 presents outcomes for the economic outcomes. The table is organized in the same fashion as Table 6.2. The outcomes pertain to adults age 25 and over and include the economic index, years of education, the ratio of family income to the poverty line, and the decile of family wealth. Regardless of the impact group or the outcome measure there is no evidence that exposure to Medicaid in childhood improves adult economic status. For example, the coefficient for the economic index in the low income group is negative (suggesting worse economic outcomes), but not significant. In the placebo groups the effect of Medicaid's introduction on the economic index is also negative and in the case high education group the coefficient is significant at the 10% level ($p \leq 0.083$). For all groups, the coefficients are estimated imprecisely and I am unable to rule out a large range of potentially meaningful effect sizes. For example, the 95% confidence interval for the low-income group ranges from -0.5 to 0.3. The economic outcomes that constitute the index are all continuous measures which helps ensure that I capture the full continuum of socioeconomic status. However, the results presented here are robust to using dichotomous indicators of relatively extreme deprivation like below the poverty line or less than a high school education. One potentially important indicator, work-status, has been specifically left out of the index because work status itself not unambiguously positive. However, inclusion of employment does not alter the results presented here. Unfortunately, the PSID lacks consistently measured occupation categories that would also be a valuable outcome of interest.

Triple Differences

In Table 6.4, I focus more squarely on groups targeted by the program and formalize the comparison of treatment effects across the impact groups. To do so I use a triple difference approach run on the full sample of adults. Following the procedure suggested by Bleakley (2007), Hoynes and Szchanzenbach (2009), and Hoynes Schanzenback and Almond (2013), I modify equation (6.1) by interacting MCAIDSHARE and the predicted probability of being enrolled in Medicaid in early childhood. Medicaid participation probabilities (called PRATE in Table 5.4) come from the 1977-78 PSID and are simple means defined within 24 demographic groups.[34] The groups consist of fully cross-classified characteristics of the family head: age (3 levels), race (2 levels), marriage (2 levels), and education (2 levels). The participation rate groups were chosen to be characteristics that people have little control over in the short-run.[35] The rates are merged back to the analytical sample according to each adult's family

[34] The PSID began collecting information on health insurance in 1977. The percentage of the non-elderly population estimated to have Medicaid by the 1977-1978 PSID is 5.6%, roughly the average of the 1976 and 1978 rate published by the National Center for Health Statistics (Cohen et al. 2009).
[35] The potential exception to this is marriage.

background characteristics defined at birth or early life. The average predicted participation probability is 0.05 and it ranges from 0.01 to 0.44. The participation rate variable captures a higher degree of variation in Medicaid participation compared to the groups examined in the previous section. Particularly, the inclusion of marital status, which was a prominent feature of AFDC eligibility, provides a much closer look at groups that were targeted by Medicaid's introduction. By leveraging variables like marriage through the continuous probability measure I gain statistical power which would be lost had I simply restricted the sample to the small number of children born to unmarried mothers.[36]

The triple difference models include the same set of demographic controls, fixed effects and contextual controls as described above, in addition to the interactions of PRATE with survey and birth year. The main effect for MCAIDSHARE captures the effect of Medicaid exposure for groups that are likely not to have participated in Medicaid (i.e. when PRATE=0). Table 6.2 suggests that the main effect for MCAIDSHARE on the condition index ought to be in the neighborhood of zero (the result observed in the placebo groups). The interaction captures the relative effect of exposure to Medicaid for groups that were likely to participate (i.e. when PRATE approaches 1).

In the top panel of Table 6.4, I present results from triple difference models of the chronic condition and economic index. As expected, the main effects for MCAIDSHARE for both outcomes were small and non-significant. In the condition index model, the

[36] It should be noted that the predicted probability of AFDC is a generated repressor that has its own sampling error, which is not fully accounted for here on in the NHIS analysis presented in chapter 4. Not accounting for this error should tend to understate the size of standard errors.

interaction term was -0.88 and significant at the 10% level (p=0.054). This maps closely to the implied estimate of the treatment-on-the-treated reported in Table 6.2. The coefficient suggests that for groups that were likely to have participated in Medicaid, exposure in early childhood was associated with a meaningful level of improvement in adult heath. In the economic index models the interaction was relatively small, -0.07, and not statistically significant.

In the bottom panel of Table 6.4 I present results from the same model after removing the contextual controls. If the coefficients are extremely sensitive to this decision it would suggest that there may be other unobserved factors that were changing according to the same pattern of Medicaid's introduction and biasing results. However, the point estimates are largely unchanged. The condition index model without contextual controls suggests that effect of Medicaid for those likely to have participated was a 0.99 standard deviation improvement in the condition index, significant at the 5% level. The results in Tables 6.2 and 6.3 are also robust to the exclusion of the contextual controls (results not shown). For example, the coefficient in the condition index regression for the low income sample is -0.42, significant at the 5% level.

The demographic covariates generally had the expected sign (see Tables E1-E11 in the appendix). For example, in the triple difference models age and white race was negatively correlated with the condition index. The contextual controls were generally non-significant, however, there were some acceptations. Children born in counties with a Family Planning program were in better health than those in counties without programs and the level of public assistance spending in a county was positively associated with the condition index. Given that the study design was specifically geared towards identifying the causal impact of the other public programs it likely that the coefficients on the other public program variables do not have a causal interpretation.

IV. Robustness

A. The role of Desegregation

I control for a number of relevant coincident policy changes, but like all natural experiments, a limitation is the possibility that the effect of some unobserved variable was absorbed in the coefficient of interest. School and hospital desegregation created important institutional shifts around the time Medicaid was introduced and I lacked specific data on the roll-out of desegregation. The models' inclusion of state-specific time trends will mitigate any potential bias that might otherwise potentially emerge from these unobserved variables. To get a sense of whether desegregation is likely to bias or otherwise explain the results, I ran a robustness check that dropped southern-born non-whites from the sample, as that is the group most likely to be affected by desegregation. My estimates, summarized in Table 6.5, were basically unchanged, but were estimated with less precision as expected from the loss of sample. The impact of Medicaid in the low-income sample was a 0.37 reduction in the health index, significant at 0.1 level. That results in Table 6.5 increase my confidence that the results reflect the effect of the Medicaid program and not the impact of desegregation.

B. Selective Migration

Another source of potential bias is selective migration. If families with relatively "good" parents moved from a late adopting state to an early adopting state to gain access to Medicaid, then the estimates presented above could be biased upwards. The direction of bias would be in the opposite direction if families with relatively sick children moved. However, there is very weak evidence in the existing literature that people move to gain access to cash-welfare benefits (Berry et al 2003) and recent evidence from the expansion of Medicaid to non-elderly adults suggests a similar pattern in the Medicaid program (Schwartz et al 2014). Nonetheless, it is possible Medicaid did incentivize interstate migration during its introduction or that secular migration patterns happened to be correlated with the Medicaid adoption schedule. In Table 6.6 I examine that hypothesis by re-estimating the models after removing observations that change state of residence during the early childhood period. The stratified models (by income and education) presented in the top panel of the table suggest a substantial loss of precision and the estimates are no longer significant. However, the point estimates are roughly the same magnitude. The results in the triple-difference models (using the AFDC predicted probability modifier) are nearly identical to those presented in Table 6.4: a .89 reduction in the condition index, significant at .05 confidence level.

C. Repeated Observations

A potential criticism of my estimation of equation (6.1) is that I do not directly account for the fact that I have repeated measurements on the same individuals. All results, thus far, have used cluster-robust standard errors (clustered on state of birth). In theory, because individuals are nested with in states, this should account for the clustering within person. However, in Table 6.7, I account for repeated measures more directly by estimating equation (6.1) using a generalized estimating equation (GEE). The GEE is specified with a normal distribution, an identity link, and an exchangeable correlation structure (assumes constant correlation over time). Standard errors are estimated with the sandwich estimator (Huber-White) which accounts for correlation within person. This GEE model is preferred to other panel models because it estimates population-averaged effects, rather than within cluster differences. For brevity, I report just the coefficient on MCAIDSHARE in the low-income sample. The first row summarizes results when from the base OLS model and the second row corresponds to the GEE estimates. The precision is slightly worse (p=.052), but not meaningfully different. In other robustness checks, I also estimated GEE with an unstructured correlation structure, OLS models with standard errors clustered on person and models fit on data that was collapsed to the person level (taking the maximum index scores). Overall, all alternative variance estimation approaches suggested p-values between the .03 and .08. Given relatively small sample sizes, lack of precision in some models is not surprising.

D. Alternative Predicted AFDC Probabilities

Table 6.8 summarizes results from alternative specifications of the triple-difference models described in Table 6.4. A limitation of the approach presented in Table

6.4 is that the AFDC probabilities were obtained from 1977-1978 and did not reflect AFDC participation rates at the time of Medicaid's introduction. In Table 6.8, the AFDC probabilities are obtained from 3 alternative datasets that cover different periods. Each alternative predicted probability is defined over the same strata (the age, race, marital status, and education of the adult's household head in childhood). The first column reproduces the results from Table 6.4, based on the 1977-1978 PSID. The predicted probabilities in the second column are from the 1966-1967 CPS and reflect the receipt of any welfare income. The interaction term suggests that the relative impact of Medicaid exposure is a 2.3 standard deviation reduction in the condition index ($p<0.05$) and a non-significant effect to the economic index. The March CPS underwent a major measurement and sample design change after 1967 that resulted in a substantial change in estimated welfare income rates. In the third column, I report results based on the 1968-1969 CPS. The second to last row of the table indicates that maximum predicted AFDC rate increased from 0.25 in 1966-67 to .46 in 1968-69, which is likely due to the measurement change. The interaction term based on the 1968-1969 CPS AFDC data is suggests that the relative impact of Medicaid exposure is a 0.88 standard deviation reduction ($p<0.1$) for the condition index and non-significant for the economic index. A limitation of both CPS periods is relatively small samples (the smallest cell size is 67). The final column presents results from obtaining the AFDC participation data from the 1970 Census Long Form (the smallest cell size is 1,112). The interaction term suggests the effect of Medicaid exposure for those with a predicted probability of 1 versus those with a predicted probability of 0 is a 1.1 standard deviation reduction in the condition index ($p<0.1$) and there is no statistically detectable effect on the economic index. A key advantage of the 1970 data is that its large sample size minimizes bias to the standard error which is biased downward because the model does not account for the fact that the predicted AFDC probabilities are generated repressors.

Table 6.8 demonstrates that main findings reported in Table 6.4 are robust to the source of information on AFDC participation (in terms of dataset and year). The clear outlier is the results based on the 1966-67 CPS. The relatively high rates from the 1966-1968 data could be caused by the fact that the estimate lacks substantial support in the data given the relatively restricted range of the AFDC probabilities.

V. Discussion

In this chapter I present the first estimates of the long term effect of exposure to Medicaid in early childhood. I observe that exposure to the Medicaid program through the duration of early childhood, compared to its absence, improves long term health. My identification strategy relied on Medicaid's staggered introduction across the states—a plausibly exogenous source of Medicaid availability. The main specifications summarized in Tables 6.2-6.4, suggest the effect in groups targeted by the program ranges from a 0.36 to a 0.9 standard deviation improvement in a composite index of health conditions, after controlling for a large set of contextual controls, state fixed effect, year of birth fixed effects, and state specific trends in birth cohort. The absence of effects in placebo groups provides additional support for the study design.

The 0.9 standard deviation reduction in the condition index that I observe in Table 6.4 implies a reduction of approximately 1 chronic condition measured by the index. I considered four measures in the condition index: high blood pressure, adult onset diabetes, heart disease/heart attack, and obesity. Table 6.2 suggests that high blood pressure is particularly sensitive. This is not surprising given the average age of the sample is 37 and the prevalence of the other conditions (save obesity) is generally low at that age. Recent work examining the long-run health impacts of a randomized intensive early childhood intervention also found substantial effects to high-blood pressure when the cohort had reached their mid-30's (their treatment effect was twice the size of mine). Similar to our findings, they found non-significant effects to measures of diabetes and obesity (Campbell et al. 2013). All of the health conditions I considered have been linked to early life health, are highly prevalent in the U.S., and consume a large amount of health care resources. Results suggest that improving access to health insurance for low income children and pregnant women can improve individual well-being into adulthood while potentially creating savings in downstream medical costs.

I found no statistical evidence that Medicaid had a long-term economic impact. However, the point estimates were imprecisely estimated and I could not exclude a large range of potentially meaningful effect sizes. The economic index combined data on years of education, family-income, and family-wealth. I chose to exclude measures of labor-force participation or employment because choosing to remain out of the labor force does not necessarily signal reduced economic well-being. However, in robustness tests I found very similar effects when including measures of employment. While the results are inconclusive they do align with Royer (2009) who found relatively small impacts of low birth weight on the educational outcomes of a cohort of U.S. twins, but relatively large effects on measures of adult health.

The results were relatively robust to alternative specifications. Statistical inference was robust to alternative methods for accounting for repeated measures., but it should be acknowledged that the results were often marginally significant and future research on larger samples is needed to confirm these results. The triple-difference models were not entirely robust to how I measured predicted AFDC participation. However, the results in Table 6.4 are superior because they provided much more granular measurement of the target population while being based on arguably exogenous sources of variation. The alternative method described in Table 6.8 was based on a broad classification of states into high versus low AFDC participation. While the results generally pointed in the same direction, they were not statistically significant.

The early origins literature suggests that the earliest years of life are critical for development (Almond & Currie, 2011; Currie, 2011). Understanding the contribution of policy exposures at specific critical periods versus marginal increase in duration (regardless of age) is important because it tells how to distribute public resources across the lifecycle. The nature of the variation in the intervention in this study, which produced an increase in Medicaid exposure occurring earlier in life, suggests that earlier exposure is perhaps beneficial to long term health. However, the study design cannot separate the effects of timing from duration. I never observe a child that had exposure in early childhood, but not later childhood because the Medicaid programs were never repealed once they started. Hoynes et al (2013) finds that while exposure to food stamps from ages 0-5 improve long-term health and economic status, additional exposure beyond age 5 does little to improve long term outcomes. Unlike Hoynes et al (2013) study which relied on county level implementation that implies appreciable variation in age at first exposure, this study relied on state level variation and therefore exposure to age 6 was highly collinear with exposure after age 6. Event study estimates, which characterize the intervention according to age at first exposure, also could be compelling, but because the timing of intervention determines duration over a given age range, event study estimates would provide little additional information beyond the fraction of time measure used in this study.[37]

I can only speculate on the exact mechanisms that link Medicaid's introduction with health gains later life. Outcomes could have been driven by improved childhood health or improved economic resources in childhood. Improvements in childhood health seem the most plausible driver because I examined a relatively deprived population that likely had few medical expenses in the absence of Medicaid (i.e. low infra-marginal spending). The results in Chapters 4 and 5 lend support to that hypothesis. However, I cannot determine which exact health services delivered at what specific developmental period were the most effective.

The introduction of Medicaid is not perfectly generalizable to the contemporary program. At its inception Medicaid targeted a lower-income group than it now covers and it is reasonable to expect that the low-income population of the late 1960's differed in

[37] It should be noted that an event study specification would allow for non-linear effects, but it would not necessarily indicate whether duration or timing of exposure was more (less) important.

substantial ways from the low-income population of today. However, the primary hypothesized mechanism linking Medicaid in childhood to later life outcomes – decreasing financial barriers to medical care that returns immediate health gains that persist throughout the lifecycle – is pertinent in the contemporary context. Despite considerable expansions in both private and public coverage since Medicaid's inception, nearly 5.5 million children under the age 18 lack health insurance coverage. Furthermore, the large share currently covered by Medicaid (35%) suggests that in the absence of Medicaid access to health insurance would decline, issues of crowd-out notwithstanding. This chapter suggests that a full accounting of Medicaid expansions and other policies that aim to increase health insurance coverage, such as the Patient Protection and Affordable Care Act, should consider downstream benefits that accrue decades in the future.

CHAPTER 7. CONCLUSION

I. Introduction

This project was inspired by a large body of evidence that shows that poor health and adverse environments in early childhood have consequences that span the life-course. However, disadvantage in early life can be overcome. An emerging literature shows that pediatric health services and comprehensive, intensive early childhood interventions can have a dramatic influence on later-life outcomes, especially for low-income children. The benefits of high quality investment in disadvantaged children include improved adult health and reduced health risk behaviors, improved educational attainment, higher wages, and reduced criminal activity. The literature suggests that the benefit-to-cost ratio is over 8 to 1 for comprehensive programs that are delivered to disadvantaged populations during the earliest ages of life (Heckman, 2006).

I examined the long-term impacts of Medicaid—the largest provider of public health insurance for low-income mothers and children. The conceptual model described in Chapter 2 suggests that Medicaid improves access to medical care and increases the financial resources of families. In turn, short-run improvements in the critical periods of early development can have lasting influences across the life-course.

I faced two major empirical challenges. The first is that Medicaid is a voluntary program and the reasons that a family chooses to enroll their child were not observed. To solve that problem I focused on intent-to-treatment estimates that measured the impact of exposure to Medicaid eligibility rather than the impact of participation. The second major empirical challenge I faced was that I needed a long follow-up period. Existing literature suggests that the impact of early life health can remain latent until well into adulthood. This limited the set of potential policy experiments that could be used as a source of variation in Medicaid eligibility. My solution was focus on Medicaid's introduction, which occurred at different times across the states and created variation in cumulative exposure to Medicaid for cohorts that are now in mid-life.

To examine my conceptual model, in the context of Medicaid's introduction, Chapter 1 set out 3 principle aims:

1. Estimate the impact of Medicaid's introduction on the utilization of health care by women of childbearing age and children.
2. Estimate the impact of Medicaid's introduction on the health of young children.
3. Measure the long-term impact of exposure to Medicaid in childhood on health and socioeconomic outcomes.

II. Summary of Empirical Findings

Chapter 4 examined the impact of Medicaid's introduction on hospital utilization using data from the 1963-1980 restricted use National Health Interview Survey (NHIS). My preferred estimates suggest that Medicaid increased the rate of any annual hospital visit, for the youngest children, by 3 percentage points—a 51% increase from the base rate. The results from Chapter 4 suggest that introduction of Medicaid did indeed increase utilization of medical services. Recent work suggests that hospital based services for young children have long-run effects (Bharadwaj, Løken & Neilson 2013). Increases in hospital utilization are one plausible mechanism for the results described in Chapter 6 (see for example, Bharadwaj, Løken, and Neilson 2013).

Another plausible avenue for the Medicaid's long run effects is through improving infant health. The large body of evidence reviewed in Chapter 2 suggests that infant health, measured by birth weight, has an impact of health and socioeconomic status in adulthood. Medicaid could improve infant health by increasing the use of prenatal care or by improving the inter-conception health of the mother. Previous work on Medicaid expansions have shown modestly sized impacts on birth weight and larger effects on infant mortality. It is plausible the Medicaid's introduction had a larger impact because take-up was better, crowd-out was smaller, and the original target population had a lower socioeconomic profile compared to the expansion populations. The medical literature suggests that the effectiveness of prenatal care is correlated with economic status. In Chapter 5, I extended previous research using data from the 1964-1969 and 1972 National Natality Survey (NNS) and found that introduction of Medicaid decreased the incidence of low birth weight by 4 percentage points in the low-income population –a 42% decline from the base rate. These relatively large effects suggest that Medicaid substantially improved health at birth.

Chapter 6 described the primary analysis of interest. Using variation in cumulative exposure to Medicaid from birth to age 6 and data from the Panel Study of Income Dynamics (PSID) I found that Medicaid reduced a composite measure of chronic conditions in adulthood (age 18-54) by .3 to .9 standard deviations, depending on the model. The result appeared to be largely driven by reductions in the prevalence of high blood pressure, a measure that has known to be sensitive to early life health and early childhood interventions. The estimates for economic outcomes were imprecisely estimated and the results were inconclusive.

III. Limitations

Each analysis had specific limitations related to available data. The NHIS lacked consistently measured variables on out-patient physician utilization which required me to limit my analysis to hospital visits—a measure that confounds morbidity and access. The NNS sample was ostensibly based on legitimate births and therefor likely excluded a large section of the target population. While I provided empirical evidence to support the claim that, in practice, the survey included illegitimate births, the data were clearly not ideal. In all analyses I extensively relied on income to identify the target population. However, both the NNS and NHIS lacked detailed income measures that could be flexibly used to focus on specific income groups.

The PSID is an invaluable source of information on the evolution of individuals across the life-course and the evolution of families across generations. However, the survey began in 1968 and I had to rely on retrospectively reported information to characterize the childhood circumstances of observations born prior to 1968. While I was aided by the survey's large over-sample of low-income populations, I was still limited by small sample sizes. To help minimize this problem I relied on composite measures of health and economic status. The draw-back of these measures is that that they are difficult to interpret in a cost-benefit sense.

I also faced more general limitations inherent in the quasi-experimental design of the study. The most glaring issue was that my description of Medicaid's introduction in Chapter 3 clearly suggests that the schedule of Medicaid's adoption across the states was not randomly determined. States that had more generous pre-Medicaid programs adopted first and this could have downwardly biased my results (relative to the effect of Medicaid from an average base level of pre-Medicaid services). Early adopters could have also had increased other social spending over-time relative to later adopters. This could have upwardly biased my results.

I took several strategies to minimize the impact of secular changes that were correlated with the timing of Medicaid adoption. Each analysis include a rich set of contextual data that measured changes in hospital and physician supply, AFDC program characteristics, and the availability of other public programs associated with the War on Poverty. I included state and year fixed effects and region-by-year effects or state specific time trends. The fixed effects controlled for unobserved characteristics of states, years, and changes over-time and within geography. Finally, I produced estimates for placebo groups that were unlikely to have directly benefited from Medicaid due to eligibility rules. If Medicaid's adoption was correlated with meaningful, but uncontrolled secular trends that were affecting the entire population, this should have been evident in the Medicaid coefficients for the placebo groups, but it was not. In Chapter 6 I also undertook robustness tests that suggested that desegregation and selective migration cannot totally explain the results I observed. My conclusion based on these efforts is that the effects I estimate reflect real causal changes in utilization, infant health, and long-term outcomes.

IV. Policy Implications

This analysis demonstrated that providing health insurance to young children acts as an investment that pays-off in improved health during the adult period. Improvements in adult health have real implications to both individual welfare and total society wide medical spending. Extrapolating the effects I observed in this project to the contemporary environment is not straightforward. On one hand, today's low-income population might experience less material deprivation than the poor of the 1960's. This would suggest that Medicaid's effect might be smaller for more recent birth cohorts. On the other hand, medical technology has improved since the 1960's which would suggest that improving access to today's medical care has more long term benefits than improving access to yesterday's medical care.

As of 2012, 5.8 million children under 19 (7.5%), 1.4 million children under the age of 6 (5.6%), and 18 million non-elderly adult women (18.7%) lack health insurance coverage. The Congressional Budget Office (CBO) predicts that that Affordable Care Act will reduce the number of non-elderly uninsured by 25 million (CBO, 2014). The results of this study suggest that the benefit of health insurance expansions may not be limited to short run measures of health and financial security, but could likely deliver returns far into the future.

Adult morbidity and mortality is not randomly distributed in the U.S. Racial minorities and those in lower socioeconomic stratum are at increased risk of experiencing a chronic condition and premature mortality (CDC, 2013). These disparities begin at the earliest ages and increase with age (Case et al. 2006). The results from this project suggest that health is not just a function of contemporaneous resources that prevent disease or reduce its impact when it occurs, but a function of the cumulative resources and environments that people experience across their life-course. Importantly, this project demonstrates that public policy can play an important role in improving early childhood circumstances, even when the policy has as many flaws as the Medicaid program.

V. Future Work

The results I present here leave several open questions. For example, did the Medicaid expansions that started in 1980's also have long-term benefits? The relatively small effect that expansion induced Medicaid eligibility had on infant health suggests perhaps not. However, it could be that while the expansions did not substantially reduce the incidence of low birth weight it did improve child health in other ways or that the expansion had a relatively large impact on the financial resources of families that switched from private to public health insurance.

In this project I chose to focus on the introduction of Medicaid rather than the expansion of the 1980's and 1990's because it provided a longer follow-up period. However, other researchers are starting to consider the long run impact of Medicaid expansions. A working paper by Brown et al. (2014) estimates the impact of Medicaid expansion in the 1980's and 1990's on outcomes found in administrative tax records. Their preliminary estimates suggest that eligibility for Medicaid through age 18 increases cumulative wages and federal tax receipts, decreases participation in the earned income tax credit program, increases college attendance, and decreases mortality by age 31. They suggest that the government recoups its investment by the time affected cohorts reach the age of 36. By age 60, the estimated return to the government is 443%. Given that their estimates measure the impact of eligibility and not participation, they suggest that for the beneficiary population, returns could be as high as 880%. Other authors are working on similar analyses of the long-run effects of expanding Medicaid eligibility (e.g. Groves, 2014; Miller et al. 2014; Cohodes et al. 2014).

The life-course effects I described here could translate into inter-generational effects. An important risk factor of poor infant health is the disease state of the mother. The effects I observed could spill over into the next generation. Estimating these relationships would be a data intensive endeavor. The PSID is one plausible data source as it contains data on multiple generations of the same families. Another possible data source are certificates of live birth which often include information on the mother's state of birth, her health status during pregnancy, and information about the child's health.

The capacity formation framework described in Chapter 2 suggests that child development is a dynamic process that requires multifaceted interventions that nurture cognitive and socio-emotional skill development in addition to promoting physical and mental health. This philosophy is already embedded in the design of early childhood programs such as Head Start that provide health services, parental counseling, and educational interventions for children prior to school age. However, there is little empirical work examining how large scale public programs such as Medicaid interact with other programs. Or for that matter, how the components of smaller programs such as Head Start or the Abbercerian program interact. Capacity formation suggests that programs like Medicaid that improve the health of children could enhance later investments such as public education. Future work is needed not only to understand what interventions are effective, but when they should be delivered, and how they interact with other features of the social environment.

VI. Conclusion

The Medicaid program serves a large role in the U.S. health system and consumes considerable resources. Evidence from this project suggests that Medicaid not only improves short term outcomes, but pays future dividends in the form of improved health status across the life-course. While my study of long-term economic status was inconclusive other recent work suggests Medicaid imparts meaningful gains to income while reducing reliance on transfer programs. These benefits come in spite of well-studied short comings in the Medicaid program. Continued efforts to improve Medicaid and to expand coverage to the entire population will likely have long lasting benefits.

TABLES AND FIGURES

Figure 2.1. Conceptual Model

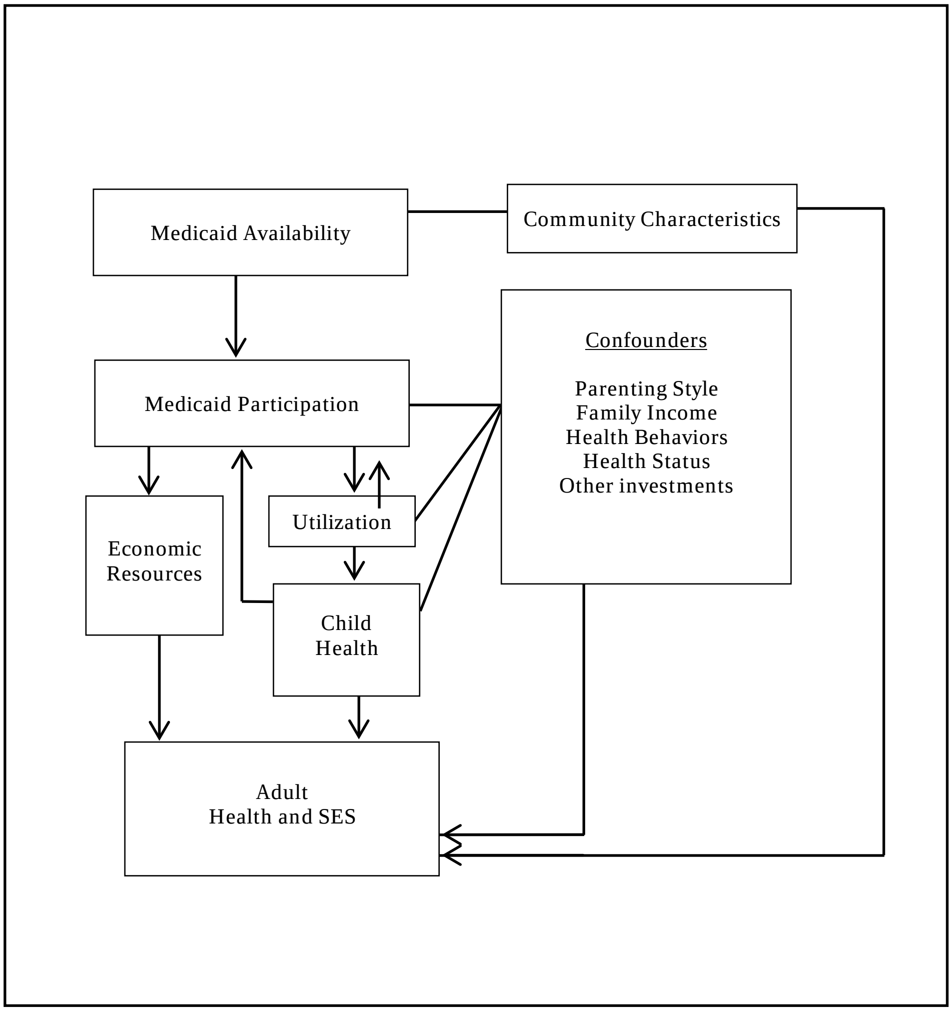


Figure 2.2 Simplified Conceptual Model

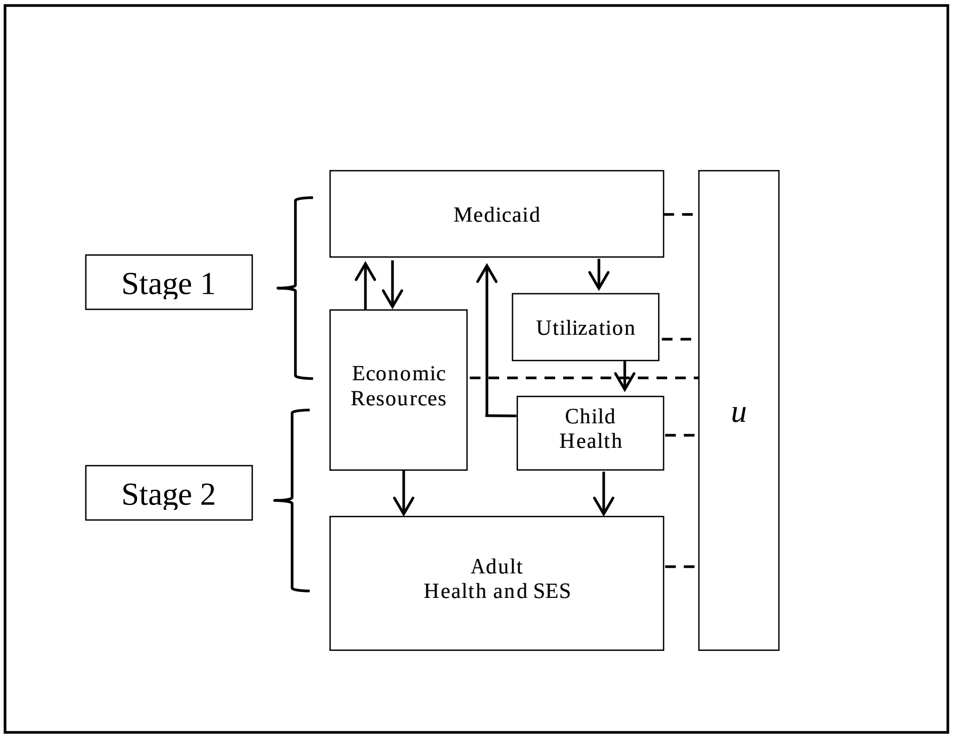
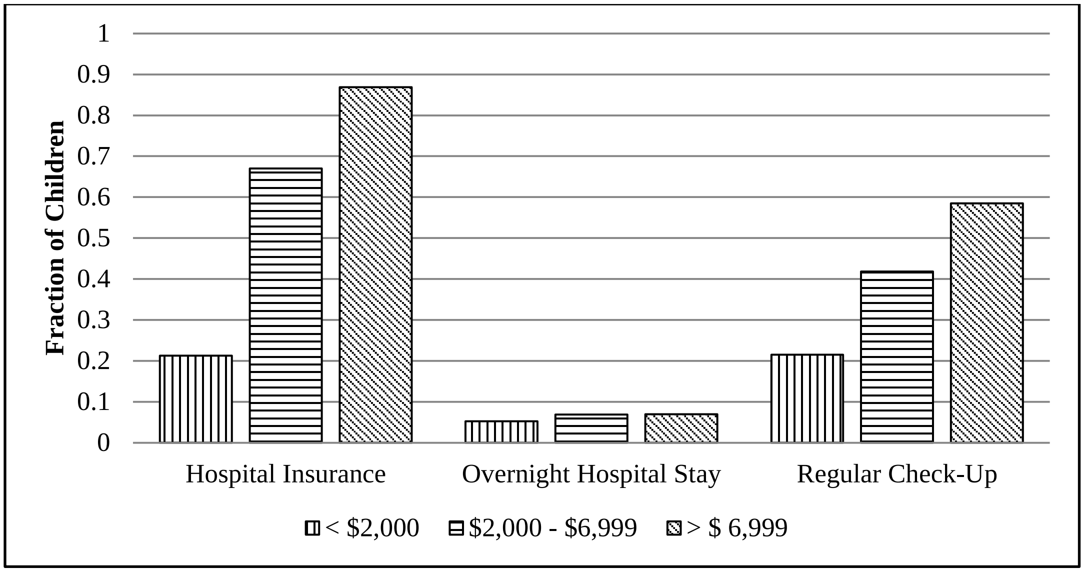
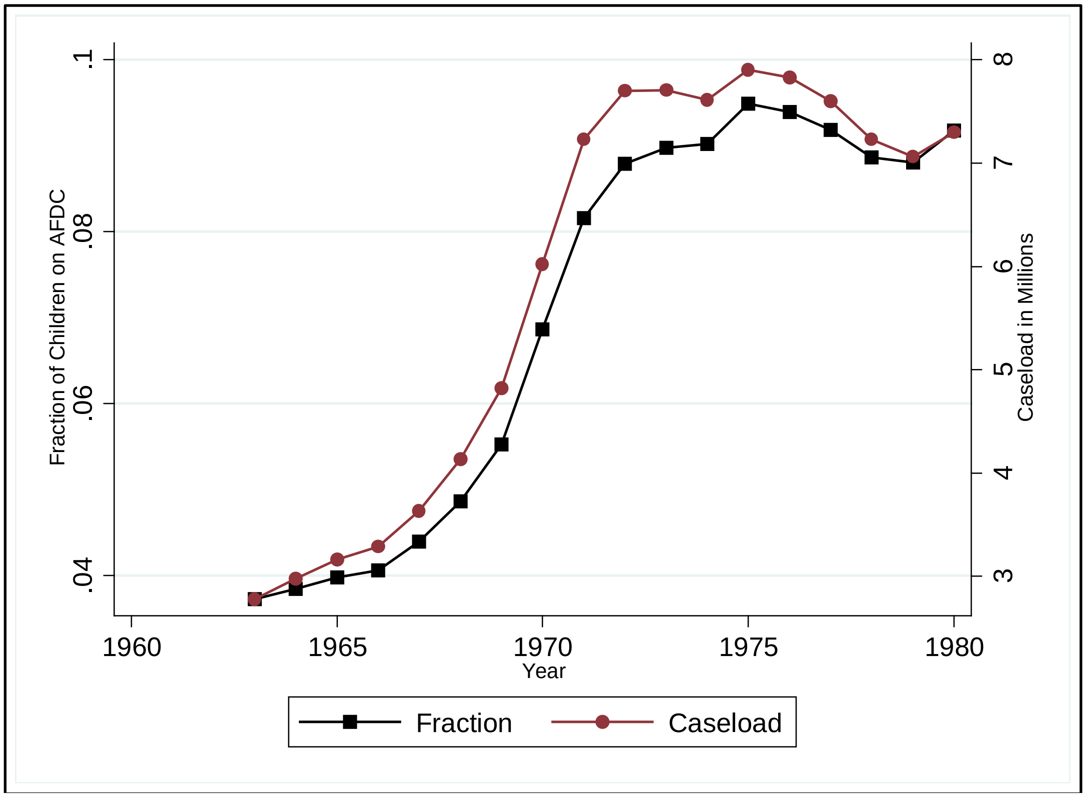


Figure 3.1. Insurance Coverage and Utilization among Children (Age 0-5)



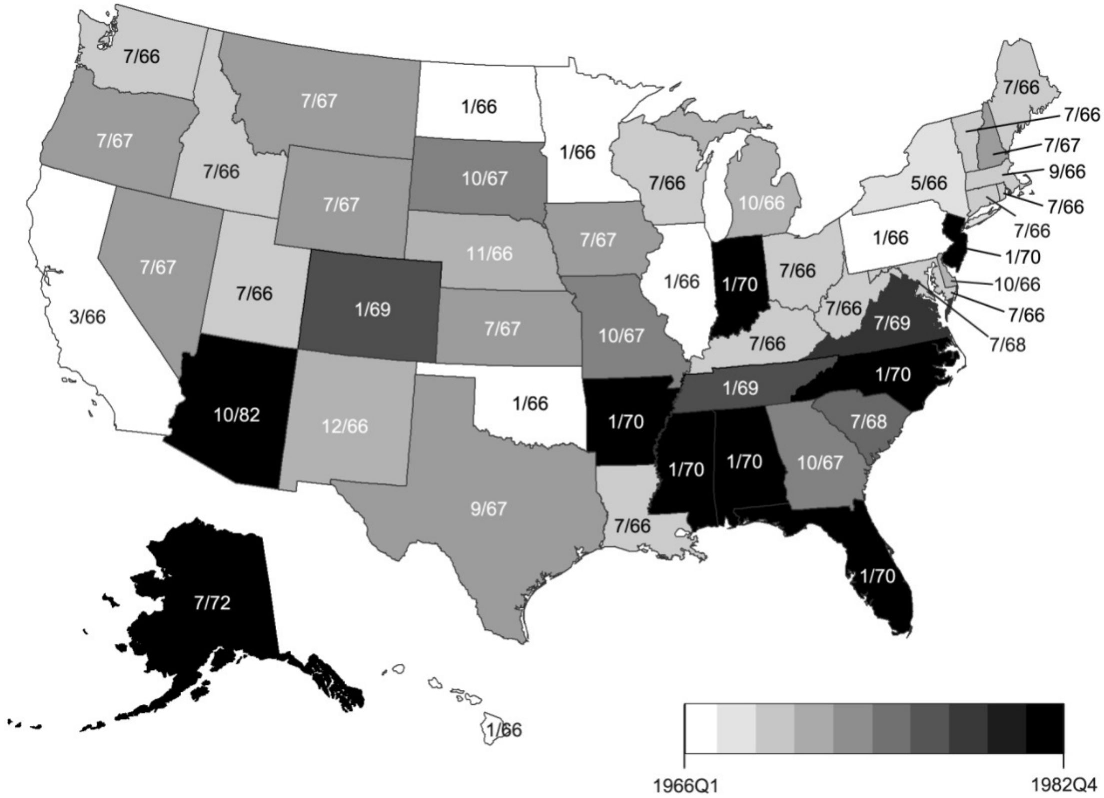
Source: Author's tabulation of the National Health Interview Survey, 1963 and 1964. Insurance coverage is measured on the date of interview and utilization is measured over the 12-months prior to interview. All differences between low-income children and both other groups are statistically significant (p<0.001).

Figure 3.2 AFDC Child Caseloads, 1963-1980.



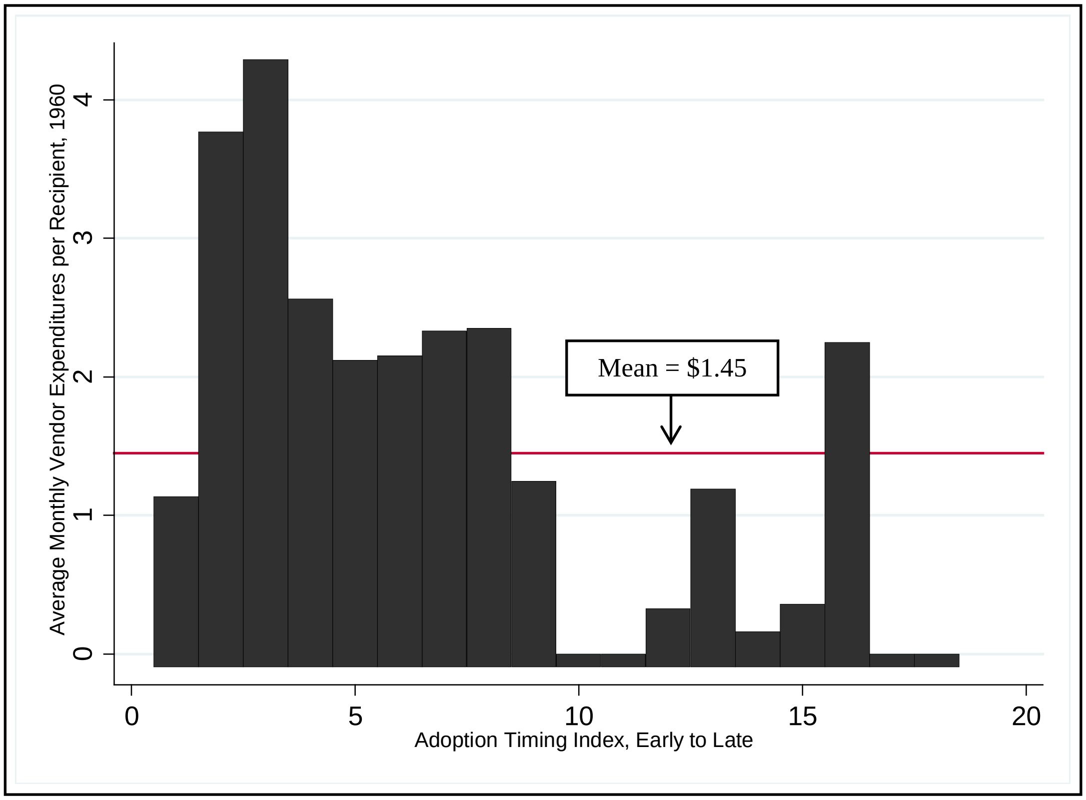
Source: AFDC Caseloads include both AFDC-BASIC and AFDC-Unemployed Parent Programs. Data are from the Administration of Children and Families (2013). Population estimates are from the 1960 Decennial Census and intercensal population estimates (SEER, 2012).

Figure 3.3 Medicaid Adoption by Quarter



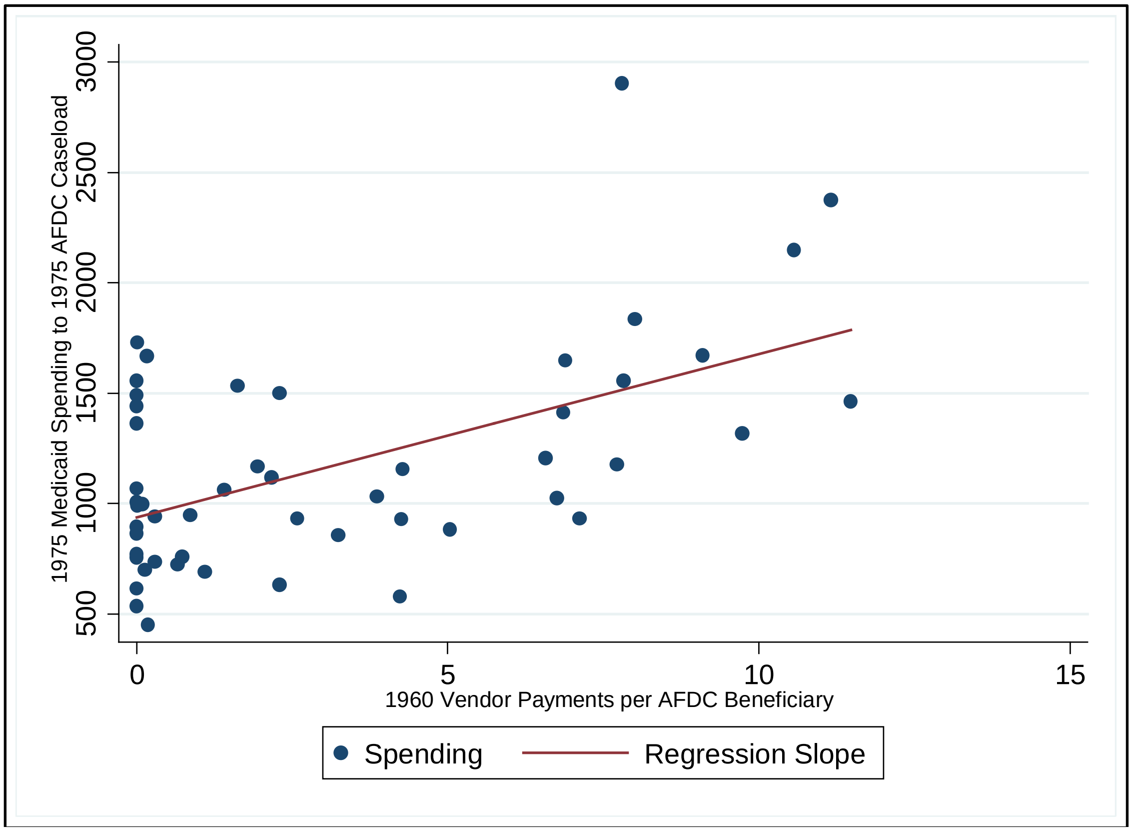
Source: Adoption dates come from the Department of Health Education and Welfare (1970) & Social Security Administration (2013). The map is shaded relative to the quarter of adoption and states are labeled with the month and year of adoption.

Figure 3.4. Average Monthly Vendor Expenditures Per Recipient in 1960, by Timing of Medicaid Adoption



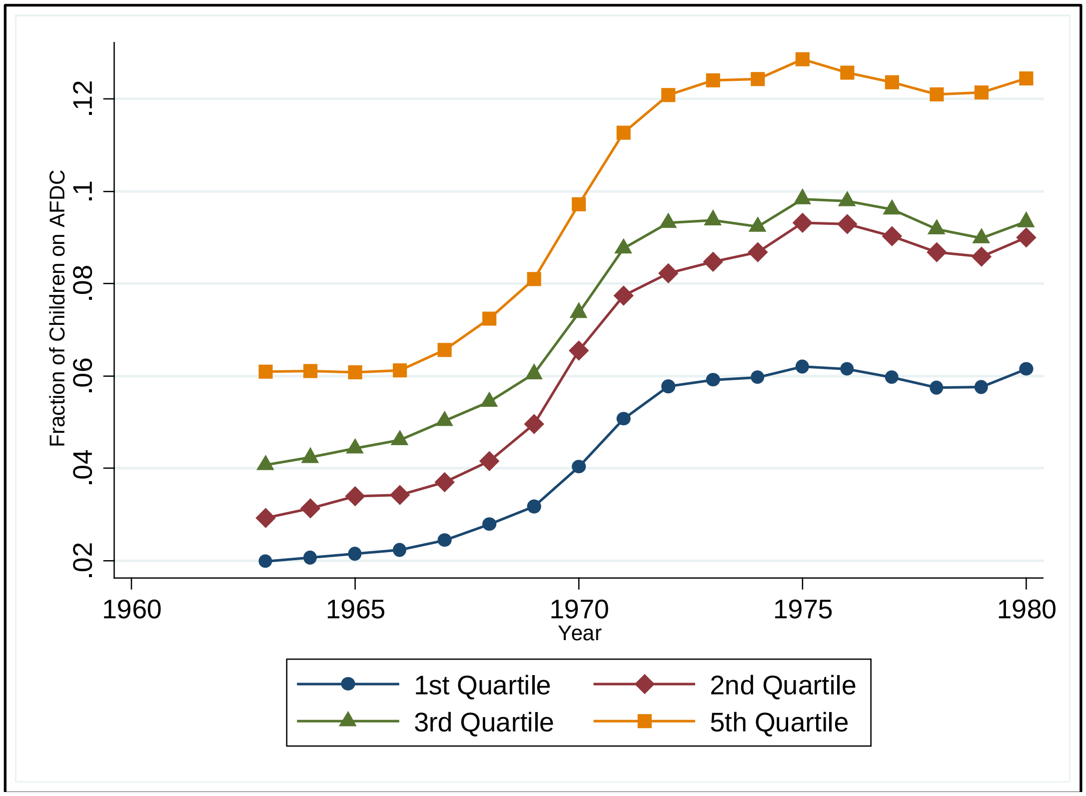
Source: Expenditure data is from Committee on Ways and Means, 1961. Timing data is from the Department of Health Education and Welfare (1970) and Social Security Administration (2013). The timing index, represents the relative month and year of adoption starting in January 1966 and ending in October 1982. The index is an ordinal measure that forces equal distance between adoption dates. The average expenditure level, shown as a horizontal line, was $1.45.

Figure 3.5. 1975 Medicaid Expenditures and 1960 Vendor Payments



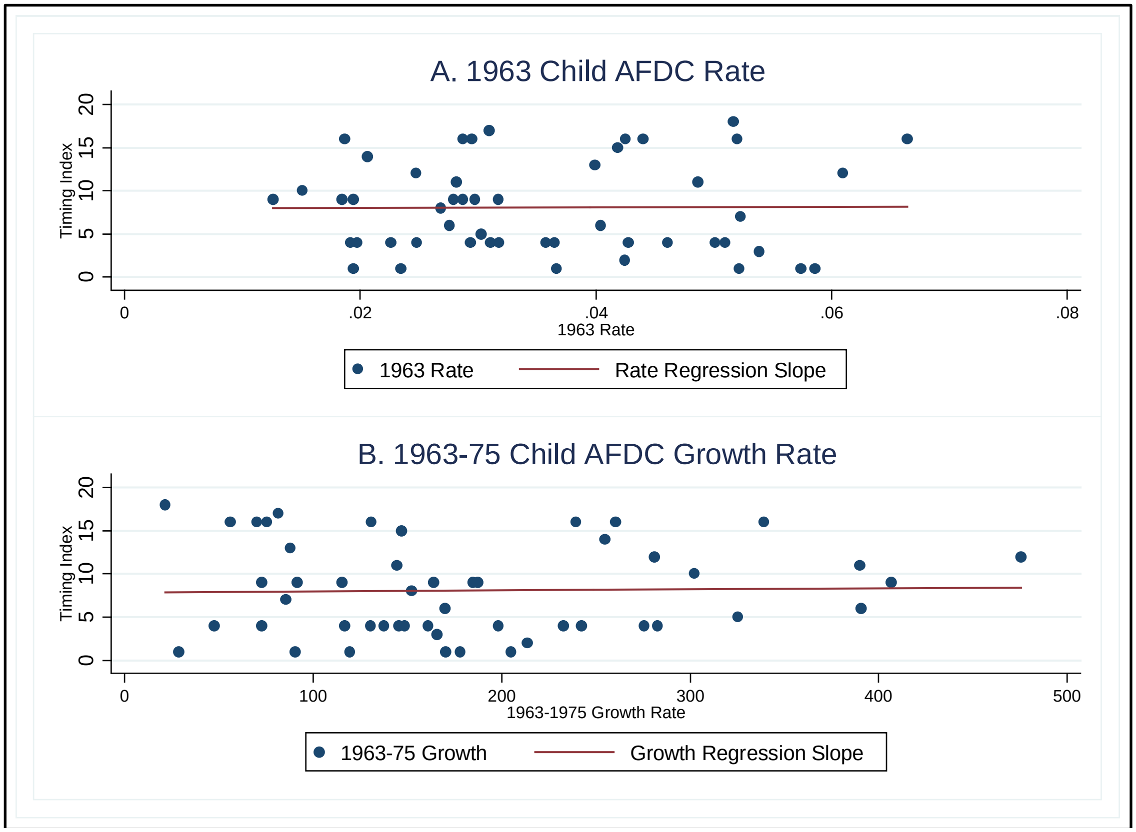
Source: Medicaid expenditures come from the Regional Economic Information System and are scaled by the total number of AFDC recipients (adults and children). Vendor payment data comes from Committee on Ways and Means (1961) and is scaled by the number of AFDC beneficiaries. The two data series are not directly comparable because Medicaid expenditures apply to all eligibility categories and vendor payments are specific to AFDC. All amounts are in 1975 dollars. The slope coefficient is 74 ($p<0.001$).

Figure 3.6. AFDC Rates for Children by Quartile of 1963 Level



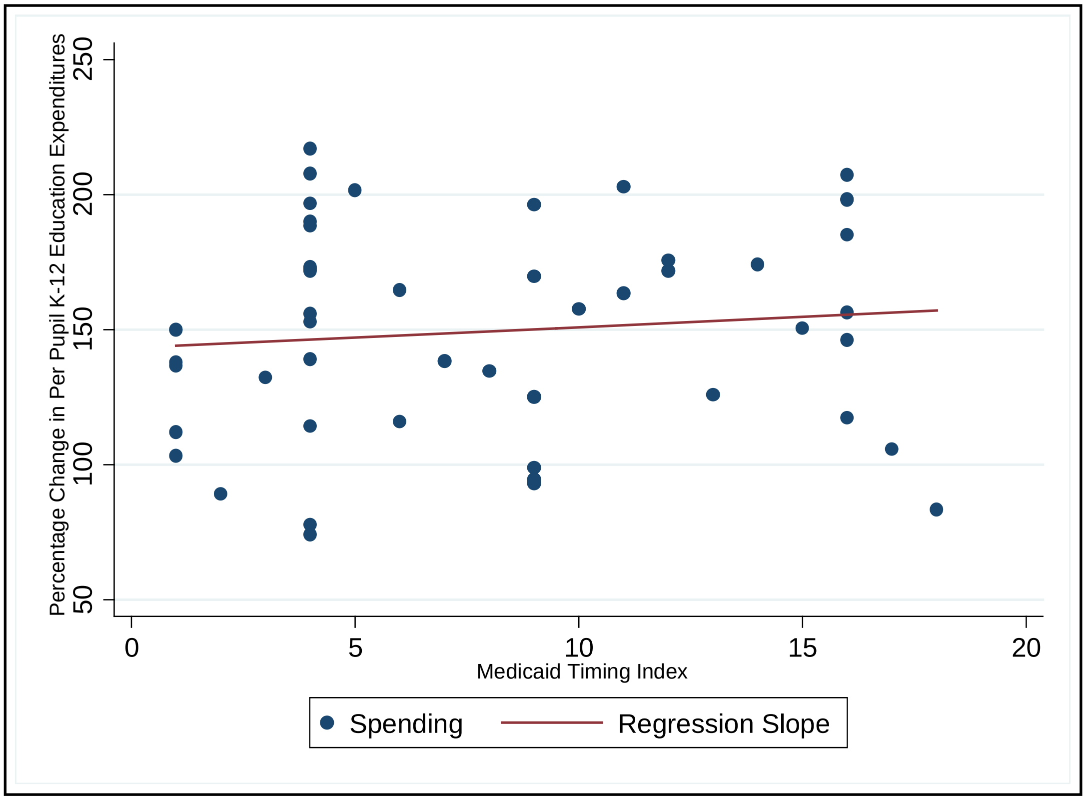
Source: AFDC Caseloads include both AFDC-BASIC and AFDC-Unemployed Parent Programs. Data are from the Administration of Children and Families (2013). Population estimates are from the 1960 Decennial Census and intercensal population estimates (SEER, 2012).

Figure 3.7. Medicaid Adoption Timing by 1963 AFDC Rates and 1963-75 Grow Rate.



Source: AFDC Caseloads include both AFDC-BASIC and AFDC-Unemployed Parent Programs. Data are from the Administration of Children and Families (2013). Population estimates are from the 1960 Decennial Census and intercensal population estimates (SEER, 2012). Timing data is from the Department of Health Education and Welfare (1970) and Social Security Administration (2013). The timing index represents the relative month and year of adoption starting in January 1966 and ending in October 1982. The index is an ordinal measure that forces equal distance between adoption dates. The slope of the rate regression line is 3.3 (p< 0.954) and the growth regression line is .001 (p=.875). West Virginia was dropped because it was a clear outlier (index=4; AFDC rate=0.12), however, retaining it does not impact the results.

Figure 3.8. Changes in Educational Spending, 1969/70 to 2005/06, by Medicaid Adoption Timing.

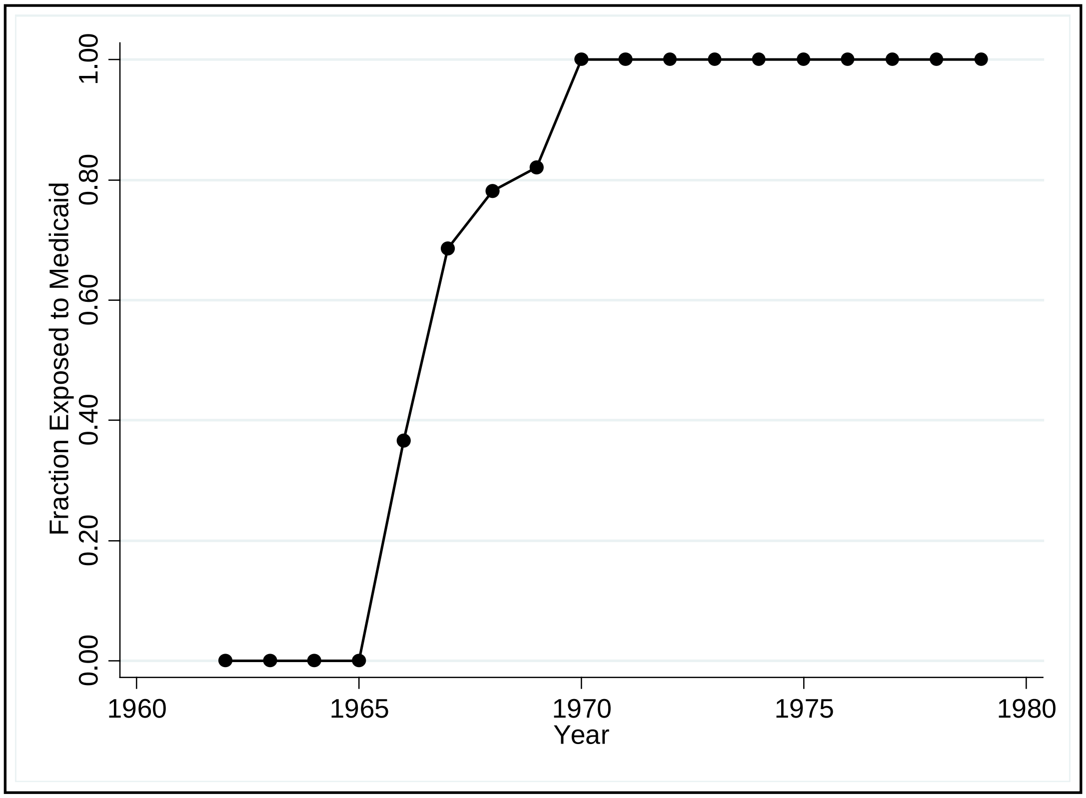


Source: Educational Spending comes from the National Center for Education Spending (2008). Changes are calculated as percentage changes of real 2006 dollars. The timing index represents the relative month and year of adoption starting in January 1966 and ending in October 1982. The index is an ordinal measure that forces equal distance between adoption dates. The slope coefficient is .77 (p<0.46).

Table 3.1 Federal Medicaid Eligibility Groups at Program Inception

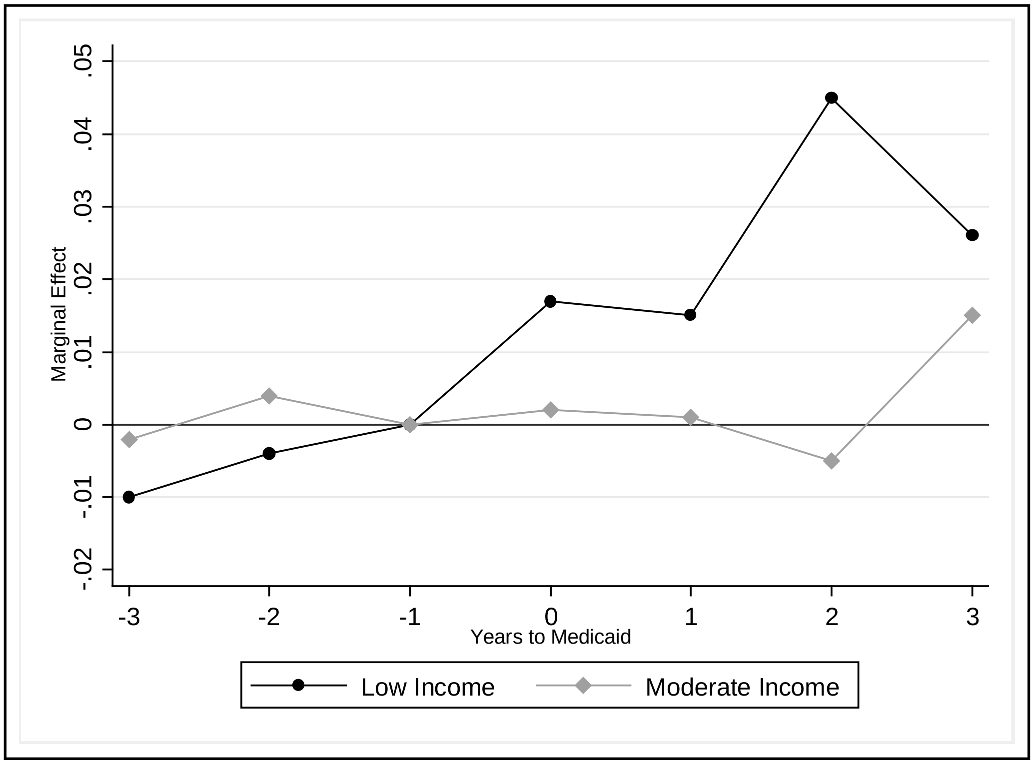
Eligibility Category	**State Option**	**Federal Match**
Categorical Needy Receiving Cash Payments from categorical public assistance programs (e.g. AFDC)	Mandatory	Yes
Categorical Related Needy Would be categorically needy except for state eligibility exceptions prohibited by Title XIX (e.g. residency restrictions, age and school attendance restrictions)	Mandatory (some categorical related needed were optional)	Yes
Categorical Medical Needy If medical expenses are disregarded from income, meet categorical need.	Optional	Yes. As of 1967, only for those with incomes below 133 % of the AFDC standard.
Non-Categorical Medical Needy All non-elderly medical needy	Optional	No
Non-Categorical Needy Enrollees on general assistance (state funded cash transfer program)	Optional	No
Source: Bernard and Feldstein, 1970		

Figure 4.1 Fraction of Children (0-17) Exposed to Medicaid



Source: 1963-1980 NHIS. An observation is considered to be exposed if Medicaid exists the first quarter the hospital visit reference period, which occurs 12 months prior to the interview date.

Figure 4.2. Event Study Estimates, Low versus Moderate Income Children (age 0-5)



Source: NHIS 1963-1980. The time scale is top and bottom coded. The model is described in the text. The marginal effects are in reference to the year prior to Medicaid's introduction.

Table 4.1 Population Characteristics by Group, National Health Interview Survey						
	Children 0-5		**Children 0-17**		**Mothers 18-45**	
	Estimate	SE	Estimate	SE	Estimate	SE
Basic Demographics						
Age (SD)	2.5 (1.7)	0.004	8.7 (5.2)	0.010	32.7 (7)	0.019
Female	0.49	0.001	0.49	0.001	1	--
Male	0.51	0.001	0.51	0.001		
White	0.84	0.003	0.85	0.003	0.88	0.002
Household Head completed at least Some HS	0.83	0.002	0.80	0.002	0.86	0.001
South	0.33	0.003	0.33	0.003	0.32	0.003
Real Income Category (1970 dollars)						
0-2999	0.11	0.001	0.10	0.001	0.08	0.001
3000-4999	0.13	0.001	0.12	0.001	0.11	0.001
5000-10000	0.43	0.002	0.39	0.001	0.40	0.001
10000+	0.33	0.002	0.40	0.002	0.41	0.002
Medicaid Exists at start of Hospital visit reference period						
Yes	0.67	0.003	0.70	0.003	0.72	0.003
Any Annual Hospital Visit in 12 months prior to interview						
1+ Visit Within Last Year	0.07	0.001	0.06	0.000	0.22	0.001
Any Annual Physician Visit in 12 months prior to interview						
Visit Within Last Year	0.87	0.001	0.73	0.001	0.83	0.001
Sample Size	235,044		760,570		263,582	

Source: National Health Interview Survey 1963-1980. SE are Taylor series linearized standard errors that account for sample clusters and strata. All estimates are weighted.

Table 4.2. Means, Standard Deviations, Minimums, Maximums, National Health Interview Survey	Mean	SE	SD	Min	Max
Children Age 0-5					
AFDC Moderator 1 (Demographic Predictors)	0.03	0.0004	0.084	0.004	0.655
AFDC Moderator 2 (State and Sex Predictors)	0.02	0.0002	0.030	0.000	0.216
Per Cap Hospitals (Per 10,000)	0.29	0.0022	0.129	0.125	1.012
Per Cap MD's (Per 100)	0.14	0.0004	0.044	0.070	0.545
Per Cap PA Spending (Per Person)	217.78	1.2194	120.258	35.368	750.061
AFDC Benefits	858.96	2.8063	270.383	167.804	1372.668
Per Cap AFDC Case Loads (Per 1000)	37.81	0.2023	18.133	6.420	146.579
Children Age 0-17					
AFDC Moderator 1 (Demographic Predictors)	0.03	0.0003	0.078	0.004	0.655
AFDC Moderator 2 (State and Sex Predictors)	0.02	0.0002	0.031	0.000	0.216
Per Cap Hospitals (Per 10,000)	0.29	0.0022	0.129	0.125	1.012
Per Cap MD's (Per 100)	0.14	0.0004	0.044	0.070	0.545
Per Cap PA Spending (Per Person)	223.46	1.2085	121.446	35.368	750.061
AFDC Benefits	859.06	2.8032	271.023	167.804	1372.668
Per Cap AFDC Case Loads (Per 1000)	38.73	0.2024	18.187	6.420	146.579
Mothers 18-45					
AFDC Moderator 1 (Demographic Predictors)	0.03	0.0003	0.072	0.004	0.655
AFDC Moderator 2 (State and Sex Predictors)	0.02	0.0002	0.032	0.000	0.216
Per Cap Hospitals (Per 10,000)	0.29	0.0022	0.128	0.125	1.012
Per Cap MD's (Per 100)	0.15	0.0004	0.044	0.070	0.545
Per Cap PA Spending (Per Person)	229.91	1.2532	122.430	35.368	750.061
AFDC Benefits	859.40	2.7300	269.640	167.804	1372.668
Per Cap AFDC Case Loads (Per 1000)	39.52	0.2058	18.138	6.420	146.579

Source: National Health Interview Survey 1963-1980. SE accounts for sample clusters and strata. All estimates are weighted.

Table 4.3 The Impact of Medicaid's Introduction on the Probability of Any Annual Hospital Visit, by Group

	Low Income			Moderate Income		
	AME	SE	P-Value	AME	SE	P-Value
Age 0-5						
Medicaid	0.03**	0.011	0.002	-0.003†	0.005	0.563
Sample Size	24,520			101,793		
Base Rate (1963 & 1964)	0.06			0.08		
Age 0-17						
Medicaid	0.01*	0.010	0.040	0.0004	0.003	0.890
Sample Size	69,402			297,626		
Base Rate (1963 & 1964)	0.05			0.06		
Mothers 18-45						
Medicaid	-0.007	0.019	0.718	-0.004	0.006	0.569
Sample Size	20,902			106,045		
Base Rate (1963 & 1964)	0.27			0.25		

Source: NHIS 1963-1980. Standard errors (SE) account for sample design strata and clusters and all estimates are weighted. Low income is below $3,000 (1970 dollars) and moderate income is $5,000-$10,000. Average marginal effects (AME) are computed from logistic regression results and each result is estimated from a separate model. Covariates include demographics, contextual controls, state fixed effects, year fixed effects, and region-by-year fixed effects. See text for fuller details. Complete logistic regression results are produced in the appendix. Significant differences from 0 are indicated with *p<0.05; **p<0.01; ***p<0.001. † indicates the difference is significantly different than the estimate in the low-income sample, using a t-test.

Table 4.4 The Impact of Medicaid's Introduction on the Probability of Any Annual Hospital Visit, AFDC Specifications									
	Children 0-5			**Children 0-17**			**Mothers (18-45)**		
	AME	SE	P-Value	AME	SE	P-Value	AME	SE	P-Value
AFDC Moderator 1									
No Medicaid	0.02	0.022	0.440	0.01	0.015	0.692	0.10***	0.029	0.000
Medicaid	0.07***	0.009	0.000	0.06***	0.004	0.000	0.10***	0.013	0.000
Difference	0.05*	0.022	0.013	0.05*	0.015	0.001	-0.006	0.030	0.852
Sample Size	232,219			751,385			261,100		
AFDC Moderator 2									
No Medicaid	0.07	0.044	0.096	0.04	0.023	0.081	-0.27***	0.083	0.001
Medicaid	0.20***	0.021	0.000	0.14***	0.010	0.000	-0.15***	0.029	0.000
Difference	0.13**	0.047	0.008	0.10***	0.025	0.000	0.120	0.089	0.170
Sample Size	232,219			751,385			261,100		

Source: National Health Interview Survey 1963-1980. Standard errors (SE) account for sample design strata and clusters and all estimates are weighted. AFDC Moderator 1 is the predicted probability of AFDC participation defined within 16 demographic groups. AFDC Moderator 2 is the predicted probability of AFDC participation defined by gender, state, and year. Significant differences from 0 are denoted with *p<0.05; **p<0.01; ***p<0.001. Average marginal effects (AME) are computed from logistic regression results and each result is estimated from a separate model. Covariates include demographics, contextual controls, state fixed effects, year fixed effects, and region-by-year fixed effects. See text for fuller details. Complete logistic regression results are produced in the appendix.

Table 5.1. Socio-demographic Characteristics From the 1967 NNS and 1970 Census			
		1970 Census	
	NNS	All New Mothers	New Married Mothers
Non White	0.13	0.13	0.11
High School Graduate or More	0.66	0.68	0.70
Age	25.40	27.08	26.98
Income Less than $3000	0.13	0.11	0.081
Welfare Income	0.05	0.04	0.016
Welfare Income | Income<$3,000	.18	.24	.054

Source: 1967 National Natality Survey and 1970 Decennial Census Long Form. Census mothers are defined as women with own children age 1 or younger.

Table 5.2. Summary Statistics, National Natality Survey				
	All Mothers		**Low or Moderate Income Mothers**	
	Mean (SE)	SD	Mean (SE)	SD
Medicaid Exists Before Birth	0.48 (0.04)	0.50	0.45 (0.04)	0.50
Low Birth Weight (<2,500 g)	0.07 (0.00)	0.26	0.08 (0)	0.27
Birth Weight in Grams	3291.07 (8.88)	577.78	3286.22 (9.67)	579.88
Log Birth Weight	8.08 (0.00)	0.21	8.08 (0)	0.21
Income Below 3000	0.15 (0.01)	0.36	0.25 (0.02)	0.43
Income $5-10k	0.46 (0.01)	0.50	0.75 (0.02)	0.43
Age of Mother	25.47 (0.13)	5.78	25.13 (0.15)	5.71
Mother Not White	0.12 (0.01)	0.33	0.13 (0.01)	0.35
Mother Completed HS	0.68 (0.01)	0.47	0.65 (0.01)	0.48
# of Living Siblings	1.62 (0.03)	1.83	1.65 (0.03)	1.85
Lives in the South	0.3 (0.10)	0.47	0.31 (0.1)	0.47
Legalized Abortion	0.02 (0.01)	0.13	0.02 (0.01)	0.13
Total All AFDC per 1000	27.92 (2.25)	14.13	26.58 (2.07)	13.05
Maximum AFDC Benefit	871.29 (54.13)	254.90	863.14 (53.03)	253.39
Total MD per 100,000	131.78 (8.72)	38.20	130.04 (8.26)	36.83
Total HOSP per 100,000	3.03 (0.24)	1.37	3.05 (0.24)	1.37
Per Cap Total PA Spending,	150.90 (16.03)	83.68	143.49 (14.63)	76.90
Head Start Share	0.46 (0.04)	0.34	0.45 (0.04)	0.34
Job Share	0.38 (0.04)	0.27	0.37 (0.04)	0.27
Oth Health Share	0.25 (0.03)	0.21	0.23 (0.03)	0.20
CHC Share	0.14 (0.03)	0.18	0.13 (0.03)	0.18
MIC Share	0.07 (0.01)	0.13	0.06 (0.01)	0.12
MCH Share	0.42 (0.02)	0.48	0.38 (0.02)	0.47
Fam. Planning Share	0.21 (0.03)	0.24	0.19 (0.02)	0.22
Food Stamp Share	0.41 (0.04)	0.34	0.39 (0.04)	0.33
Sample Size	24,203		14,295	

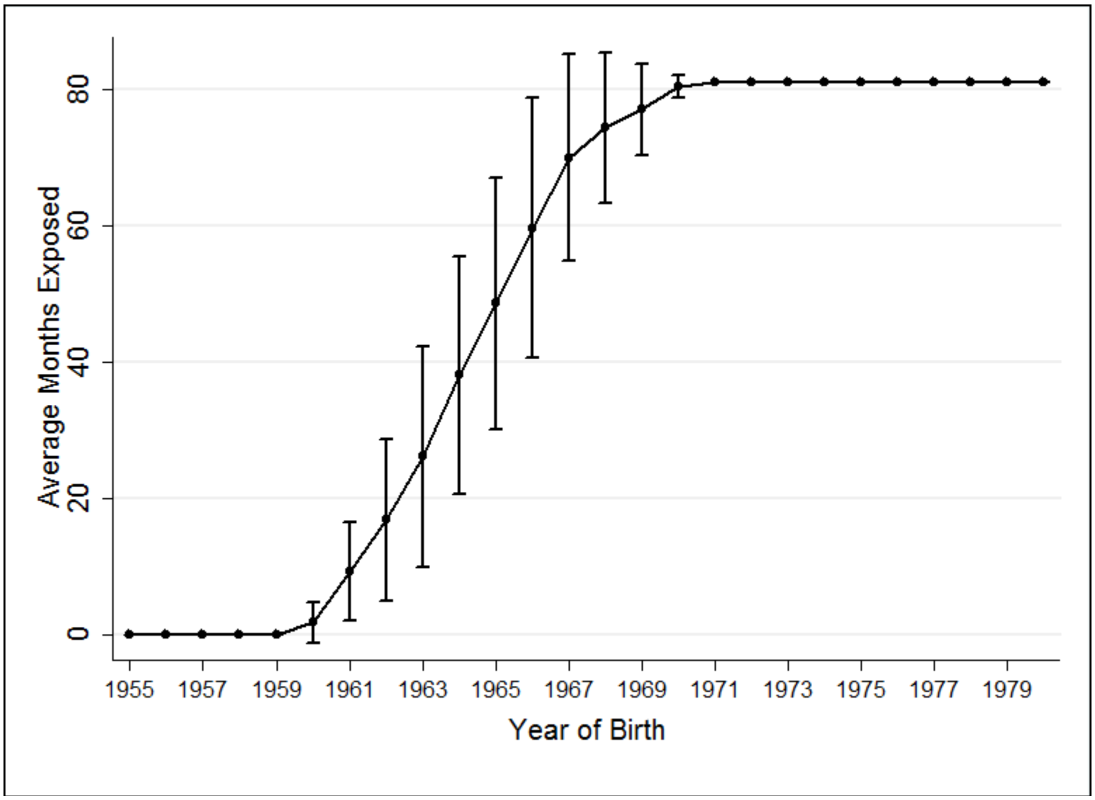
Source: 1964-1969 and 1972 National Natality Survey. All estimates are weighted. Standard errors (SE) are clustered on state.

Table 5.3. The Impact of Medicaid's Introduction on Birth Weight

	Low Birth Weight		Log of Birth Weight (g)	
	AME	SE	AME	SE
Full Sample (Panel A)				
Moderate Income	0.002	0.0097	0.001	0.0083
Low Income	-0.041**	0.0143	0.007	0.0143
Difference	-0.044**	0.017	0.005	0.016
Base Rate (1964) | Income < $3000	0.098		8.057	
Remove Small Sample States (Panel B)				
Moderate Income	-0.01	0.0096		
Low Income	-0.040*	0.0184		
1964-1966 NNS (Panel C)				
Moderate Income	-0.005	0.0124		
Low Income	-0.059**	0.0195		

Source 1964-1969 and 1972 National Natality Survey. Each column represents a separate regression. Low birth weight is modeled with logistic regression and average-marginal-effects (AME) are reported. Log birth weight is modeled with OLS. The models control for demographics, contextual controls, state and year fixed effects interacted with income, and region-by-year fixed effects. All estimates are weighted and standard errors (SE) are clustered on state of birth. *p<0.05; **p<0.01.

Figure 6.1: Number of Months Exposed to Medicaid During Early Childhood, by Birth Cohort



Notes: Source: The 1955-1980 birth cohorts from the 1968-2009 Panel Study of Income Dynamics (person level observations). Error bars represent 1 standard deviation above (below) the mean. The error bars are not constrained by the maximum plausible value.

Table 6.1. Descriptive Statistics, Panel Study of Income Dynamics

	Mean	SD	Min	Max
Medicaid Exposure				
MCAIDSHARE	0.37	0.43	0	1
Basic Demographics				
Gender (Male)	0.46	0.5	0	1
Race (White)	0.83	0.38	0	1
Age	37.34	7.84	18	54
Married	0.65	0.48	0	1
Family Background				
Poverty Ratio Less than 1.5, Early Childhood	0.22	0.42	0	1
Poverty Ratio between 1.75 and 3.0, Early Childhood	0.37	0.48	0	1
Childhood Head Less Than High School	0.35	0.48	0	1
Health Outcomes				
Health Index	-0.038	0.557	-0.3	4.1
Fair Health or worse	0.07	0.26	0	1
High Blood Pressure	0.14	0.34	0	1
Heart Disease or Heart Attack	0.03	0.16	0	1
Adult Onset Diabetes	0.02	0.14	0	1
Obese	0.24	0.43	0	1
Economic Outcomes (for 25+ year olds)				
Economic Index	0.272	0.905	-2.7	39.3
Years of Education, Top code at 17	13.41	2.1	1	17
Harmonized Income to Poverty Ratio	4.58	6.17	0	570.2
Decile of Family Wealth	6.11	2.94	1	10
Sample Size				
Person-Year Observations		18,243		
Unique Person Observations		3,863		
Average years observed (min, max)		3.6 (1,6)		

Source: 1968-2009 Panel Study of Income Dynamics. See text for a description of the analytical sample. All estimates are weighted. Sample size describes observations with non-missing condition index values which applies only to the 1999-2009 waves.

Table 6.2. The Impact of Medicaid Exposure in Early Childhood on a Composite Index of Adult Chronic Conditions, by Group								
	High Impact				Low Impact (Placebo)			
	Low Income		Low Education		Moderate Income		High Education	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Chronic Condition Index	-0.36**	0.17	-0.18	0.18	0.05	0.14	0.01	0.12
Sample Size	5,926		6,960		5,695		10,802	
Mean of Y	0.09		0.1		-0.05		-0.09	
R^2	0.2		0.18		0.15		0.08	
Fair Health or Worse	0.03	0.07	0.01	0.05	0.02	0.04	0.04	0.04
High Blood Pressure	-0.23**	0.10	-0.24**	0.11	-0.07	0.09	0.02	0.07
Heart Disease/Heart Attack	-0.01	0.05	-0.03	0.04	-0.04	0.05	-0.02	0.03
Adult Onset Diabetes	-0.05	0.06	0.03	0.04	0.07	0.05	0.03	0.05
Obesity (BMI≥30)	-0.20	0.14	-0.13	0.16	0.01	0.15	-0.01	0.07

Source: Panel Study of Income Dynamics 1968-2009. All parameters are from separate regressions. Models include a quadratic in age, gender, race, marital status, the full set of contextual controls described in the text, interview year fixed effects, state of birth fixed effects, year of birth fixed effects, and state specific trend in birth cohort. See text for an explanation of the analytical sample, the impact groups, and construction of the chronic condition index. All estimates are weighted and standard errors (SE) are clustered on state of birth. *p<0.1; **p<0.05; ***p<0.01.

Table 6.3. The Impact of Medicaid Exposure in Early Childhood on a Composite Index of Adult Economic Attainment, by Group								
	High Impact				**Low Impact (Placebo)**			
	Low Income		**Low Education**		**Moderate Income**		**High Education**	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Economic Index	-0.11	0.21	-0.18	0.19	0.14	0.29	-0.29*	0.16
Sample Size	5,973		7,181		5,739		10,579	
Mean of Y	-0.24		-0.10		0.23		0.44	
R^2	0.33		0.30		0.14		0.16	
Years of Education	-0.04	0.52	-0.28	0.6	0.37	0.7	0.12	0.50
Continuous Income to Poverty Ratio	-1.06*	0.59	-0.91	0.81	-0.9	1.54	-0.62	0.94
Decile of Family Wealth	-0.11	0.74	-0.33	0.59	1.55*	0.86	-0.86	0.52

Source: Panel Study of Income Dynamics 1968-2009. All parameters are from separate regressions. Models include a quadratic in age, gender, race, marital status, the full set of contextual controls described in the text, interview year fixed effects, state of birth fixed effects, year of birth fixed effects, and state specific trend in birth cohort. See text for an explanation of the analytical sample, the impact groups, and construction of the Economic index. All estimates are weighted and standard errors (SE) are clustered on state of birth. *p<0.1; **p<0.05; ***p<0.01.

Table 6.4. The Impact of Medicaid Exposure in Early Childhood, Triple Difference Models				
	Chronic Condition Index		**Economic Index**	
	Coef.	SE	Coef.	SE
With Contextual Controls				
Medicaid Exposure	-0.03	0.08	-0.16	0.14
Medicaid Exposure*Predicted AFDC	-0.88*	0.45	-0.07	1.10
Sample Size	18,094		17,970	
Mean of Y	-0.04		0.19	
R^2	0.12		0.25	
Without Contextual Controls				
Medicaid Exposure	-0.04	0.11	0.12	1.04
Medicaid Exposure*Predicted AFDC	-0.99**	0.44	-0.01	0.96
Sample Size	18,241		18,112	
Mean of Y	-0.04		0.19	
R^2	0.11		0.24	

Source: Panel Study of Income Dynamics 1968-2009. Models include a quadratic in age, gender, marital status, race, the full set of contextual controls described in the text, interview year fixed effects, state of birth fixed effects, year of birth fixed effects, state specific trend in birth cohort, and interactions of the predicted probability of AFDC participation ("Predicted AFDC") and year of birth and interview year. See text for an explanation of the analytical sample and construction of the outcome variables. Estimates are weighted and standard errors (SE) are clustered on state of birth. *p<0.1; **p<0.05; ***p<0.01.

Table 6.5. Desegregation Robustness Check: Results in Chronic Condition and Economic Index Regressions After Removing Southern Born Non-Whites

	Low Income		Low Education		Moderate Income		High Education	
Panel A: Condition Index	Coef.	SE	Coef.	SE	Coef.	SE	Coeff.	SE
Medicaid Exposure	-0.374*	0.210	-0.290	0.250	-0.041	0.16	-0.014	0.123
Sample Size	2692		3778		4841		9364	
Mean of Y	0.09		0.12		-0.02		-0.06	
R-Squared	0.24		0.2		0.16		0.09	
Panel B: Economic Index								
Medicaid Exposure	-0.161	0.357	-0.098	0.197	-0.041	0.16	-0.014	0.123
Sample Size	2685		3921		4841		9364	
Mean of Y	-0.33		-0.17		-0.02		-0.06	
R-Squared	0.37		0.32		0.16		0.09	

	Condition Index		Economic Index	
Panel C: Triple Difference	Coef.	SE	Coef.	SE
Medicaid Exposure	-0.029	0.106	-0.144	0.145
Medicaid Exposure*Predicted AFDC	-1.373*	0.744	0.057	1.728
Sample Size	13293		13297	
Mean of Y	-0.01		0	
R-Squared	0.1		0.19	

Source: Panel Study of Income Dynamics 1968-2009. All parameters are from separate regressions. Models include a quadratic in age, gender, race, marital status, the full set of contextual controls described in the text, interview year fixed effects, state of birth fixed effects, year of birth fixed effects, and state specific trend in birth cohort. See main text for a description of models and data. Estimates are weighted and standard errors (SE) are clustered on state of birth. *p<0.1; **p<0.05; ***p<0.01.

Table 6.6. Migration Robustness Check: Results in Chronic Condition and Economic Index Regressions After Removing Observations that Move States in Early Childhood Period

	Low Income		Low Education		Moderate Income		High Education	
Panel A: Condition Index	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Medicaid Exposure	-0.239	0.157	-0.131	0.185	0.063	0.151	0.035	0.119
Sample Size	5513		6528		5000		9486	
Mean of Y	0.09		0.1		-0.05		-0.1	
R-Squared	0.21		0.18		0.15		0.09	
Panel B: Economic Index								
Medicaid Exposure	-0.235	0.221	-0.215	0.187	0.063	0.151	0.035	0.119
Sample Size	5576		6776		5000		9486	
Mean of Y	-0.24		-0.1		-0.05		-0.1	
R-Squared	0.34		0.31		0.15		0.09	

	Condition Index		Economic Index	
Panel C: Triple Difference	Coef.	SE	Coef.	SE
Medicaid Exposure	0.037	0.086	-0.180	0.152
Medicaid Exposure*Predicted AFDC	-0.891**	0.461	-0.061	1.124
Sample Size	16160		16324	
Mean of Y	-0.04		-0.03	
R-Squared	0.1		0.19	

Source: Panel Study of Income Dynamics 1968-2009. All parameters are from separate regressions. Models include a quadratic in age, gender, race, marital status, the full set of contextual controls described in the text, interview year fixed effects, state of birth fixed effects, year of birth fixed effects, and state specific trend in birth cohort. See main text for a description of models and data. Estimates are weighted and standard errors (SE) are clustered on state of birth. *p<0.1; **p<0.05; ***p<0.01.

Table 6.7. The Impact of Medicaid Exposure in Early Childhood on a Composite Index of Chronic Conditions, GEE Specifications						
	Condition Index			Economic Index		
	Coef.	SE	P	Coef.	SE	P
OLS (SE Clustered on state)	-0.36**	0.165	0.037	-0.11	0.237	0.652
GEE (SE Clustered on individual)	-0.42*	0.214	0.052	-0.06	0.21	0.771
Sample Size	5,926			6,227		

Source: Panel Study of Income Dynamics 1968-2009. All parameters are from separate regressions. See main text for a description of models and data. *p<0.1; **p<0.05; ***p<0.01.

Table 6.8 Alternative Predicted AFDC Probability Definitions, Condition Index and Economic Index Regressions

	1977-1978 PSID		1966-1967 March CPS		1968-1969 March CPS		1970 Census Long Form	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Condition Index Regressions								
Medicaid Exposure	0.004	0.090	0.12	0.126	-0.01	0.087	0.001	0.089
Medicaid Exposure* Predicted AFDC	-0.88*	0.445	-2.24**	1.053	-0.88*	0.498	-1.11*	0.569
R2	0.10		0.10		0.10		0.10	
Economic Index Regressions								
Medicaid Exposure	-0.16	0.139	-0.18	0.208	-0.16	0.133	-0.16	0.137
Medicaid Exposure* Predicted AFDC	-0.074	1.099	0.09	2.47	0.05	1.278	0.16	1.447
R2	0.19		0.19		0.19		0.19	
Predicted AFDC Descriptive Statistics (Mean, Min, Max)	.05, .01, .44		.07, .04, .25		.04, .004 ,.46		.04,.01,.42	
Predicted AFDC Strata	Characteristics of Household Head: Age (<25, 25-44, 45+;) Race (Non-White, White); Marriage (No, Yes); Education (LT HS, HS+)							

Source: Panel Study of Income Dynamics 1968-2009. All parameters are from separate regressions. Models include a quadratic in age, gender, race, marital status, the full set of contextual controls described in the text, interview year fixed effects, state of birth fixed effects, year of birth fixed effects, and state specific trend in birth cohort. See main text for a description of models and data. Estimates are weighted and standard errors (SE) are clustered on state of birth. *p<0.1; **p<0.05; ***p<0.01. The alternative predicted probabilities are merged to the PSID by the characteristics described in the last row of the table. In the PSID, the adult's childhood household head is used as the linking key. Sample size is 17,935.

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APPENDIX

The appendix provides additional information on data sources and results described in the main text. Section 1 describes data sources and Section 2 provides tables referenced in section 1 of the appendix and the full set of detailed regression results and robustness checks described in the main body of the text.

I. Auxiliary Data

Micro-data from the National Health Interview Survey, the National Natality Survyes and the Panel Study of Income dynamics is described in the main text. Below is a description of supplementary data that was merged with the micro-data. I first describe the contextual data and then describe the creation of the AFDC predicted probabilities and specific income dollar amounts that were primarily used in the NHIS analyses.

Medicaid Adoption Dates

The month and year a state implemented a Medicaid program comes from the Department of Health, Education and Welfare (1970). The dates in the DHEW volume were cross-referenced and supplemented by a list obtained from the Office of the Historian, Social Security Administration (SSA, 2013).

Population Estimates

State and county population estimates, used to scale contextual variables (i.e. number of physicians), were obtained from the 1960 Decennial Census (MPC, 2011) and 1969-1986 intercensal population estimates (SEER, 2012). Intercensal population estimates do not exist prior to 1969. Linear interpolation was used to fill in missing values.

Unemployment Rates and AFDC Benefit Standards and Caseloads

Annual state-level unemployment rates and AFDC maximum benefit levels for a family of 4, expressed in real 2000 year dollars, were obtained from Berry et al. (2004). The file contains information for 1960-1986. Detailed information on the source of these data is available from ICPSR. The PSID includes cohorts starting with 1955 and I assumed no annual change in unemployment and benefit standards from 1955 to 1960. This decision was based on visual inspection of line plots that showed no regular pattern by year in the 1960-1986 data. Unemployment and benefit standards (inflated with CPI-U) were measured in all PSID models as the average level across the early childhood period. In the NHIS and NNS they were measured as annual levels. Annual state level AFDC caseloads for 1960-1986 were obtained from the Administration of Children and Families (2013). Linear interpolation was used to fill in 1955-1959. In all PSID models caseloads were measured as the average per capita level during early childhood and in the NNS and NHIS they were measured as per capita annual levels.

Legalized Abortion

The date a state legalized abortion was obtained from Levine et al (1996). Five states legalized abortion (at request) in 1970. Roe v. Wade legalized abortion in all states in 1973. Legalized abortion is measured as an indicator equal to 1 if the observation state and years with legalized abortion.

Health Care Supply

The year and county specific number of physicians was obtained from the Area Health Resource File (HRSA, 2013). Missing years were filled in using linear interpolation. All active physicians engaged in patient care were counted. The year and county specific number of general, short-term hospitals for 1955-1975 was generously provided by Amy Finkelstein (2013). These data were compiled from reports of the American Hospital Association. Remaining years of data were obtained from the Area Health Resource File and any remaining missing cells were filled in with linear interpolation. Both the number of physicians and the number of hospitals were expressed in per capita terms and measured in all PSID models as the average level in the early childhood period. In the NHIS and NNS the county specific values were aggregated to the state level and annual levels were used.

Public Assistance Spending and Food Stamp Program Implementation Dates

Annual county-level per-capita spending on public assistance (expressed in 2000 year dollars), including AFDC, SSI, General Assistance and Food Stamps expenditures, was obtained from the Regional Economic Information System (BEA, 2012). The Bureau of Economic Statistics (BEA, 2013) makes these data available, in machine readable format, for the 1969-1986 time series. Data for 1959-1968 were hand entered from microfiche by Almond et al. (2011) and made available through the Review of Economics and Statistics Dataverse. Douglas Almond and Hillary Hoynes graciously provided guidance in using these data. County-years with missing data were imputed using interpolation.

County specific food stamp implementation dates (month and year) were also obtained from the replication data provided for Almond et al. (2011). In all models exposure to food stamps is measured as the fraction of months exposed during the early childhood period.

In all PSID models the level of per capita spending is measured as the average in the early childhood period and food stamp implementation is described as the fraction of time in the early childhood period that person was exposed to a food-stamp county, defined from their county of birth. In the NNS, spending was measured as state-level annual levels and food stamp implementation was measured as the fraction of a state's population that lived in county with a food-stamps program. Expenditures and food stamp implementation dates were not included in the NHIS analysis due to difficulty in merging them with the restricted use file.

War on Poverty Grants

County specific start dates (month and year) for a set of War on Poverty programs was obtained from Bailey and Goodman-Bacon (2013). Martha Bailey kindly provided the data and guided me in their use. The data originated with the National Archives Community Action Program and the National Archives Federal Outlays. See Bailey (2012) for a fuller description. The data describe the county specific start date of programs for fiscal years 1965 through 1981. I impose a number of assumptions on the data. I assume no programs began prior to calendar year 1964 (the year the Office of Economic Opportunity opened); that once a program began it was never destroyed; and that no programs began after 1981. I use information on several programs: Head Start, Community Health Centers, Family Planning grants, Maternal and Child Health/Maternal and Infant Health Grants, and job training grants. Exposure to each program is parameterized in the PSID as the fraction of months exposed during the early childhood period. In the NNS, program availability is measured as the fraction of the state's population that lives in a county with a program. The NHIS analyses do not include the grant's data.

Income Assignment in the NHIS

There are two limitations of the family income data in the NHIS. There is item-missing values which increases over time. The questionnaire item asks for categorical rather than precise income amounts and these categories change over time in manner that does not reflect the shifting shape of the income distribution. Categorical rather than specific dollar amounts prevents a straight forward solution to inflation-adjustment and a flexible approach to defining the low-income population. In the following I describe my treatment of income in the NHIS. The description includes discussion of data through the 1986 NHIS, however, I ultimately only included the 1963-1980 NHIS in analyses.

Over the study period the NHIS gathered income information in two ways. From 1963-1981 a single question was asked that prompted respondents to describe what category best described their family income, from all sources, during the preceding 12 months. From 1982-1986 two questions were asked. First respondents were asked if they had above or below $20,000 and then were asked to place themselves into a detailed category.

The number and range of the categories changed over time. The levels were harmonized by the Integrated Health Interview Series (IHIS). Their harmonization rules made slight changes to the available categories in each year. The harmonized levels still contain a fair amount of variability from year to year. In 1963, the IHIS variable contained 8 levels beginning at <$1,000 and ending at >=$10,000. By 1970 there were 11 categories spanning <$1,000 to >=$25,000. In 1982-1986 there were 27 levels extending to >=$50,000. In all years, the bottom of the income range is coded in $1,000 increments and the top of the range into $5,000 or $10,000 increments.

NCHS did not change the categories fast enough to be consistent with the shifting income distribution. The category definitions do not always represent the same quantiles of the distribution. Therefore, due to income growth (both real and inflationary), the sample bunches up at the top code over time, until the categories are expanded and the distribution evens out. For example, in 1970 3% of the sample is at the top code of $25,000+. In 1981, one year before the top code was expanded to $50,000, the $25,000 top code had 36% of the sample. In 1982 the top code of $50,000 had 6% of the sample. Similarly, the bottom code contains a shrinking fraction of the sample over the study period.

Over time an increasing segment of the sample refused to provide income information. Table 2 describes the rate of missingness and information about the category definitions for the 25 cross-sections used in this study. In 1963 5% of the sample lacked income information. By 1986, 13.4% were missing.

The NHIS defines families as people related by blood, marriage, or adoption. It is possible to have multiple families per household so long as the two groups are not related by blood, marriage, or adoption. However, the family relationship variables included in the NHIS are of limited quality (per experience and communication with NCHS). In many years there are multiple family heads in a family unit or no heads at all and the family income variable is not always constant by family id. The absence of a head could be because the family head was ineligible for interview (e.g. they were active duty military). However, there are a number of other potential reasons that could cause errors in the family groups—occurring in the field or during data processing.

Prior to imputing income, the family relationship variable was cleaned so that there was 1 family head per family ID. For reasons that will be clear later it was important to have a single family head per family. The cleaning algorithm first assigned heads when none were present. If there was a spouse in the family they were assigned as the head. If no spouse was present then the oldest person in the family unit with the same personal education as the education coded in the education of the family head variable was assigned as the head (ties going to the first person in the family roster). Finally, if no such person existed the oldest person in the family unit (ties going the first person in the family roster) was assigned to be the family head. A similar process was used to reassign people when there were multiple family heads. The excess family heads that were not the oldest were reassigned to be "Other relatives". During analysis, I will followed previous authors by conducting sensitivity tests that include households with only nuclear families as defined by the original relationship coding and came to very similar conclusions as those reported in the main text.

There have been two basic strategies for dealing with missing income data in the NHIS during years in which NCHS did not provide an imputed income variable. Authors such as Case, Lubotsky, and Paxson (2002) use list wise deletion. Others such as Currie et al (2008) implement imputation models that mimic the imputation methods used by NCHS to impute income since 1990. List-wise deletion is likely not the optimal method. It is unlikely that missing income data is completely random (MCAR). However, if we assume that they are missing at random conditional on a set of other covariates (MAR) we can use standard imputation methods to fill in missing income values.

The NCHS imputation method used for the 1990-1996 files uses "sequential hot deck imputation within matrix cells." Their documentation suggests that this method operates like regular hot deck but that the donor value is obtained by sorting the data and using the nearest non-missing neighbor, rather than randomly drawing the value, with replacement, from a donor matrix. Currie et al (for the 1985-1989 NHIS) use sequential regression multivariate imputation. The potential benefits of a regression approach rather than a non-parametric hot-deck strategy is it can leverage continuous covariates (despite the fact that Currie et al don't seem to leverage that advantage) and it can borrow strength across cells in the prediction step.

However, in practice, these advantages did not result in substantially different results and so I used a standard hot deck routine to impute family income due to its ease and to be closer in spirit to the 1990-1996 NHIS methods. Income was imputed only for family heads and their values were applied to all other family members. The covariates from the hot deck were based on the variables used by NCHS for the 1990 income imputation. Unlike NCHS I use the same imputation routine for everyone in the data. NCHS uses a separate covariate set depending on the age of the family reference person. The basic covariate set I used included age (0-34; 35-64; 65+), the number of workers in the family (0, 1, 2+), education (1963-1965: some high school vs no high school; 1966-1986: high school grad vs less than high school grad), race (white vs non-white); and aggregated geographic sample segment income. The education definition changed over time to accommodate changes in the NHIS education variable. Segment income, used by both NCHS and Currie et al, is the median income value in the sample segment, broken down into 3 quantiles. Because segment members are geographic neighbors they should have similar incomes. Using segment income is less appropriate in the public use data because the PSU variable is randomized (segments occur within PSU's) in order to protect confidentiality. There is no documentation on what randomization means in this context. However, in regression analyses the segment income variable was strongly predictive and highly significant of income among reported cases and it was more predictive than the median PSU income. That finding aligns with the notion that people in the same segments are closer neighbors than people within a PSU.

A list of more detailed covariates (including marriage, gender, higher detailed age) was attempted. However, the final covariate set was chosen to ensure that every strata had donors. In some years, the covariate strata had to be collapsed in order to ensure a minimum number of donors per strata. In 1970 and 1980 only 2 segment income quantiles were used and in 1973 segment income was removed entirely. In 1982-1986 when there were two family income variables, the broad income variable (less than 20k vs more than 20k) was imputed first and the detailed family income categories were imputed using the broad income variable. Both variables were imputed using the same covariates.

Table A1 includes results of a linear regression model predicting reported family income using the hot deck covariate set. The dependent variable was coded using an index coded in equally spaced ascending integers. All variables are highly significant in each year. The direction and maginitude of the results also make sense. The only peculiar result is for age. In 1964 being 35-44 is associated with less income than 0-34 years olds; in 1974 and there is no association; and in 1984 the direction was as expected. I expected 35-64 year olds to make the most money, even controlling for education, because they have more work experience than younger workers. By 1984, this pattern emerges. It remains unclear exactly what explains the pattern in 1964 and 1974. The poor quality family relationship data could be causing this result. Table A2 compares the distribution of imputed income data to list-wise deleted income (i.e. reported income) in selected years. The two distributions are nearly identical.

An important limitation to the imputation method is that I use a single imputed version of the variable. Multiple-Imputation methods exist that can account for imputation error. However, because I use a single variable, during analysis the imputation error will not be accounted for and my standard errors will be biased downwards, In practice this issue is often ignored. For example, using a single variable, the Census Bureau fully imputes income data in the CPS on roughly 25% missing data (in current years of data). No author that I know of has ever attempted to account for the bias this might cause to variance estimates. Currie et al. 2008 uses a single imputation in the NHIS and argues that this is appropriate because even if she inflates her standard errors by a 50% her conclusions are unchanged. (The standard rule of thumb is that using a single variable biases the standard errors by 15%, but this rule is sensitive to the level of missingness.) I expect this issue to have very minimal impact.

The categorization of income and the changing relationship of the categories to points on the income distribution make it difficult to create a single harmonized income variable, in real dollar terms, that has a meaningful interpretation in every year. Both Case et al. (2002) and Currie et al. (2008) assign precise dollar amounts to facilitate their analyses. I follow these authors and assign precise dollar amounts to the imputed NHIS income variable. I create two versions. The first method follows the previous papers. The second method uses hot deck to draw specific dollar amount from a known distribution so that the final NHIS has a smoother continuous distribution. In practice all analyses use the hot deck derived dollar amounts.

The first method follows previous methods for assigning precise dollar figures to the income categories using information from the 1964-1986 March Current Population Survey. For each year I determine the mean family income in the CPS for each income category and by a binary variable that reflects if the maximum education in the household was above or below 9 years (1963-1965) or 12 years (1966 forward). I assign this mean as the precise income amount in the NHIS data. This method creates a new categorical variable in the NHIS that has twice as many levels as the original income variable (within each original category there is now one income value for high education households and one for low education households).

Since the NHIS operated on a fiscal year schedule from 1963-1967 I append the fiscal year files to create calendar year files that are comparable to the CPS. The CPS asks about income in the previous calendar year and the NHIS about income in the 12 months preceding the interview – so the reference periods are not identical. There is no income information in the CPS prior to 1964 so the 1963 calendar year NHIS is assigned values from the 1964 CPS. The second half of Calendar Year 1962 in the NHIS is missing a precise income value and is excluded from all analyses. There are a couple of additional limitations. Prior to 1968 the CPS only includes 14+ year olds in the sample so the two samples are not exactly comparable. Secondly, the family groups in the CPS are different than the NHIS making the family income variables not exactly comparable.

With these limitations in mind, after assignment, the mean income from NHIS tracks relatively closely to the mean income in the CPS (Figure A1) for both the total population and a low income population defined as having income below $14,425 in 2000 year dollars (roughly the AFDC needs standard in a generous state in 1970). In the full population, the NHIS consistently underestimates income from the CPS. This issue has been described by previous researchers (Czaijka et al 2006) and was expected. The relationship between the NHIS and CPS for the low-income group is not consistent by year, but it too tracks relatively closely with the CPS.

As a final examination of the income imputation and assignment process demographic characteristics of the CPS and NHIS by low-income status in selected years. Since the two surveys differ in many respects the demographics should not exactly align. However, a large difference in a specific income group that was not apparent in the total samples would signal an issue in the imputation or assignment models. For the most part the demographics between the two surveys match closely. The low-income sample in the NHIS was more likely to be married than the low-income group in the CPS and this relationship was not as strong in the total population. However, in the context of the full set of results that shows very little demographic differences, the difference in marriage is likely the result of measurement of marriage in the survey and not the imputation or assignment process.

The second method used hot deck to assign precise dollar amounts. For each year, the NHIS data and the CPS data were stacked and the missing continuous income variable found in the CPS was imputed onto the NHIS. The hot deck covariates were the imputed income categories and the maximum educational attainment in the household. More detailed covariates were not possible because I chose to use narrow income categories that yielded small cell sizes. Narrow income categories are likely the most predictive covariate of continuous income.

Figure A2 shows that mean income from the hot deck method tracks exactly with the mean assignment method (sometimes called “cold deck”). Figure A3 shows the value of the hot deck method over mean assignment. Hot Deck did a fairly good job of mimicking the CPS distribution whereas the 25th, 50th, and 75th percentile from the mean assignment method jumps around from year to year.

Predicted ADFC Participation in the NHIS

In the NHIS I impute the probability of AFDC participation onto individual cases using 3 methods. A robustness exercise in the Chapter 6 (the PSID analyses) also uses

AFDC information described here. Information about AFDC participation comes from the March Current Population Survey. There are several weaknesses in the CPS data.

1. AFDC data is completely missing from 1963-1966.
2. There is a major measurement change in the AFDC variable starting in 1968 that renders the 1966 and 1967 variable incomparable to later years.
3. The CPS does not include people under the age 14 prior to 1968. While the original CPS micro-data appears to have a variable indicating the presence of children in the household (even if they were not included in the survey), this variable is not included in IPUMS-CPS and obtaining the original data was overly time consuming. Thus, an important predictor of AFDC participation is not available.
4. The CPS data has a different family unit definition and a different family relationship variable than the NHIS
5. AFDC participation is universally underreported in survey data.

Prior to imputing AFDC participation onto the NHIS, the CPS data were cleaned. To compensate for the fact that during the study period the CPS defined relationships between individuals at the household and not the family level the CPS data were restricted to households that contained only the household head’s nuclear family members (any combination of a household head, their spouse, and their own children). Between 1968-1986 you are considered to have AFDC if you or you or your spouse report “Welfare Income” (which could come from a number of sources, but primarily AFDC) or if your parents report “Welfare Income” and you are a child of the household head and less than 18 years old. All people 65 years old and older were considered not to have AFDC even if they reported some “Welfare Income”.

Next, AFDC participation was imputed in the CPS for years 1964-1967 using hot deck. 1963 was left missing because there is no family income data in 1963 and I did not want the imputed AFDC variable to be distributed irrespective of income. The two years following the year to be imputed were used to create donors. For example 1967 was imputed using 1968 and 1969, 1966 was imputed using the imputed 1967 file and the non-imputed 1968 file and so-forth. This rolling donor scheme helps to smooth out the demographics over time.

To account for the fact that the CPS sample did not include children less than 14 prior to 1968 the imputation was done just for household heads. The hot deck covariates included family income category (defined as being in the lower third of that year's income distribution vs the top 2/3rds), age (less than 25, 25-39, 40+) race (white vs non white), if there were any workers in the household, the maximum educational status in the household (at least high school completion versus lower) and marital status. The same covariate set was used in all years except 1964 in which education was dropped to facilitate a large enough donor pool.

Table B1 presents the rate of AFDC participation from the original 1966 to 1969 data (prior to cleaning and imputation) along with the mean family income by AFDC status. The table includes only household heads. In 1969, the CPS estimates about 2.5% of household heads had AFDC and their average income was about $3,500. The AFDC needs standard in 1970 for a generous state was about $3,250 so the income figure roughly matches expectations (recall the income deeming in the AFDC program disregarded some income). The prevalence rate is likely an underestimate caused by the under-reporting of welfare income. The prevalence of AFDC is lower in 1968 and the average income moves down a bit. However, in 1967 the rate of AFDC jumps to just less than 9% and the family income goes up to $6,500. While, the rate goes back down to 3% in 1966 the average family income remains unrealistically high. Table B1 demonstrates why the 1966 and 1967 AFDC indicator was set to missing for those years despite the fact that there was an indicator for welfare income.

Table B2 present the same statistics for selected years from the 1964-1986 data after it had been cleaned and imputed. The series after 1967 is smooth and consistent. During the imputation years it jumps around more. However, the problems that were observed in the original 1966 and 1967 data are mostly gone (although family income in 1967 is a bit high).

Two methods are used to assign AFDC participation probabilities from the CPS to the NHIS. The first method assigns the participation rates by demographic groupings at the national level. The second method assigns them by groupings at the state level. The later has the advantage of picking up state level variation in the generosity of AFDC eligibility rules and take-up. The former has the advantage of larger cell sizes and thus more refined demographic groupings. In both methods the probabilities are assigned to the family head and all family members will share the same participation rate as the head.

Inflating an intent-to-treat estimate to a treatment-on-the-treated estimate using group level participation rates has been used by Bleakly (2007), Hoynes and Schazenbach (2009), and Hoynes, Schanzenbach and Almond (2012). In Hoynes, Schanzenbach, and Almond’s food stamp paper based on PSID data, they define the participation rate by 12 groups: education (3 levels), race(2 levels), and marital status (2 levels). While not apparent in the current draft of their paper, previous versions imply that the food stamp participation rates were obtained from a single year of the PSID. Given that CPS and NHIS are cross-sections it is possible to allow the participation rates to vary by year.

The national level demographic groups used to assign AFDC participation rates to the NHIS were sex, the maximium education in the household (2 levels), race (white vs non white), and an indicator of whether there was any workers in the family. This created 16 groupings. Marital status was left out because in both the CPS and the NHIS males were nearly always defined as the family reference person when the head of the family is married. Marriage and gender of the household head are synonymous. I also considered income categories. However, because income might be endogenous to AFDC participation when someone is at the income margin of eligibility I decide to leave it out. I do include work status which could be endogenous in the same way as income. The benefit of work status is that it vastly improves the variation in the predicted AFDC probabilities.

The mean, min and max cell sizes from the CPS were inspected to ensure adequate sample size for producing statistically reliable estimates. There was only only a single cell in a single year that is arguably low (n=32), but overall the cell size looks adequate (generally over 100). Table B3 reports the AFDC participation rate by demographic merge group for selected years (sorted by the 1965 mean). The means increase across year, but the ranking of the groups by the participation rate is relatively constant across years. This means that using a single year to assign probabilities should return similar results to allowing the rates to vary across year because ultimately it is the ranking of the groups that matters. The patterns across the groups at a cross-section are mostly consistent with expectations. Non-whites have a higher probability of participation compared to whites. Workers have a lower probability compared to non-workers. Women have a higher chance than men.

Because eligibility rules and benefit standards are defined at the state level a significant amount of AFDC variability is determined by state of residence. The addition of state to the participation groups significantly reduces the cell size. Using the full set of demographic strata and state creates between 25 to 148 cells with 0 observations, depending on the year. Between 73 and 135 cells have more than 0, but less than 10 observations. Nonetheless, because of the prominence of state in AFDC participation, a separate AFDC predicted probability was created. To increase cell size I reduced the stratification to state and sex.

The CPS data does not identify the full distribution of states in every year. From 1963 to 1967 and from 1977-1986 each state can be identified. From 1968 through 1976 states are grouped and the grouping changes in 1973. States are grouped with their neighbors and large states are uniquely identified in each year. To generate AFDC rates for each state in each year, the state-level rates between 1968 and 1976 were interpolated. Every state was interpolated so that the interpolation error would be similar across states. A simple interpolation method was used. 1967 and 1977 were the reference years and a linear interpolation for each cell was generated. There are other alternatives to this method such as shirnking the interpolated value to the value of the state group, etc. However, the key to this variable will be its rank and it was unclear if more nuanced methods would produce such a better rank to justify the effort. Figure B1 plots the AFDC rate for men and women in the state of California. The top panel has the actual values from the survey (CA is identified in each year). The bottom panel has the interpolated series. The point of the figure is to show that, for California, the interpolated series, while not a perfect match for the actual evaluation of AFDC rates across time, is a fair approximation.

The final step was to fill in cells where the rate was 0 due to small cell size. 105 of the total 2346 cells were 0. In all instances the small cell sizes occurred for women. To handle this problem the small cells (less than 20 observations) were replaced by the AFDC rate for children defined from the administrative count of child AFDC beneficiaries divided by the child population count.

Chapter 4 describes the mean, max, min, and standard deviation of each of these measures after they have been merged on to the NHIS.

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II. Detailed Tables

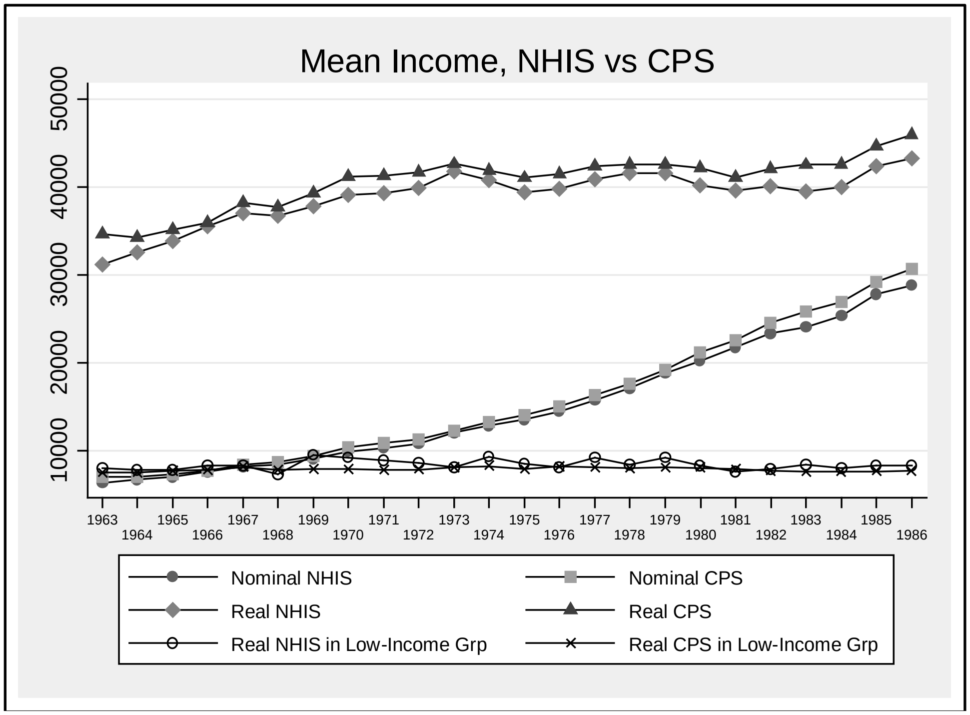
This section presents tables and figures referenced by the appendix and in the main text. The tables are as follows.

Tables and Figures A1-B1. Appendix Tables and Figures
Tables C1-C3. NHIS Robustness Checks (Chapter 4)
Tables C4-C8. NHIS Regression Models (Chapter 4)
Table D1. NNS Regression (Chapter 5)
Tables E1-E11. PSID Regression Models (Chapter 6)

Table A1. Coefficients from Linear Regression of Income on Hot Deck Covariates, NHIS			
	1964	1974	1984
Age			
35-64	-0.083***	0.025	0.649***
65+	-0.908***	-0.740***	0.128
# Workers in Family			
1	1.259***	1.772***	4.565***
2+	1.885***	2.459***	6.854***
Education			
High	0.274***	0.283***	0.873***
Race			
White	0.850***	0.711***	1.297***
Segment Income			
2nd Quant	1.322***	1.654***	5.605***
3rd Quant	2.438***	2.599***	8.761***
Intercept	3.248***	6.329***	8.157***
R^2	0.464	0.476	0.548

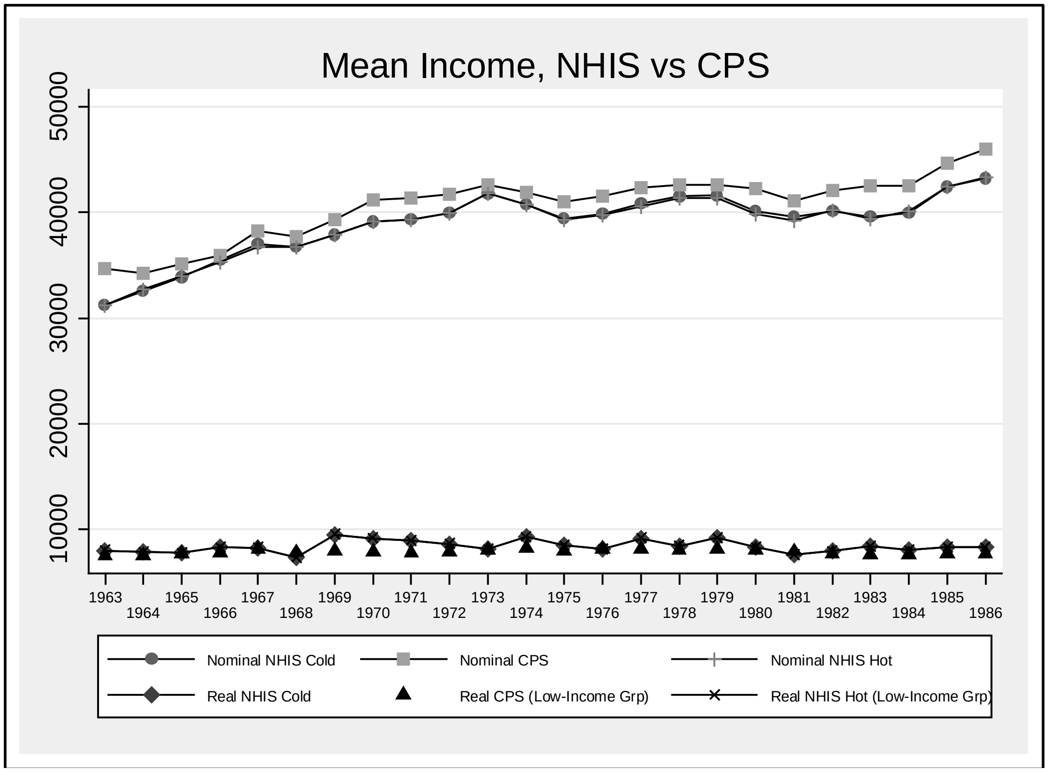
Source: National Health Interview Survey, selected years. Estimates are weighted and significance testing computed from design based standard errors. *p<0.05; **p<0.01; ***p<0.001

Figure A.1. Average Real and Nominal Income (Mean Assignment Method)



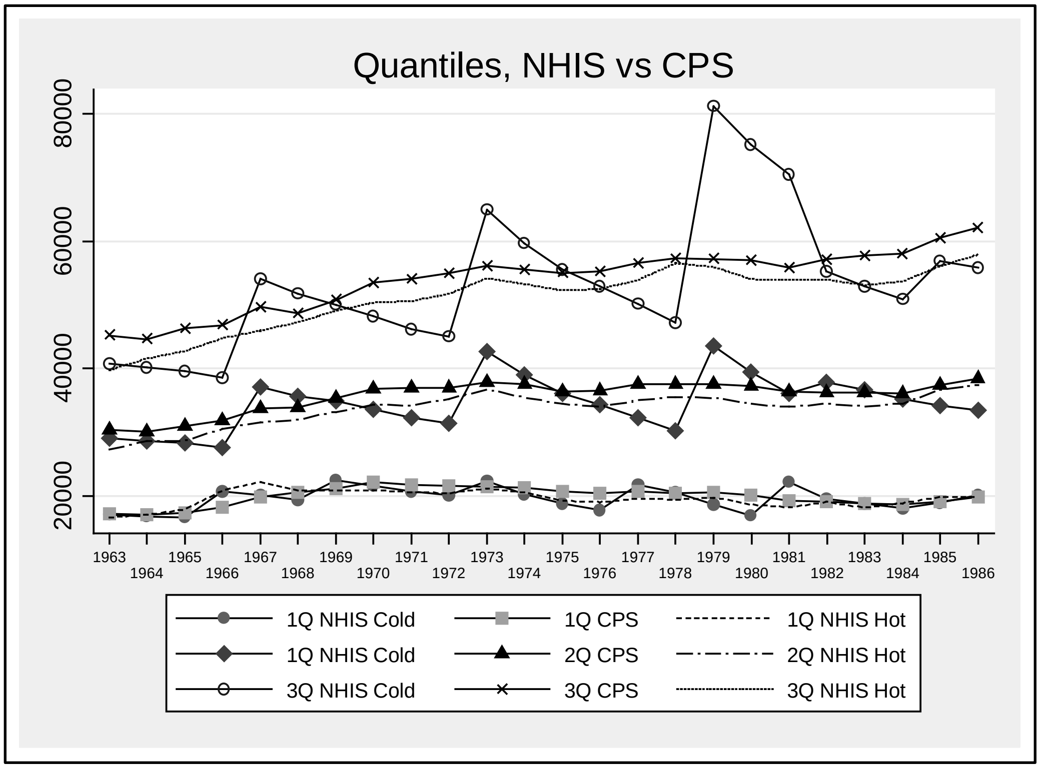
Source: March Current Population Survey, 1964-1987 and National Health Interview Survey, 1963-1986. Estimates are weighted.

Figure A2. Real and Nominal Income, Mean Assignment vs Hot Deck Assignment



Source: March Current Population Survey, 1964-1987 and National Health Interview Survey, 1963-1986. Estimates are weighted.

Figure A3. Quantiles in Real 2000 Dollars, Mean Assignment vs Hot Deck.



Source: March Current Population Survey, 1964-1987 and National Health Interview Survey, 1963-1986. Estimates are weighted.

Table B1. AFDC Rate and Family Income by AFDC Status, Original 1966-1969 CPS (Household Heads)			
	Mean	Family Income	
Year		No AFDC	AFDC
1966	0.03	6892	6515
1967	0.08	7678	6517
1968	0.02	7934	3149
1969	0.02	8675	3502
Source: March Current Population Survey, selected years. Estimates are weighted.			

Table B2. AFDC Rate and Income, CPS household heads

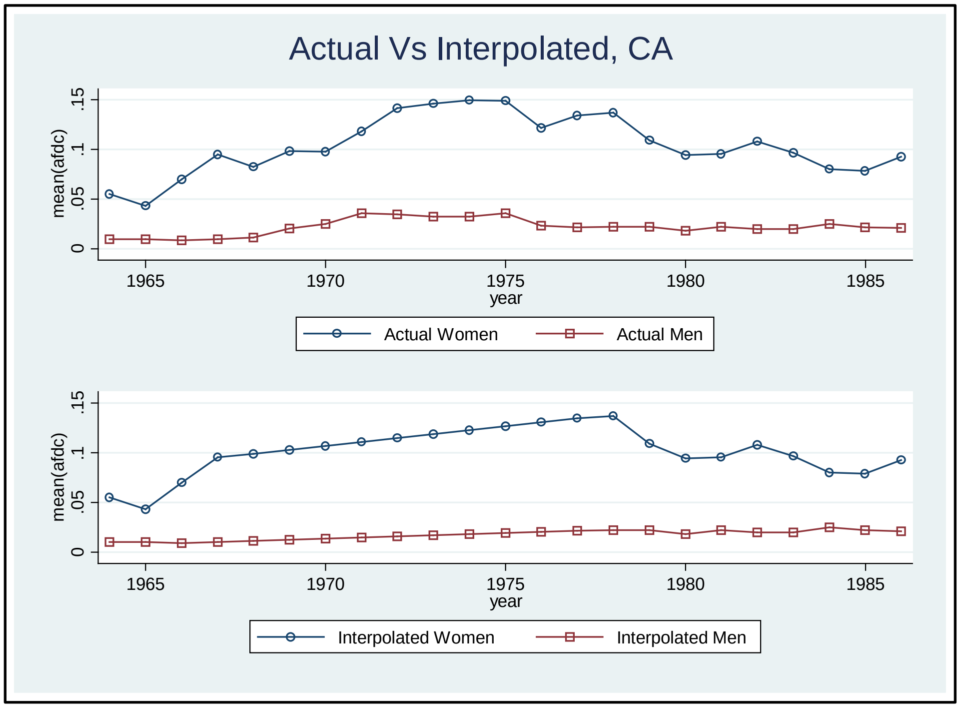
	Mean	Nominal Family Income	
		No AFDC	AFDC
1964	0.019	6360	2622
1965	0.015	6628	3088
1966	0.021	6961	2972
1967	0.018	7650	3598
1968	0.023	7934	3149
1969	0.027	8675	3502
1970	0.029	9492	3564
1971	0.036	9948	3915
1972	0.039	10403	3898
1980	0.034	19438	6586
1981	0.037	20945	6554
1982	0.037	22924	6806
1986	0.038	29130	6970

Source: March Current Population Survey, selected years. Estimates are weighted.

Table B3. AFDC Means and Ranks for Selected CPS Years	1965		1969		1975		1979	
	Mean	Rank	Mean	Rank	Mean	Rank	Mean	Rank
Women, High Ed, Nonwhite, No Workers	0.28	1	0.43	1	0.56	1	0.52	1
Women, Low Ed, Nonwhite No Workers	0.2	2	0.42	2	0.46	2	0.41	2
Women, High Ed, Nonwhite, Workers	0.14	3	0.13	5	0.15	6	0.14	5
Men, High Ed, Nonwhite, No Workers	0.14	4	0.11	6	0.16	5	0.15	4
Women, Low Ed, Nonwhite, Workers	0.11	5	0.19	3	0.27	3	0.21	3
Men, Low Ed, Nonwhite, No Workers	0.08	6	0.19	4	0.16	4	0.1	6
Women, Low Ed, White, No Workers	0.04	7	0.09	7	0.13	7	0.1	7
Men, Low Ed, Nonwhite, Workers	0.03	8	0.04	10	0.06	10	0.04	10
Women, Low Ed, White, Workers	0.02	9	0.05	8	0.1	8	0.1	8
Women, High Ed, White, No Workers	0.02	10	0.05	9	0.1	9	0.08	9
Men, Low Ed, White, No Workers	0.02	11	0.04	11	0.06	11	0.03	12
Men, High Ed, Nonwhite, Workers	0.02	12	0.02	12	0.03	14	0.02	14
Women, High Ed, White, Workers	0.01	13	0.02	13	0.05	12	0.04	11
Men, Low Ed, White, Workers	0.01	14	0.02	14	0.03	15	0.02	15
Men, High Ed, White, No Workers	0.01	15	0.02	15	0.04	13	0.03	13
Men, High Ed, White, Workers	0	16	0	16	0.01	16	0.01	16

Source: March Current Population Survey, selected years. Estimates are weighted. Table is sorted by the 1965 Rank. High Ed is High School graduate or more. Low Ed is less than high school completion.

Figure B1. Actual vs Interpolated AFDC rates in CA.



Source: March Current Population Survey, 1964-1987

Table C.1 AME of Medicaid on Annual Doctor Visits, by Income and Age

	Low Income			Moderate Income		
Children 0-5	AME	SE	P	AME	SE	P
No Medicaid	(Ref)			(Ref)		
Medicaid	0.00573	0.015	0.712	-0.00503	0.006	0.457
Sample Size	18,674			71,463		
Base Rate	0.06			0.0774		
Base SE	0.01			0.003		
Mothers	AME	SE	P	AME	SE	P
No Medicaid	(Ref)			(Ref)		
Medicaid	0.02567	0.016	0.129	0.01301	0.008	0.110
Sample Size	16,786			78,296		
Base Rate	0.27			0.24		
Base SE	0.011			0.005		

Source: National Health Interview Survey 1963-1980. Standard errors (SE) account for sample design strata and clusters. Average-marginal-effects (AME) are computed from logistic regression models (see main text for a description of the models and samples). *p<0.05; **p<0.01; ***p<0.001.

Table C2. Comparison of Standard Error Methods		
	State Clusters	**Survey Design Clusters and Strata**
Low Income Children 0-5		
Logistic Coefficients	0.48233**	0.48233**
SE	0.1786	0.1673
P-Value	0.0096	0.0041
AME	0.03303**	0.03303**
SE	0.0115	0.0110
P-Value	0.0061	0.0020
Low Income Mothers		
Logistic Coefficients	-0.0381	-0.0381
SE	0.1051	0.1049
P-Value	0.7185	0.7165
Margins	-0.0070	-0.0070
SE	0.0194	0.0194
P-Value	0.7198	0.7175

Source: 1963-1980 NHIS. This table displays results from separate logistic regressions and average marginal effect (AME) calculations (see main text for a description of the models and samples). State clusters have standard errors (SE) clustered on state. Survey design uses survey based PSUs and strata that are constant within survey design periods and differ across periods.

Table C3. Event Time Estimates, by Income								
	Low Income Age 0-5				Moderate Income Age 0-5			
			95% Confidence Interval				95% Confidence Interval	
Marginal Effects from Logistic Regression	AME	SE	Lower	Upper	AME	SE	Lower	Upper
3 Years or More Prior to Adoption	-0.010	0.018	-0.045	0.025	-0.002	0.005	-0.012	0.009
2 Years Prior to Adoption	-0.004	0.015	-0.034	0.025	0.004	0.005	-0.007	0.015
1 Year Prior to Adoption	(Ref)				(Ref)			
Implementation Year	0.017	0.014	-0.012	0.045	0.002	0.005	-0.008	0.011
1 Year After	0.015	0.017	-0.019	0.049	0.001	0.006	-0.011	0.013
2 Years After	0.045	0.020	0.007	0.084	-0.005	0.007	-0.019	0.009
3 or More Years After	0.026	0.016	-0.006	0.059	0.015	0.008	-0.001	0.032
Sample Size	24,520				101,793			
	Low Income Age 0-5				Moderate Income Age 0-5			
	mean	SE	Lower	Upper	mean	SE	Lower	Upper
Unconditional Means								
3 Years or More Prior to Adoption	0.058	0.003	0.052	0.065	0.073	0.002	0.069	0.076
2 Years Prior to Adoption	0.071	0.007	0.057	0.086	0.078	0.003	0.071	0.085
1 Year Prior to Adoption	0.067	0.011	0.045	0.089	0.073	0.003	0.067	0.079
Implementation Year	0.084	0.010	0.065	0.103	0.073	0.003	0.067	0.080
1 Year After	0.085	0.008	0.068	0.101	0.069	0.003	0.063	0.075
2 Years After	0.114	0.011	0.093	0.135	0.068	0.004	0.060	0.076
3 or More Years After	0.092	0.003	0.087	0.097	0.078	0.002	0.075	0.081
Sample Size	25,129				102,921			

Source: NHIS 1963-1980. Standard errors (SE) account for sample design strata and clusters. Implementation year is defined as Medicaid adoption in the year of the hospital visit reference period. See main text for a description of the models and samples.

Table C4. Logistic Regressions of Any Annual Hospital Visits, by Income and Age

	Low Income Age 0-5			Moderate income Age 0-5			Low Income 0-18			Moderate Income 0-18		
	Coeff.	SE	P	Coeff.	SE	P	Coeff.	SE	P	Coeff.	SE	P
NoMedicaid	(Ref)			(Ref)			(Ref)			(Ref)		
Medicaid	0.48233**	0.167	0.004	-0.04162	0.072	0.561	0.23203*	0.117	0.047	0.00706	0.051	0.890
Female	(Ref)			(Ref)			(Ref)			(Ref)		
Male	0.24473***	0.049	0.000	0.26771***	0.024	0.000	-0.05442	0.033	0.097	0.13104***	0.015	0.000
NonWhite	(Ref)			(Ref)			(Ref)			(Ref)		
White	0.16852**	0.062	0.007	0.18680***	0.049	0.000	0.21934***	0.040	0.000	0.22677***	0.031	0.000
NoHighSchool	(Ref)			(Ref)			(Ref)			(Ref)		
SomeHighschool	-0.004	0.062	0.954	0.09178*	0.039	0.018	-0.00475	0.041	0.907	0.07095**	0.024	0.003
HeadLessThan25	(Ref)			(Ref)			(Ref)			(Ref)		
Head2544	-0.13499*	0.060	0.024	-0.05632	0.038	0.142	-0.33082***	0.052	0.000	-0.18473***	0.034	0.000
Head4564	-0.03166	0.095	0.740	-0.07438	0.059	0.207	-0.54389***	0.065	0.000	-0.32210***	0.040	0.000
Head65Over	-0.25759	0.205	0.209	-0.00009	0.149	1.000	-0.75970***	0.111	0.000	-0.23201**	0.088	0.008
Age0	(Ref)			(Ref)			(Ref)			(Ref)		
Age1	-0.49620***	0.073	0.000	-0.47178***	0.037	0.000	-0.47891***	0.072	0.000	-0.46512***	0.037	0.000
Age2	-0.84065***	0.080	0.000	-0.74962***	0.042	0.000	-0.80601***	0.080	0.000	-0.73367***	0.042	0.000
Age3	-1.03901***	0.089	0.000	-0.93480***	0.042	0.000	-0.98255***	0.089	0.000	-0.91468***	0.042	0.000
Age4	-1.03307***	0.082	0.000	-0.94565***	0.042	0.000	-0.97080***	0.081	0.000	-0.91742***	0.042	0.000
Age5	-1.17958***	0.089	0.000	-0.81980***	0.040	0.000	-1.09614***	0.088	0.000	-0.78540***	0.040	0.000
PerCapHosp	1.46089	1.502	0.331	-0.2443	0.937	0.794	0.32773	1.146	0.775	-0.83463	0.583	0.153
PerCapMD	-6.21678	5.975	0.299	-3.67231	2.801	0.190	-4.10403	3.847	0.287	-3.73309	2.155	0.084
PerCapPA	-0.00043	0.001	0.689	0.00007	0.001	0.903	-0.00057	0.001	0.408	0.00018	0.000	0.631
AFDCBen	-0.0003	0.000	0.364	-0.00029	0.000	0.083	-0.00007	0.000	0.786	-0.00039**	0.000	0.002
AFDCCase	-0.00115	0.006	0.858	-0.00167	0.003	0.602	0.00396	0.004	0.301	-0.00284	0.002	0.164
AL	(Ref)			(Ref)			(Ref)			(Ref)		
AR	0.12284	0.240	0.609	-0.36706	0.196	0.062	0.16167	0.198	0.415	-0.12488	0.160	0.436
CA	2.30183**	0.764	0.003	0.74437*	0.364	0.041	1.28684*	0.550	0.020	0.48295	0.294	0.101
CO	1.81850**	0.679	0.008	0.63755	0.339	0.061	1.20002*	0.489	0.015	0.3984	0.266	0.135
CT	0.99138	0.940	0.292	0.19645	0.475	0.679	0.78974	0.584	0.177	0.00498	0.310	0.987
DE	1.71707**	0.568	0.003	0.29278	0.301	0.331	0.96009**	0.327	0.003	-0.05884	0.182	0.746
DC	2.92971	1.844	0.113	1.2303	1.009	0.223	1.69193	1.217	0.165	1.00271	0.851	0.239

FL	0.93677*	0.404	0.021	0.19163	0.210	0.361	0.43013	0.278	0.122	-0.07446	0.151	0.623
GA	0.57376*	0.229	0.012	0.07571	0.152	0.618	0.15868	0.162	0.327	0.12241	0.099	0.216
ID	0.73481	0.803	0.361	0.56339	0.372	0.131	0.26367	0.552	0.633	0.51578	0.293	0.080
IL	0.9954	0.700	0.156	0.49281	0.320	0.124	1.01621*	0.454	0.026	0.33148	0.218	0.128
ID	0.70128	0.682	0.304	0.26334	0.284	0.354	0.79945	0.423	0.059	-0.01809	0.178	0.919
IO	0.49778	0.720	0.490	0.72949**	0.267	0.007	0.86543	0.478	0.071	0.55558**	0.182	0.002
KS	-0.21495	0.864	0.804	0.57128	0.391	0.145	0.45039	0.637	0.480	0.44998	0.247	0.069
KY	0.50160*	0.228	0.029	-0.19811	0.155	0.201	0.19693	0.185	0.287	-0.12741	0.119	0.284
LA	0.3558	0.292	0.224	0.21893	0.157	0.163	0.13375	0.200	0.504	0.16855	0.109	0.122
ME	0.96558	0.541	0.075	0.45788	0.286	0.110	0.60952	0.404	0.132	0.25402	0.185	0.171
MD	0.94209	0.838	0.262	0.42572	0.387	0.272	0.47361	0.521	0.364	0.19175	0.270	0.477
MA	1.80672*	0.918	0.050	0.6976	0.454	0.125	0.98385	0.610	0.107	0.42744	0.314	0.174
MI	1.11578	0.695	0.109	0.74190*	0.295	0.012	0.84931	0.444	0.056	0.40679*	0.196	0.038
MN	0.55482	0.793	0.485	0.77271*	0.327	0.019	0.61587	0.563	0.274	0.64555**	0.213	0.003
MS	-0.01194	0.248	0.962	-0.03242	0.202	0.872	0.01831	0.211	0.931	-0.10272	0.126	0.416
MO	0.27677	0.624	0.658	0.32832	0.272	0.229	0.65277	0.401	0.104	0.04215	0.174	0.809
MT	1.50815	1.026	0.142	1.12265*	0.568	0.049	1.31054	0.848	0.123	1.13767**	0.378	0.003
NE	0.28624	0.843	0.734	0.47222	0.416	0.257	0.83824	0.625	0.181	0.40211	0.259	0.121
NV	0.41121	0.783	0.600	-0.44338	0.617	0.473	0.54718	0.879	0.534	-0.26913	0.356	0.450
NH	1.47763*	0.608	0.015	0.6306	0.340	0.064	0.83696	0.454	0.066	0.28135	0.214	0.190
NJ	1.73871**	0.643	0.007	0.24122	0.371	0.516	0.75169	0.462	0.105	0.07782	0.252	0.757
NM	1.05741	0.586	0.072	0.78437**	0.258	0.003	0.72224	0.436	0.098	0.41689	0.220	0.059
NY	2.14798*	0.937	0.022	0.60401	0.498	0.226	1.20457	0.636	0.059	0.34918	0.362	0.335
NC	0.59718	0.312	0.057	0.04971	0.188	0.791	0.35378	0.222	0.112	-0.02782	0.121	0.819
ND	-0.62442	1.204	0.604	1.03322	0.620	0.096	0.72853	0.911	0.425	1.04844*	0.407	0.010
OH	0.91653	0.681	0.179	0.38949	0.319	0.223	0.89082*	0.442	0.044	0.17745	0.210	0.398
OK	0.13988	0.363	0.700	0.01254	0.232	0.957	0.32283	0.309	0.297	0.11363	0.147	0.439
OR	1.82533**	0.625	0.004	0.62773	0.329	0.057	1.13626*	0.487	0.020	0.36759	0.255	0.151
PE	1.30383*	0.607	0.032	0.2572	0.343	0.453	0.73951	0.435	0.089	0.12808	0.241	0.595
RI	1.23514	0.744	0.098	0.36326	0.372	0.330	0.79472	0.546	0.146	0.18373	0.274	0.502
SC	0.26392	0.291	0.364	-0.02964	0.197	0.880	0.15785	0.194	0.416	-0.29265*	0.116	0.012
SD	0.16179	1.201	0.893	0.86499	0.567	0.128	0.55262	0.877	0.529	0.97335**	0.355	0.006
TN	0.56452*	0.270	0.037	-0.24325	0.180	0.176	0.36119*	0.180	0.046	0.01108	0.113	0.922

TX	0.16064	0.292	0.583	-0.06188	0.166	0.710	0.115	0.227	0.613	-0.0101	0.107	0.925
UT	1.52860*	0.641	0.018	0.52242	0.306	0.088	1.01901*	0.447	0.023	0.29489	0.243	0.226
VT	0.85391	0.738	0.248	0.75145	0.418	0.073	-0.42715	0.592	0.471	0.32604	0.348	0.350
VA	0.64263	0.443	0.147	0.14871	0.250	0.552	0.54543	0.291	0.061	0.05312	0.171	0.756
WA	1.97263**	0.654	0.003	0.69333*	0.310	0.026	1.26299**	0.460	0.006	0.41202	0.253	0.105
WV	1.08789***	0.326	0.001	0.43287**	0.164	0.009	0.60427**	0.219	0.006	0.20833	0.124	0.094
WI	1.13117	0.692	0.103	0.66457*	0.277	0.017	1.09755*	0.438	0.013	0.52914**	0.180	0.003
WY	1.68073	0.940	0.075	0.23171	0.583	0.691	1.26486	0.745	0.090	0.74634*	0.343	0.030
1963	(Ref)			(Ref)			(Ref)			(Ref)		
1964	0.4001	0.372	0.283	0.20708	0.171	0.227	0.04103	0.291	0.888	-0.02005	0.112	0.858
1965	0.65973	0.450	0.144	0.17851	0.134	0.184	0.14386	0.300	0.631	-0.03314	0.079	0.674
1966	0.70131	0.428	0.102	0.18176	0.162	0.262	0.40619	0.298	0.173	0.07895	0.106	0.455
1967	1.01378	0.567	0.074	0.24193	0.145	0.095	0.30475	0.466	0.513	0.07718	0.105	0.462
1968	0.58674	0.448	0.191	0.11573	0.156	0.458	0.32816	0.311	0.292	-0.00888	0.117	0.939
1969	1.20062**	0.434	0.006	0.16683	0.190	0.380	0.6549	0.335	0.051	0.04953	0.123	0.686
1970	0.12724	0.484	0.793	0.33841	0.226	0.134	0.18892	0.414	0.648	0.18444	0.154	0.231
1971	0.221	0.560	0.693	0.26753	0.198	0.178	-0.11766	0.367	0.749	0.04258	0.155	0.783
1972	0.69457	0.485	0.153	0.46229*	0.223	0.039	0.31563	0.348	0.365	0.19362	0.153	0.206
1973	0.61868	0.492	0.209	0.12348	0.209	0.555	0.25801	0.343	0.452	0.01345	0.165	0.935
1974	0.56649	0.576	0.326	0.65443**	0.231	0.005	0.45306	0.414	0.274	0.25759	0.153	0.094
1975	0.63996	0.545	0.241	0.15093	0.213	0.478	0.29793	0.392	0.448	0.05818	0.180	0.747
1976	1.12199*	0.557	0.045	0.66949**	0.243	0.006	0.57778	0.392	0.142	0.2845	0.185	0.126
1977	0.68142	0.536	0.204	0.51858*	0.241	0.032	0.36293	0.392	0.354	0.18696	0.175	0.285
1978	0.68611	0.591	0.246	0.35979	0.247	0.146	0.27042	0.384	0.482	0.11691	0.186	0.529
1979	0.79247	0.570	0.165	0.26828	0.274	0.328	0.20524	0.400	0.608	-0.01477	0.221	0.947
1980	1.06757	0.570	0.062	0.24019	0.277	0.386	0.4046	0.408	0.322	-0.13917	0.237	0.557
1967fy	0.06486	0.444	0.884	0.00545	0.156	0.972	0.00568	0.374	0.988	-0.15205	0.128	0.235
Midwest*1963	0.41155	0.703	0.559	-0.13763	0.214	0.521	-0.07134	0.514	0.890	-0.20335	0.154	0.187
Midwest*1964	0.694	0.630	0.272	-0.31422	0.225	0.163	0.02403	0.416	0.954	-0.16956	0.187	0.365
Midwest*1965	0.26753	0.648	0.680	-0.17628	0.192	0.360	-0.25086	0.456	0.583	-0.1989	0.140	0.156
Midwest*1966	-0.21714	0.667	0.745	-0.29	0.180	0.108	-0.65133	0.413	0.116	-0.38356*	0.155	0.014
Midwest*1967	0.15436	0.815	0.850	-0.16038	0.206	0.436	-0.03153	0.577	0.956	-0.18562	0.160	0.246
Midwest*1968	-0.01085	0.638	0.986	-0.15856	0.216	0.464	-0.43306	0.426	0.310	-0.26376	0.171	0.124

Midwest*1969	-0.36985	0.603	0.540	-0.02005	0.249	0.936	-0.65904	0.422	0.119	-0.21807	0.176	0.217
Midwest*1970	0.77041	0.666	0.248	-0.57732*	0.275	0.036	-0.28129	0.525	0.593	-0.42571*	0.192	0.027
Midwest*1971	0.44184	0.672	0.511	-0.24273	0.236	0.305	0.01452	0.441	0.974	-0.19979	0.182	0.273
Midwest*1972	0.34438	0.596	0.564	-0.47956*	0.244	0.050	-0.10066	0.418	0.810	-0.30768	0.183	0.093
Midwest*1973	0.11166	0.630	0.859	-0.12697	0.245	0.604	-0.24142	0.420	0.566	-0.12728	0.185	0.492
Midwest*1974	0.45541	0.676	0.501	-0.4123	0.257	0.109	-0.40806	0.476	0.392	-0.21928	0.174	0.209
Midwest*1975	0.40659	0.614	0.508	0.02072	0.242	0.932	0.01013	0.428	0.981	-0.07075	0.184	0.701
Midwest*1976	-0.36704	0.611	0.548	-0.50130*	0.249	0.045	-0.95920*	0.419	0.023	-0.25194	0.191	0.188
Midwest*1977	0.06212	0.622	0.921	-0.47339	0.270	0.080	-0.41963	0.439	0.340	-0.43622*	0.184	0.018
Midwest*1978	0.45787	0.644	0.477	-0.27028	0.260	0.299	-0.04098	0.410	0.920	-0.11908	0.181	0.511
Midwest*1979	0.39159	0.617	0.526	-0.20571	0.263	0.434	0.06458	0.441	0.884	-0.09022	0.200	0.651
Midwest*1980	0.26488	0.586	0.652	-0.29024	0.269	0.281	-0.05693	0.398	0.886	-0.00753	0.208	0.971
South*1963	0.27886	0.567	0.623	0.10282	0.235	0.662	-0.078	0.422	0.854	-0.01083	0.177	0.951
South*1964	0.01125	0.512	0.982	0.021	0.225	0.926	0.01642	0.354	0.963	-0.00213	0.201	0.992
South*1965	0.01363	0.539	0.980	-0.0339	0.203	0.867	0.09849	0.377	0.794	0.02043	0.147	0.889
South*1966	0.31731	0.481	0.510	-0.0755	0.217	0.728	-0.00403	0.379	0.992	-0.17781	0.171	0.300
South*1967	-1.08287	0.713	0.129	0.03713	0.189	0.845	-0.40714	0.538	0.450	-0.10528	0.150	0.483
South*1968	-0.44615	0.489	0.362	0.03794	0.213	0.859	-0.10409	0.335	0.756	-0.108	0.174	0.536
South*1969	-0.85193	0.460	0.065	0.06159	0.241	0.798	-0.46162	0.363	0.204	-0.13947	0.171	0.416
South*1970	0.61373	0.512	0.232	-0.21402	0.279	0.443	0.13162	0.448	0.769	-0.21405	0.198	0.281
South*1971	0.48336	0.532	0.364	0.24198	0.236	0.306	0.45411	0.373	0.224	-0.01015	0.191	0.958
South*1972	0.00909	0.460	0.984	-0.29288	0.249	0.240	0.069	0.347	0.842	-0.30067	0.183	0.101
South*1973	0.00829	0.487	0.986	0.33883	0.239	0.156	0.29476	0.353	0.404	0.12491	0.187	0.504
South*1974	-0.0845	0.560	0.880	-0.17002	0.254	0.504	0.14664	0.414	0.723	-0.08381	0.172	0.627
South*1975	0.2377	0.496	0.632	0.51172*	0.233	0.029	0.26003	0.374	0.488	0.12138	0.187	0.516
South*1976	-0.44516	0.484	0.359	-0.07007	0.236	0.766	-0.0931	0.356	0.794	-0.17011	0.190	0.372
South*1977	-0.14307	0.488	0.770	0.04204	0.246	0.864	-0.01357	0.369	0.971	-0.1219	0.187	0.516
South*1978	-0.02482	0.530	0.963	0.10657	0.264	0.687	0.26298	0.352	0.456	-0.04527	0.184	0.806
South*1979	0.16627	0.479	0.729	0.29428	0.267	0.271	0.35115	0.347	0.312	0.18169	0.198	0.359
South*1980	-0.0279	0.459	0.952	0.48419	0.252	0.056	0.10477	0.347	0.763	0.29779	0.204	0.145
West*1963	-1.03696	0.650	0.111	-0.35194	0.299	0.240	-1.44247*	0.600	0.017	-0.23998	0.237	0.312
West*1964	-0.89079	0.513	0.083	-0.28621	0.252	0.256	-0.29949	0.414	0.470	-0.17207	0.275	0.532
West*1965	-0.84823	0.529	0.110	-0.51879*	0.239	0.031	-0.56641	0.418	0.176	-0.23422	0.196	0.232

West*1966	-1.01582*	0.440	0.021	-0.20448	0.252	0.418	-0.6944	0.408	0.089	-0.19476	0.215	0.365
West*1967	-0.69872	0.697	0.317	-0.41941	0.269	0.120	-0.02603	0.584	0.964	-0.27378	0.238	0.252
West*1968	-0.80088	0.549	0.145	-0.32687	0.244	0.182	-0.47624	0.416	0.253	-0.22592	0.225	0.317
West*1969	-1.76844***	0.507	0.001	-0.32436	0.253	0.200	-1.19378*	0.483	0.014	-0.2365	0.217	0.275
West*1970	-0.31232	0.562	0.579	-0.33688	0.339	0.321	-0.43176	0.492	0.381	-0.26054	0.240	0.278
West*1971	-0.61946	0.574	0.281	-0.45583	0.272	0.094	-0.09374	0.426	0.826	-0.23481	0.234	0.317
West*1972	-0.82495	0.532	0.122	-0.32235	0.279	0.249	-0.96134*	0.460	0.037	-0.15996	0.240	0.506
West*1973	-0.68106	0.495	0.169	-0.32755	0.259	0.207	-0.21239	0.414	0.608	-0.40964	0.241	0.089
West*1974	-0.83008	0.580	0.153	-0.60585*	0.290	0.037	-0.5881	0.457	0.199	-0.56366*	0.241	0.020
West*1975	-0.91991	0.522	0.079	-0.34952	0.275	0.205	-0.50374	0.434	0.247	-0.25512	0.246	0.301
West*1976	-1.13543*	0.549	0.039	-0.54466*	0.269	0.044	-0.63085	0.411	0.126	-0.3781	0.242	0.119
West*1977	-0.94574	0.486	0.052	-0.61181*	0.295	0.039	-0.80138	0.451	0.077	-0.60593*	0.244	0.014
West*1978	-0.94042	0.579	0.105	-0.56059	0.322	0.083	-0.44011	0.427	0.303	-0.28827	0.247	0.244
West*1979	-1.64482**	0.606	0.007	-0.70965*	0.315	0.025	-1.03506*	0.430	0.016	-0.46698	0.268	0.082
West*1980	-1.40201**	0.517	0.007	-0.67995*	0.338	0.045	-0.87191*	0.426	0.041	-0.47355	0.264	0.074
Age6							-1.14358***	0.096	0.000	-0.75587***	0.041	0.000
Age7							-1.14251***	0.093	0.000	-0.88307***	0.042	0.000
Age8							-1.30028***	0.100	0.000	-0.97221***	0.044	0.000
Age9							-1.37793***	0.098	0.000	-1.15837***	0.050	0.000
Age10							-1.32131***	0.107	0.000	-1.27403***	0.053	0.000
Age11							-1.45360***	0.112	0.000	-1.23517***	0.051	0.000
Age12							-1.20598***	0.107	0.000	-1.29841***	0.051	0.000
Age13							-1.35705***	0.106	0.000	-1.17964***	0.053	0.000
Age14							-1.17114***	0.099	0.000	-1.13005***	0.048	0.000
Age15							-0.76417***	0.095	0.000	-0.97966***	0.050	0.000
Age16							-0.45021***	0.074	0.000	-0.79397***	0.048	0.000
Age17							-0.24580**	0.074	0.001	-0.49754***	0.045	0.000
Intercept	-2.98969***	0.896	0.001	-1.89276***	0.449	0.000	-2.11495**	0.657	0.001	-1.10270***	0.308	0.000
Sample Size	24520			101793			69402			297626		
F Value	7.4			12.889			11.722			22.715		
P Value	0.000			0.000			0.000			0.000		

Source: NHIS 1963-1980. SE account for sample design strata and clusters. *p<0.05; **p<0.01; ***p<0.001

Table C5. Logistic Regressions of Annual Hospital Visits for Mothers, by Income

	Low Income Mothers			Moderate Income Mothers		
	Coeff.	SE	P	Coeff.	SE	P
NoMedicaid	(Ref)			(Ref)		
Medicaid	-0.03812	0.105	0.717	-0.00352	0.006	0.569
White	0.05649	0.039	0.149	0.00652	0.004	0.128
NoHighSchool	(Ref)			(Ref)		
SomeHighschool	-0.06103	0.039	0.118	-0.00202	0.004	0.620
Age18	(Ref)			(Ref)		
Age19	0.00289	0.117	0.980	-0.06334*	0.028	0.024
Age20	-0.12582	0.122	0.302	-0.10872***	0.025	0.000
Age21	-0.37256***	0.109	0.001	-0.16409***	0.023	0.000
Age22	-0.51218***	0.109	0.000	-0.20393***	0.023	0.000
Age23	-0.72736***	0.122	0.000	-0.22212***	0.023	0.000
Age24	-0.75708***	0.118	0.000	-0.27009***	0.022	0.000
Age25	-0.79773***	0.118	0.000	-0.29520***	0.023	0.000
Age26	-0.89956***	0.109	0.000	-0.31337***	0.022	0.000
Age27	-0.88756***	0.112	0.000	-0.32026***	0.022	0.000
Age28	-1.10479***	0.114	0.000	-0.35178***	0.022	0.000
Age29	-1.12561***	0.118	0.000	-0.38313***	0.022	0.000
Age30	-1.09428***	0.114	0.000	-0.38916***	0.022	0.000
Age31	-1.31219***	0.129	0.000	-0.40607***	0.022	0.000
Age32	-1.21496***	0.133	0.000	-0.42804***	0.022	0.000
Age33	-1.37334***	0.123	0.000	-0.43354***	0.022	0.000
Age34	-1.51459***	0.140	0.000	-0.44012***	0.022	0.000
Age35	-1.36073***	0.131	0.000	-0.44720***	0.022	0.000
Age36	-1.44744***	0.143	0.000	-0.44613***	0.022	0.000
Age37	-1.35894***	0.140	0.000	-0.45479***	0.022	0.000
Age38	-1.47478***	0.140	0.000	-0.46910***	0.022	0.000
Age39	-1.64205***	0.151	0.000	-0.47612***	0.022	0.000
Age40	-1.50015***	0.146	0.000	-0.47221***	0.022	0.000
Age41	-1.60180***	0.150	0.000	-0.48262***	0.022	0.000
Age42	-1.77448***	0.154	0.000	-0.47458***	0.022	0.000
Age43	-1.91363***	0.172	0.000	-0.48859***	0.023	0.000
Age44	-1.60758***	0.164	0.000	-0.48341***	0.023	0.000
Age45	-1.84546***	0.174	0.000	-0.49302***	0.022	0.000
PerCapHosp	-0.78263	1.036	0.451	-0.0075	0.086	0.931
PerCapMD	0.8155	3.583	0.820	-0.30105	0.236	0.203
PerCapPA	-0.00034	0.001	0.619	-0.00002	0.000	0.743
AFDCBen	0.00022	0.000	0.320	-0.00005***	0.000	0.000
AFDCCase	0.00477	0.004	0.188	-0.00008	0.000	0.790
AL	(Ref)			(Ref)		
AR	0.3091	0.186	0.098	-0.0164	0.020	0.406
CA	0.57392	0.593	0.334	0.06731*	0.033	0.041
CO	0.92207	0.540	0.088	0.05477	0.032	0.090
CT	-0.40798	0.655	0.534	0.08643*	0.042	0.039

DE	-0.20145	0.479	0.674	0.00846	0.027	0.751
DC	-0.68599	1.211	0.571	0.08881	0.076	0.245
FL	-0.02504	0.260	0.923	0.00461	0.019	0.808
GA	-0.08943	0.185	0.629	0.01094	0.015	0.452
ID	0.63365	0.624	0.311	0.06535	0.037	0.078
IL	-0.41672	0.427	0.330	0.08967**	0.028	0.001
ID	-0.35503	0.367	0.334	0.04816	0.025	0.056
IO	-0.36035	0.377	0.340	0.07028**	0.026	0.006
KS	-0.20022	0.494	0.685	0.09202*	0.040	0.023
KY	0.06413	0.193	0.740	0.03135	0.016	0.056
LA	-0.02757	0.191	0.885	0.02851	0.016	0.082
ME	-0.01579	0.475	0.974	0.0342	0.029	0.241
MD	-0.49405	0.479	0.303	0.03575	0.033	0.278
MA	-0.33442	0.676	0.621	0.10332*	0.040	0.010
MI	-0.45572	0.416	0.274	0.08982***	0.026	0.001
MN	-0.23884	0.501	0.634	0.09716**	0.031	0.002
MS	-0.20909	0.203	0.303	0.00669	0.017	0.699
MO	-0.46052	0.357	0.198	0.04931*	0.024	0.040
MT	1.30398	0.883	0.140	0.05249	0.055	0.343
NE	-0.08057	0.495	0.871	0.07628	0.040	0.057
NV	1.32572**	0.470	0.005	0.03287	0.032	0.303
NH	0.33862	0.579	0.559	0.04964	0.034	0.150
NJ	-0.38774	0.572	0.499	0.07249*	0.034	0.035
NM	1.28301**	0.466	0.006	0.03344	0.027	0.221
NY	-0.45351	0.718	0.528	0.11592**	0.042	0.006
NC	-0.03514	0.219	0.873	0.01159	0.018	0.515
ND	-0.08735	0.738	0.906	0.06762	0.075	0.366
OH	-0.41907	0.406	0.303	0.07544**	0.028	0.008
OK	0.57462*	0.233	0.014	-0.00191	0.020	0.923
OR	0.51431	0.532	0.334	0.05561*	0.028	0.049
PE	-0.31015	0.550	0.573	0.07934**	0.030	0.009
RI	0.31576	0.559	0.572	0.03299	0.032	0.304
SC	0.13805	0.225	0.540	0.00798	0.019	0.675
SD	0.02248	0.654	0.973	0.09561	0.054	0.080
TN	-0.07744	0.201	0.700	0.01015	0.017	0.549
TX	0.3293	0.207	0.112	0.01538	0.016	0.351
UT	0.63164	0.523	0.228	0.10496**	0.033	0.001
VT	-0.85932	0.819	0.295	0.04211	0.031	0.179
VA	-0.07722	0.316	0.807	0.04543*	0.022	0.040
WA	0.75556	0.506	0.136	0.04259	0.029	0.139
WV	0.34223	0.248	0.168	0.04389*	0.020	0.030
WI	-0.30484	0.394	0.440	0.09193***	0.025	0.000
WY	1.65800*	0.696	0.018	0.04783	0.052	0.355
1963	(Ref)			(Ref)		
1964	0.43062*	0.181	0.018	-0.00656	0.018	0.710
1965	-0.04212	0.247	0.865	-0.00446	0.015	0.765
1966	0.22414	0.235	0.340	-0.00237	0.014	0.869
1967	0.61181	0.397	0.124	-0.0204	0.017	0.236
1968	-0.02114	0.217	0.922	-0.02451	0.018	0.171

1969	0.25145	0.282	0.374	-0.00124	0.018	0.944
1970	-0.22415	0.287	0.435	0.00988	0.022	0.660
1971	0.14468	0.274	0.598	0.00811	0.019	0.669
1972	0.09174	0.305	0.764	-0.02599	0.022	0.231
1973	-0.22247	0.325	0.494	-0.03059	0.021	0.147
1974	0.16809	0.292	0.565	-0.03074	0.022	0.163
1975	0.01938	0.335	0.954	-0.00798	0.027	0.765
1976	-0.01707	0.299	0.954	-0.00413	0.024	0.861
1977	-0.10019	0.306	0.743	-0.02567	0.022	0.242
1978	-0.22977	0.307	0.455	-0.04114	0.024	0.088
1979	-0.11887	0.320	0.711	-0.0363	0.025	0.144
1980	-0.0531	0.333	0.874	-0.02316	0.027	0.392
1967fy	0.05521	0.412	0.894	-0.00784	0.015	0.613
Midwest*1963	0.54331	0.480	0.259	0.00764	0.021	0.712
Midwest*1964	0.2327	0.523	0.657	-0.00116	0.023	0.960
Midwest*1965	0.61142	0.484	0.207	-0.00266	0.023	0.908
Midwest*1966	0.14805	0.497	0.766	-0.02147	0.024	0.363
Midwest*1967	-0.41467	0.659	0.529	-0.00795	0.024	0.744
Midwest*1968	0.28256	0.497	0.570	-0.02173	0.024	0.368
Midwest*1969	0.1312	0.510	0.797	-0.02337	0.025	0.345
Midwest*1970	0.79559	0.513	0.122	-0.03193	0.027	0.241
Midwest*1971	0.58013	0.486	0.233	-0.01735	0.026	0.503
Midwest*1972	0.70437	0.501	0.160	0.00033	0.025	0.989
Midwest*1973	0.7481	0.522	0.153	-0.00272	0.027	0.919
Midwest*1974	0.23291	0.485	0.631	0.00145	0.026	0.955
Midwest*1975	0.2831	0.510	0.579	-0.01586	0.030	0.596
Midwest*1976	0.34404	0.494	0.487	-0.03411	0.026	0.193
Midwest*1977	0.56114	0.482	0.245	0.01094	0.026	0.677
Midwest*1978	0.43651	0.476	0.360	0.01575	0.027	0.565
Midwest*1979	0.59197	0.485	0.223	0.00469	0.027	0.861
Midwest*1980	0.48012	0.483	0.320	-0.00745	0.029	0.798
South*1963	-0.12112	0.453	0.789	0.01433	0.021	0.498
South*1964	-0.42109	0.484	0.385	0.0171	0.021	0.416
South*1965	0.05702	0.437	0.896	0.01581	0.020	0.430
South*1966	-0.13464	0.478	0.778	0.02088	0.021	0.324
South*1967	-0.82303	0.601	0.171	0.02679	0.022	0.227
South*1968	-0.24381	0.449	0.587	0.01137	0.022	0.604
South*1969	-0.12176	0.475	0.798	-0.01091	0.021	0.608
South*1970	0.23585	0.490	0.631	-0.00758	0.025	0.762
South*1971	-0.1539	0.456	0.736	0.0133	0.022	0.550
South*1972	-0.04456	0.476	0.925	0.04322	0.024	0.072
South*1973	0.28556	0.496	0.565	0.03686	0.024	0.127
South*1974	-0.33943	0.470	0.471	0.0311	0.024	0.201
South*1975	-0.08914	0.482	0.853	0.01051	0.027	0.698
South*1976	-0.41895	0.465	0.368	-0.00148	0.024	0.951
South*1977	-0.09163	0.462	0.843	0.01122	0.023	0.626
South*1978	0.20205	0.462	0.662	0.03669	0.025	0.145
South*1979	-0.07087	0.465	0.879	0.04452	0.025	0.078
South*1980	0.12556	0.462	0.786	0.0421	0.026	0.104

West*1963	-0.71834	0.569	0.207	-0.01664	0.024	0.491
West*1964	-1.00643	0.629	0.110	0.01101	0.026	0.670
West*1965	-0.31564	0.554	0.569	0.00613	0.027	0.820
West*1966	-0.91666	0.656	0.163	-0.01537	0.025	0.543
West*1967	-1.42308*	0.691	0.040	0.00002	0.027	0.999
West*1968	-0.74187	0.605	0.221	-0.01832	0.026	0.488
West*1969	-0.57898	0.620	0.351	0.00465	0.028	0.868
West*1970	-0.22743	0.606	0.708	0.00101	0.032	0.975
West*1971	-0.76982	0.599	0.199	-0.01215	0.031	0.691
West*1972	-0.79823	0.625	0.202	0.01958	0.028	0.482
West*1973	-0.76334	0.626	0.223	0.00089	0.030	0.976
West*1974	-1.03149	0.598	0.085	-0.01317	0.030	0.658
West*1975	-1.04997	0.628	0.095	0.01964	0.033	0.547
West*1976	-1.02979	0.605	0.089	0.01019	0.030	0.738
West*1977	-0.57033	0.615	0.354	0.02639	0.031	0.390
West*1978	-0.66407	0.594	0.265	0.02502	0.029	0.396
West*1979	-0.77204	0.606	0.203	0.00793	0.031	0.799
West*1980	-1.07694	0.606	0.076	-0.01169	0.034	0.733
Intercept	-0.10899	0.648	0.867	0.65688***	0.048	0.000
Sample Size	20902			106045		
F	8.816			52.538		
p	0.000			0.000		

Source: NHIS 1963-1980. SE account for sample design strata and clusters. *p<0.05; **p<0.01; ***p<0.001

Table C6. Logistic Regression Results, Predicted AFDC Participation Modifiers, Children 0-5

	AFDC 1			AFDC 2		
	Coeff.	SE	P	Coeff.	SE	P
NoMedicaid	(Ref)			(Ref)		
Medicaid	-0.00922	0.052	0.860	-0.02124	0.053	0.690
AFDC Moderator	0.25332	0.328	0.440	1.10501	0.663	0.096
Medicaid*AFDC Moderator	0.76108*	0.329	0.021	1.75010*	0.709	0.014
Female	(Ref)			(Ref)		
Male	0.27180***	0.017	0.000	0.27213***	0.017	0.000
NonWhite	(Ref)			(Ref)		
White	0.25848***	0.037	0.000	0.19348***	0.031	0.000
NoHighSchool	(Ref)			(Ref)		
SomeHighschool	0.04732	0.025	0.063	0.04373	0.025	0.086
HeadLessThan25	(Ref)			(Ref)		
Head2544	-0.12125***	0.023	0.000	-0.11360***	0.022	0.000
Head4564	-0.15664***	0.039	0.000	-0.16236***	0.039	0.000
Head65Over	-0.27323**	0.091	0.003	-0.28968**	0.091	0.001
Age0	(Ref)			(Ref)		
Age1	-0.46483***	0.026	0.000	-0.46697***	0.026	0.000
Age2	-0.79410***	0.029	0.000	-0.79685***	0.029	0.000
Age3	-0.92534***	0.029	0.000	-0.93047***	0.030	0.000
Age4	-0.95045***	0.029	0.000	-0.95561***	0.029	0.000
Age5	-0.81154***	0.029	0.000	-0.81718***	0.029	0.000
PerCapHosp	0.05319	0.599	0.929	0.10473	0.598	0.861
PerCapMD	-6.93927**	2.182	0.002	-7.02609**	2.183	0.001
PerCapPA	-0.00019	0.000	0.595	-0.00036	0.000	0.325
AFDCBen	-0.0002	0.000	0.103	-0.00022	0.000	0.068
AFDCCase	0.00169	0.002	0.405	0.00184	0.002	0.364
AL	(Ref)			(Ref)		
AR	-0.13564	0.113	0.231	-0.15461	0.112	0.169
CA	1.20285***	0.268	0.000	1.24777***	0.268	0.000
CO	1.08345***	0.243	0.000	1.09389***	0.245	0.000
CT	0.71972	0.371	0.053	0.76302*	0.372	0.041
DE	0.53936**	0.196	0.006	0.53716**	0.198	0.007
DC	2.22520**	0.793	0.005	2.24347**	0.792	0.005
FL	0.44971**	0.146	0.002	0.46336**	0.146	0.002
GA	0.26611**	0.097	0.006	0.27856**	0.096	0.004
ID	0.52551	0.290	0.070	0.55772	0.290	0.055
IL	0.65992**	0.207	0.002	0.70078***	0.208	0.001
ID	0.40489*	0.184	0.029	0.43121*	0.185	0.020
IO	0.64659**	0.196	0.001	0.67764***	0.196	0.001
KS	0.43334	0.286	0.131	0.42083	0.287	0.143
KY	0.11805	0.105	0.263	0.13314	0.105	0.206
LA	0.31889**	0.114	0.005	0.32591**	0.113	0.004
ME	0.61019**	0.220	0.006	0.63256**	0.220	0.004

MD	0.66638*	0.276	0.016	0.68366*	0.276	0.014
MA	1.27358***	0.318	0.000	1.33092***	0.319	0.000
MI	0.78926***	0.197	0.000	0.81462***	0.198	0.000
MN	0.91522***	0.232	0.000	0.94721***	0.233	0.000
MS	-0.03247	0.125	0.795	-0.05593	0.125	0.654
MO	0.42573*	0.178	0.017	0.45998*	0.179	0.010
MT	1.06753**	0.398	0.008	1.05624**	0.398	0.008
NE	0.43498	0.297	0.143	0.44671	0.297	0.133
NV	0.58831*	0.264	0.026	0.60784*	0.258	0.019
NH	0.85897***	0.245	0.001	0.91338***	0.246	0.000
NJ	0.64060*	0.261	0.014	0.69247**	0.261	0.008
NM	0.72237***	0.213	0.001	0.73531***	0.213	0.001
NY	1.19270**	0.364	0.001	1.25378***	0.365	0.001
NC	0.1957	0.129	0.129	0.19854	0.129	0.123
ND	0.82516*	0.404	0.042	0.79970*	0.405	0.049
OH	0.58852**	0.200	0.003	0.62502**	0.201	0.002
OK	0.12691	0.137	0.355	0.1347	0.137	0.326
OR	0.92003***	0.234	0.000	0.93473***	0.233	0.000
PE	0.68021**	0.244	0.006	0.72580**	0.244	0.003
RI	0.69601**	0.251	0.006	0.69262**	0.252	0.006
SC	0.06576	0.117	0.573	0.04743	0.117	0.685
SD	0.75727	0.393	0.054	0.75955	0.392	0.053
TN	0.12319	0.130	0.344	0.11965	0.129	0.356
TX	0.14816	0.118	0.209	0.15069	0.118	0.201
UT	0.88502***	0.209	0.000	0.91578***	0.210	0.000
VT	1.10435***	0.278	0.000	1.12039***	0.277	0.000
VA	0.40633*	0.163	0.013	0.40496*	0.162	0.013
WA	0.92949***	0.226	0.000	0.95994***	0.226	0.000
WV	0.52842***	0.128	0.000	0.52824***	0.127	0.000
WI	0.75812***	0.193	0.000	0.79689***	0.194	0.000
WY	0.70342	0.366	0.056	0.72636*	0.366	0.048
1963	(Ref)			(Ref)		
1964	0.16062	0.140	0.252	0.16022	0.140	0.253
1965	0.16035	0.131	0.222	0.16123	0.131	0.220
1966	0.15103	0.157	0.337	0.14928	0.157	0.343
1967	0.22799*	0.100	0.023	0.23375*	0.100	0.020
1968	0.07355	0.132	0.578	0.08566	0.132	0.518
1969	0.31987*	0.153	0.037	0.33746*	0.154	0.029
1970	0.20539	0.163	0.209	0.22649	0.164	0.167
1971	0.20391	0.171	0.233	0.22758	0.170	0.182
1972	0.30885	0.171	0.072	0.32826	0.171	0.055
1973	0.20344	0.162	0.211	0.21746	0.162	0.181
1974	0.43396*	0.177	0.015	0.44855*	0.178	0.012
1975	0.29186	0.192	0.129	0.30481	0.191	0.112
1976	0.58300**	0.185	0.002	0.58421**	0.185	0.002
1977	0.51974**	0.188	0.006	0.50515**	0.189	0.008
1978	0.46203*	0.194	0.018	0.45367*	0.194	0.020
1979	0.37597	0.207	0.070	0.36755	0.208	0.078
1980	0.48692*	0.208	0.019	0.48647*	0.208	0.020

1967fy	0.11015	0.189	0.561	0.1211	0.190	0.524
Midwest*1963	0.05157	0.218	0.813	0.05712	0.219	0.794
Midwest*1964	-0.03007	0.186	0.871	-0.02564	0.186	0.891
Midwest*1965	0.01819	0.171	0.915	0.02516	0.171	0.883
Midwest*1966	-0.06867	0.150	0.648	-0.06321	0.150	0.674
Midwest*1967	0.01056	0.179	0.953	0.00841	0.179	0.962
Midwest*1968	-0.04661	0.176	0.791	-0.04585	0.176	0.795
Midwest*1969	-0.16175	0.193	0.402	-0.16858	0.193	0.384
Midwest*1970	-0.08468	0.194	0.662	-0.09237	0.194	0.634
Midwest*1971	-0.0147	0.193	0.939	-0.01634	0.192	0.932
Midwest*1972	0.00423	0.191	0.982	0.0089	0.191	0.963
Midwest*1973	-0.01813	0.190	0.924	-0.01042	0.190	0.956
Midwest*1974	-0.15038	0.193	0.437	-0.14224	0.194	0.464
Midwest*1975	0.05139	0.205	0.802	0.05185	0.205	0.800
Midwest*1976	-0.24184	0.195	0.215	-0.23226	0.195	0.235
Midwest*1977	-0.31439	0.193	0.105	-0.30434	0.194	0.118
Midwest*1978	-0.20664	0.206	0.315	-0.18463	0.205	0.369
Midwest*1979	0.07421	0.207	0.720	0.08919	0.208	0.669
Midwest*1980	-0.02521	0.199	0.899	-0.02016	0.199	0.920
South*1963	0.26832	0.215	0.214	0.27981	0.216	0.196
South*1964	0.10284	0.179	0.566	0.11501	0.180	0.523
South*1965	0.06338	0.169	0.708	0.07453	0.169	0.659
South*1966	0.15787	0.159	0.322	0.16981	0.159	0.286
South*1967	0.07178	0.171	0.674	0.08024	0.171	0.638
South*1968	0.13437	0.175	0.444	0.13497	0.176	0.442
South*1969	-0.04775	0.194	0.805	-0.04847	0.194	0.803
South*1970	0.02307	0.205	0.910	0.02448	0.205	0.905
South*1971	0.27458	0.193	0.156	0.28382	0.193	0.142
South*1972	0.06859	0.192	0.721	0.08899	0.191	0.642
South*1973	0.27318	0.183	0.135	0.29626	0.182	0.105
South*1974	0.03659	0.191	0.848	0.07092	0.192	0.712
South*1975	0.41428*	0.203	0.042	0.45791*	0.203	0.024
South*1976	0.01785	0.189	0.925	0.06448	0.189	0.733
South*1977	0.04649	0.193	0.810	0.1077	0.193	0.578
South*1978	0.13958	0.207	0.501	0.19653	0.207	0.342
South*1979	0.33381	0.211	0.114	0.38317	0.212	0.072
South*1980	0.36304	0.191	0.058	0.41791*	0.192	0.030
West*1963	-0.24788	0.272	0.363	-0.23104	0.272	0.397
West*1964	-0.34062	0.208	0.103	-0.32236	0.208	0.123
West*1965	-0.41045	0.214	0.056	-0.39321	0.214	0.066
West*1966	-0.23301	0.197	0.238	-0.21383	0.197	0.278
West*1967	-0.43416	0.230	0.059	-0.4281	0.228	0.061
West*1968	-0.18012	0.209	0.389	-0.18236	0.208	0.381
West*1969	-0.44824*	0.217	0.040	-0.45529*	0.217	0.037
West*1970	-0.19244	0.252	0.446	-0.20329	0.252	0.421
West*1971	-0.36621	0.214	0.088	-0.36757	0.214	0.086
West*1972	-0.2771	0.226	0.221	-0.27875	0.227	0.220
West*1973	-0.32351	0.210	0.124	-0.32983	0.209	0.116
West*1974	-0.45096*	0.224	0.045	-0.45749*	0.225	0.042

West*1975	-0.48860*	0.241	0.043	-0.48899*	0.240	0.043
West*1976	-0.50170*	0.223	0.025	-0.49733*	0.223	0.026
West*1977	-0.48035*	0.230	0.037	-0.47017*	0.230	0.042
West*1978	-0.44629	0.240	0.063	-0.4343	0.239	0.070
West*1979	-0.4701	0.250	0.061	-0.46071	0.250	0.066
Intercept	-1.99932***	0.348	0.000	-1.95825***	0.346	0.000
N_sub	232219			232219		
F	24.253			24.063		
p	0.000			0.000		

Source: NHIS 1963-1980. SE account for sample design strata and clusters.. *p<0.05; **p<0.01; ***p<0.001. AFDC Moderator 1 and 2 are described in the text.

Table C7. Logistic Regression Results, Predicted AFDC Participation Modifiers, Children 0-17

	AFDC 1			AFDC 2		
	Coeff.	SE	P	Coeff.	SE	P
NoMedicaid	(Ref)			(Ref)		
Medicaid	0.00704	0.037	0.848	-0.00324	0.037	0.931
AFDC Moderator	0.1124	0.283	0.692	0.76552	0.439	0.082
AFDC Moderator*Medicaid	0.98443***	0.283	0.001	1.84523***	0.470	0.000
Female	(Ref)			(Ref)		
Male	0.11057***	0.010	0.000	0.11115***	0.010	0.000
NonWhite	(Ref)			(Ref)		
White	0.31823***	0.022	0.000	0.24560***	0.020	0.000
NoHighSchool	(Ref)			(Ref)		
SomeHighschool	0.01067	0.016	0.515	0.00486	0.016	0.767
HeadLessThan25	(Ref)			(Ref)		
Head2544	-0.31987***	0.023	0.000	-0.32033***	0.022	0.000
Head4564	-0.46074***	0.028	0.000	-0.46167***	0.027	0.000
Head65Over	-0.49782***	0.053	0.000	-0.50977***	0.053	0.000
Age0	(Ref)			(Ref)		
Age1	-0.45761***	0.026	0.000	-0.45931***	0.026	0.000
Age2	-0.77598***	0.029	0.000	-0.77762***	0.029	0.000
Age3	-0.89721***	0.030	0.000	-0.90083***	0.030	0.000
Age4	-0.91220***	0.030	0.000	-0.91544***	0.030	0.000
Age5	-0.76491***	0.029	0.000	-0.76857***	0.029	0.000
Age6	-0.79790***	0.030	0.000	-0.80223***	0.030	0.000
Age7	-0.88453***	0.030	0.000	-0.88914***	0.029	0.000
Age8	-1.03448***	0.029	0.000	-1.03950***	0.029	0.000
Age9	-1.18720***	0.034	0.000	-1.19233***	0.034	0.000
Age10	-1.25316***	0.033	0.000	-1.25876***	0.033	0.000
Age11	-1.26877***	0.032	0.000	-1.27503***	0.032	0.000
Age12	-1.30610***	0.032	0.000	-1.31215***	0.032	0.000
Age13	-1.26333***	0.031	0.000	-1.27028***	0.032	0.000
Age14	-1.18574***	0.030	0.000	-1.19248***	0.030	0.000
Age15	-0.98895***	0.032	0.000	-0.99669***	0.032	0.000
Ag16	-0.75067***	0.030	0.000	-0.75827***	0.030	0.000
Age17	-0.49697***	0.028	0.000	-0.50700***	0.028	0.000
PerCapHosp	0.14143	0.427	0.740	0.198	0.427	0.643
PerCapMD	-3.68229*	1.498	0.014	-3.79110*	1.503	0.012
PerCapPA	-0.00022	0.000	0.379	-0.00037	0.000	0.145
AFDCBen	-0.00025**	0.000	0.003	-0.00027**	0.000	0.001
AFDCCase	-0.00006	0.001	0.965	0.00013	0.001	0.925
AL	(Ref)			(Ref)		
AR	-0.0792	0.085	0.352	-0.09149	0.085	0.285
CA	0.68911**	0.208	0.001	0.73272***	0.209	0.001
CO	0.65119***	0.185	0.000	0.67133***	0.186	0.000
CT	0.39444	0.225	0.081	0.43601	0.225	0.054
DE	0.23343	0.145	0.107	0.23724	0.145	0.102
DC	1.24098*	0.562	0.028	1.26481*	0.557	0.024
FL	0.16531	0.098	0.094	0.18355	0.098	0.063
GA	0.14305*	0.068	0.035	0.15627*	0.067	0.021
ID	0.18614	0.219	0.396	0.21855	0.218	0.316
IL	0.42384**	0.146	0.004	0.46346**	0.146	0.002
ID	0.21023	0.122	0.084	0.23853	0.122	0.051

IO	0.46513**	0.146	0.002	0.49485***	0.146	0.001
KS	0.20419	0.180	0.258	0.19104	0.181	0.291
KY	-0.01958	0.085	0.818	-0.00404	0.085	0.962
LA	0.15954	0.082	0.052	0.16508*	0.082	0.044
ME	0.25808	0.153	0.092	0.27837	0.153	0.070
MD	0.32405	0.178	0.069	0.34329	0.178	0.055
MA	0.64848**	0.219	0.003	0.70274**	0.220	0.002
MI	0.49262***	0.136	0.000	0.51613***	0.136	0.000
MN	0.46519**	0.156	0.003	0.49594**	0.157	0.002
MS	-0.06294	0.076	0.410	-0.08547	0.077	0.267
MO	0.25224*	0.117	0.032	0.28622*	0.118	0.016
MT	0.66351*	0.290	0.023	0.65496*	0.291	0.025
NE	0.22568	0.201	0.262	0.23987	0.202	0.235
NV	0.13793	0.188	0.463	0.16173	0.185	0.382
NH	0.32703*	0.162	0.044	0.38199*	0.163	0.019
NJ	0.3177	0.179	0.077	0.36718*	0.180	0.042
NM	0.33988*	0.160	0.034	0.35084*	0.160	0.028
NY	0.60507*	0.252	0.017	0.66351**	0.253	0.009
NC	0.06598	0.088	0.451	0.07078	0.088	0.421
ND	0.46744	0.294	0.113	0.44707	0.295	0.130
OH	0.38241**	0.143	0.008	0.41881**	0.144	0.004
OK	0.03757	0.095	0.693	0.04427	0.095	0.641
OR	0.49037**	0.177	0.006	0.50606**	0.177	0.004
PE	0.39411*	0.170	0.021	0.43687*	0.170	0.010
RI	0.33979	0.189	0.073	0.34142	0.189	0.072
SC	-0.11872	0.074	0.108	-0.12968	0.074	0.081
SD	0.48151	0.278	0.084	0.48026	0.278	0.085
TN	0.10499	0.076	0.171	0.10485	0.077	0.173
TX	0.0233	0.079	0.768	0.02762	0.079	0.728
UT	0.52066**	0.164	0.002	0.55235***	0.165	0.001
VT	0.3566	0.268	0.184	0.38044	0.262	0.148
VA	0.25184*	0.110	0.023	0.25417*	0.110	0.021
WA	0.53454**	0.175	0.002	0.56768**	0.175	0.001
WV	0.30645***	0.091	0.001	0.30775***	0.090	0.001
WI	0.51842***	0.130	0.000	0.55533***	0.131	0.000
WY	0.49022	0.257	0.057	0.50894*	0.258	0.050
1963	(Ref)			(Ref)		
1964	0.03145	0.062	0.615	0.03166	0.062	0.612
1965	0.03432	0.063	0.585	0.03557	0.063	0.573
1966	0.11644	0.083	0.162	0.1162	0.083	0.161
1967	0.10814	0.068	0.112	0.11371	0.068	0.095
1968	0.00313	0.070	0.964	0.01328	0.070	0.849
1969	0.15418	0.087	0.079	0.16782	0.087	0.056
1970	0.19892	0.102	0.052	0.21570*	0.103	0.036
1971	0.10418	0.102	0.306	0.12494	0.102	0.220
1972	0.16515	0.101	0.103	0.18276	0.101	0.072
1973	0.10159	0.105	0.335	0.1134	0.105	0.281
1974	0.22336*	0.102	0.030	0.23509*	0.103	0.023
1975	0.14982	0.115	0.192	0.1587	0.114	0.166
1976	0.27334*	0.122	0.025	0.26884*	0.122	0.028
1977	0.21707	0.115	0.060	0.20377	0.116	0.079
1978	0.13303	0.133	0.319	0.1189	0.133	0.373
1979	0.10927	0.137	0.427	0.10058	0.137	0.465
1980	0.11828	0.138	0.393	0.11504	0.138	0.407
1967fy	-0.04525	0.111	0.685	-0.03707	0.112	0.740
Midwest*1963	-0.07153	0.138	0.605	-0.06829	0.138	0.620

Midwest*1964	-0.01818	0.133	0.892	-0.01572	0.133	0.906
Midwest*1965	-0.09056	0.123	0.461	-0.08684	0.122	0.477
Midwest*1966	-0.24173	0.142	0.089	-0.23913	0.141	0.091
Midwest*1967	-0.1192	0.144	0.408	-0.1231	0.144	0.392
Midwest*1968	-0.13278	0.136	0.328	-0.13225	0.135	0.329
Midwest*1969	-0.19202	0.139	0.167	-0.19671	0.138	0.156
Midwest*1970	-0.24347	0.148	0.100	-0.24935	0.148	0.093
Midwest*1971	-0.1111	0.140	0.429	-0.11353	0.140	0.418
Midwest*1972	-0.12348	0.141	0.381	-0.12182	0.141	0.387
Midwest*1973	-0.00302	0.142	0.983	0.00343	0.142	0.981
Midwest*1974	-0.15761	0.137	0.252	-0.15076	0.137	0.271
Midwest*1975	0.00628	0.140	0.964	0.00741	0.140	0.958
Midwest*1976	-0.2233	0.148	0.131	-0.21443	0.148	0.148
Midwest*1977	-0.32933*	0.143	0.021	-0.32239*	0.143	0.025
Midwest*1978	-0.04934	0.152	0.745	-0.02545	0.151	0.867
Midwest*1979	-0.02125	0.150	0.887	-0.00852	0.150	0.955
Midwest*1980	0.02569	0.141	0.856	0.03098	0.141	0.827
South*1963	0.08096	0.134	0.545	0.08817	0.134	0.511
South*1964	0.04003	0.124	0.748	0.04753	0.125	0.704
South*1965	0.05342	0.124	0.667	0.06044	0.124	0.626
South*1966	-0.01604	0.141	0.910	-0.00811	0.141	0.954
South*1967	-0.02894	0.133	0.828	-0.02443	0.133	0.854
South*1968	0.07069	0.128	0.581	0.06862	0.128	0.592
South*1969	-0.06006	0.135	0.657	-0.06108	0.135	0.652
South*1970	-0.10879	0.146	0.456	-0.10731	0.146	0.463
South*1971	0.07441	0.140	0.595	0.08329	0.140	0.551
South*1972	-0.02973	0.136	0.827	-0.0117	0.136	0.931
South*1973	0.12896	0.138	0.349	0.1537	0.137	0.262
South*1974	0.03179	0.133	0.811	0.06635	0.133	0.617
South*1975	0.15613	0.139	0.263	0.20018	0.138	0.149
South*1976	-0.03006	0.141	0.831	0.01733	0.141	0.902
South*1977	-0.01367	0.141	0.923	0.04137	0.141	0.769
South*1978	0.14421	0.151	0.340	0.20294	0.151	0.179
South*1979	0.18172	0.146	0.215	0.22683	0.146	0.121
South*1980	0.1815	0.142	0.202	0.23508	0.142	0.099
West*1963	-0.2003	0.181	0.269	-0.18698	0.181	0.302
West*1964	-0.19428	0.167	0.244	-0.18006	0.167	0.281
West*1965	-0.27072	0.165	0.103	-0.25752	0.165	0.119
West*1966	-0.20754	0.159	0.192	-0.19225	0.158	0.225
West*1967	-0.26926	0.179	0.134	-0.26581	0.179	0.138
West*1968	-0.17234	0.169	0.308	-0.17545	0.168	0.298
West*1969	-0.35576*	0.174	0.042	-0.36096*	0.174	0.038
West*1970	-0.26724	0.179	0.137	-0.27677	0.180	0.124
West*1971	-0.26764	0.172	0.120	-0.27066	0.171	0.115
West*1972	-0.25769	0.185	0.165	-0.26188	0.186	0.160
West*1973	-0.25962	0.178	0.145	-0.2626	0.177	0.138
West*1974	-0.37849*	0.172	0.029	-0.38357*	0.172	0.026
West*1975	-0.36607*	0.172	0.034	-0.36504*	0.171	0.034
West*1976	-0.38593*	0.169	0.023	-0.38399*	0.169	0.024
West*1977	-0.43463*	0.179	0.016	-0.43231*	0.180	0.016
West*1978	-0.31187	0.191	0.103	-0.30089	0.190	0.114
West*1979	-0.46637*	0.191	0.015	-0.45537*	0.191	0.017
West*1980	-0.54221**	0.185	0.004	-0.51993**	0.185	0.005
afdc_mod2				0.76552	0.439	0.082
Intercept	-1.67791***	0.239	0.000	-1.62086***	0.238	0.000
Sample Size	751385			751385		

F Value	63.923	62.667
P Value	0	0

Source: NHIS 1963-1980. SE account for sample design strata and clusters.. *p<0.05; **p<0.01; ***p<0.001. AFDC Moderator 1 and 2 are described in the text.

Table C8. Logistic Regression Results, Predicted AFDC Participation Modifiers, Mothers

	AFDC 1			AFDC 2		
	Coeff.	SE	P	Coeff.	SE	P
NoMedicaid	(Ref)			(Ref)		
Medicaid	0.00843	0.032	0.794	-0.00444	0.032	0.888
AFDC Modrator	0.62354***	0.172	0.000	-1.68996***	0.502	0.001
AFDC Moderator*Medicaid	-0.00002	0.182	1.000	0.68496	0.541	0.206
White	0.09007***	0.019	0.000	-0.00647	0.016	0.693
NoHighSchool	(Ref)			(Ref)		
SomeHighschool	-0.05007**	0.016	0.002	-0.05350***	0.016	0.001
Age18	(Ref)			(Ref)		
Age19	-0.07989	0.067	0.232	-0.08008	0.066	0.228
Age20	-0.27037***	0.062	0.000	-0.27260***	0.061	0.000
Age21	-0.47324***	0.059	0.000	-0.47818***	0.059	0.000
Age22	-0.64610***	0.057	0.000	-0.65146***	0.056	0.000
Age23	-0.76021***	0.057	0.000	-0.77090***	0.057	0.000
Age24	-0.90902***	0.053	0.000	-0.92013***	0.053	0.000
Age25	-0.99451***	0.056	0.000	-1.00668***	0.055	0.000
Age26	-1.07357***	0.054	0.000	-1.08666***	0.054	0.000
Age27	-1.14785***	0.052	0.000	-1.16147***	0.052	0.000
Age28	-1.29864***	0.052	0.000	-1.31276***	0.052	0.000
Age29	-1.43492***	0.054	0.000	-1.44938***	0.054	0.000
Age30	-1.48405***	0.053	0.000	-1.49872***	0.053	0.000
Age31	-1.58547***	0.056	0.000	-1.60084***	0.056	0.000
Age32	-1.72771***	0.056	0.000	-1.74152***	0.056	0.000
Age33	-1.75592***	0.055	0.000	-1.76973***	0.054	0.000
Age34	-1.83913***	0.056	0.000	-1.85366***	0.056	0.000
Age35	-1.90978***	0.055	0.000	-1.92489***	0.055	0.000
Age36	-1.90450***	0.056	0.000	-1.92024***	0.055	0.000
Age37	-2.01626***	0.055	0.000	-2.03103***	0.055	0.000
Age38	-2.07556***	0.057	0.000	-2.09122***	0.057	0.000
Age39	-2.10523***	0.059	0.000	-2.12194***	0.058	0.000
Age40	-2.14755***	0.059	0.000	-2.16457***	0.058	0.000
Age41	-2.17327***	0.057	0.000	-2.19011***	0.057	0.000
Age42	-2.20871***	0.057	0.000	-2.22627***	0.057	0.000
Age43	-2.22747***	0.060	0.000	-2.24455***	0.059	0.000
Age44	-2.21761***	0.059	0.000	-2.23545***	0.059	0.000
Age45	-2.28251***	0.061	0.000	-2.29984***	0.060	0.000
PerCapHosp	0.07264	0.379	0.848	0.04495	0.378	0.905
PerCapMD	-1.79753	1.140	0.116	-1.83241	1.145	0.110
PerCapPA	0.00011	0.000	0.609	0.0002	0.000	0.360
AFDCBen	-0.00015*	0.000	0.022	-0.00014*	0.000	0.031
AFDCCase	-0.00046	0.001	0.724	-0.00043	0.001	0.739
AL	(Ref)			(Ref)		
AR	-0.0775	0.086	0.366	-0.06263	0.085	0.460
CA	0.23671	0.154	0.124	0.22511	0.153	0.143
CO	0.28313*	0.144	0.049	0.29664*	0.144	0.040
CT	0.34603	0.186	0.064	0.33646	0.186	0.071
DE	0.03191	0.140	0.820	0.05266	0.144	0.714
DC	0.46698	0.400	0.244	0.48202	0.403	0.232
FL	0.04334	0.082	0.599	0.04534	0.083	0.583
GA	0.03194	0.060	0.597	0.02959	0.061	0.626
ID	0.184	0.168	0.273	0.18726	0.165	0.256
IL	0.30755*	0.136	0.024	0.29946*	0.135	0.027
ID	0.11621	0.119	0.327	0.10738	0.118	0.364

IO	0.21214	0.125	0.091	0.20245	0.125	0.105
KS	0.27955	0.174	0.110	0.30076	0.174	0.084
KY	0.01975	0.074	0.791	0.02211	0.074	0.766
LA	0.05536	0.072	0.442	0.04862	0.073	0.503
ME	0.10892	0.125	0.384	0.10668	0.126	0.399
MD	0.1802	0.150	0.231	0.1806	0.151	0.232
MA	0.42052*	0.185	0.024	0.40907*	0.185	0.028
MI	0.28316*	0.131	0.031	0.27720*	0.130	0.033
MN	0.34953*	0.152	0.022	0.34252*	0.152	0.024
MS	-0.03669	0.079	0.645	-0.03597	0.080	0.653
MO	0.15022	0.119	0.206	0.14094	0.118	0.232
MT	0.17665	0.238	0.458	0.20245	0.239	0.397
NE	0.21065	0.179	0.240	0.21397	0.179	0.232
NV	0.13026	0.262	0.619	0.13338	0.259	0.607
NH	0.25335	0.137	0.065	0.24005	0.136	0.079
NJ	0.28319	0.158	0.074	0.26917	0.157	0.088
NM	0.20264	0.126	0.108	0.20681	0.126	0.101
NY	0.42455*	0.203	0.037	0.41140*	0.203	0.043
NC	0.00699	0.073	0.924	0.00358	0.073	0.961
ND	0.04687	0.279	0.867	0.07145	0.278	0.797
OH	0.24764	0.136	0.069	0.23813	0.135	0.079
OK	0.00363	0.088	0.967	0.0043	0.088	0.961
OR	0.18445	0.131	0.161	0.18476	0.131	0.159
PE	0.32521*	0.145	0.025	0.31257*	0.144	0.030
RI	0.19242	0.156	0.217	0.21469	0.155	0.168
SC	-0.0272	0.083	0.742	-0.01949	0.084	0.816
SD	0.34129	0.252	0.176	0.34481	0.252	0.172
TN	0.01827	0.065	0.780	0.02083	0.065	0.749
TX	0.07864	0.071	0.268	0.08393	0.071	0.236
UT	0.42672**	0.136	0.002	0.42133**	0.135	0.002
VT	-0.12032	0.191	0.528	-0.12303	0.193	0.524
VA	0.16744	0.094	0.077	0.1803	0.094	0.057
WA	0.13153	0.130	0.313	0.12792	0.130	0.325
WV	0.19720*	0.088	0.026	0.19986*	0.089	0.024
WI	0.30055*	0.123	0.015	0.29132*	0.123	0.018
WY	0.17295	0.240	0.472	0.18863	0.239	0.430
1963	(Ref)			(Ref)		
1964	0.0531	0.075	0.482	0.05518	0.076	0.468
1965	-0.02617	0.080	0.742	-0.02431	0.080	0.761
1966	-0.01577	0.065	0.807	-0.0108	0.065	0.868
1967	-0.12006	0.072	0.098	-0.12051	0.073	0.098
1968	-0.22582**	0.083	0.007	-0.22509**	0.083	0.007
1969	-0.09584	0.090	0.287	-0.09431	0.090	0.294
1970	-0.07539	0.095	0.427	-0.07542	0.095	0.426
1971	-0.10466	0.084	0.213	-0.09978	0.085	0.239
1972	-0.13846	0.088	0.118	-0.1288	0.088	0.146
1973	-0.23920**	0.092	0.010	-0.22592*	0.093	0.015
1974	-0.20894*	0.103	0.043	-0.19116	0.103	0.065
1975	-0.15969	0.111	0.150	-0.14057	0.112	0.208
1976	-0.22879*	0.108	0.034	-0.21181	0.109	0.052
1977	-0.23068*	0.101	0.023	-0.20746*	0.102	0.042
1978	-0.26321*	0.111	0.018	-0.23503*	0.111	0.035
1979	-0.29983*	0.121	0.013	-0.27596*	0.122	0.024
1980	-0.26466*	0.120	0.027	-0.24821*	0.120	0.040
1967fy	-0.08283	0.090	0.357	-0.0838	0.090	0.351
Midwest*1963	0.09545	0.111	0.392	0.09981	0.111	0.370

Midwest*1964	0.02815	0.110	0.799	0.02898	0.111	0.793
Midwest*1965	0.04095	0.106	0.700	0.03733	0.106	0.725
Midwest*1966	-0.03989	0.096	0.677	-0.04431	0.096	0.643
Midwest*1967	0.02403	0.116	0.836	0.02265	0.116	0.845
Midwest*1968	0.00486	0.111	0.965	0.00466	0.111	0.967
Midwest*1969	-0.04166	0.111	0.707	-0.03805	0.110	0.731
Midwest*1970	-0.06481	0.114	0.569	-0.0594	0.113	0.600
Midwest*1971	0.08272	0.109	0.449	0.08467	0.109	0.439
Midwest*1972	0.06213	0.102	0.543	0.0628	0.102	0.538
Midwest*1973	0.06923	0.110	0.530	0.0672	0.110	0.542
Midwest*1974	0.03148	0.115	0.784	0.02792	0.115	0.808
Midwest*1975	0.01318	0.123	0.915	0.01048	0.123	0.932
Midwest*1976	0.06173	0.114	0.588	0.05746	0.114	0.614
Midwest*1977	0.12524	0.108	0.249	0.12084	0.109	0.267
Midwest*1978	0.14371	0.110	0.194	0.13464	0.110	0.221
Midwest*1979	0.21329	0.121	0.079	0.20706	0.121	0.089
Midwest*1980	0.11176	0.123	0.363	0.10585	0.123	0.388
South*1963	0.02824	0.118	0.812	0.02729	0.119	0.818
South*1964	-0.01773	0.109	0.871	-0.01901	0.110	0.862
South*1965	0.09749	0.100	0.329	0.09848	0.100	0.327
South*1966	0.03002	0.098	0.759	0.02681	0.097	0.783
South*1967	0.09197	0.104	0.375	0.08905	0.103	0.389
South*1968	0.05092	0.106	0.630	0.04785	0.106	0.650
South*1969	0.0059	0.107	0.956	0.00438	0.107	0.967
South*1970	-0.05284	0.109	0.629	-0.05476	0.109	0.615
South*1971	0.08863	0.109	0.415	0.08192	0.109	0.451
South*1972	0.12166	0.103	0.240	0.113	0.103	0.274
South*1973	0.21198*	0.107	0.048	0.20268	0.106	0.057
South*1974	0.1222	0.115	0.288	0.10356	0.115	0.367
South*1975	0.08413	0.113	0.459	0.06848	0.113	0.546
South*1976	0.07768	0.113	0.494	0.05889	0.113	0.603
South*1977	0.10639	0.109	0.330	0.08199	0.109	0.451
South*1978	0.12405	0.112	0.267	0.10056	0.111	0.366
South*1979	0.19791	0.118	0.095	0.17673	0.118	0.136
South*1980	0.24055*	0.115	0.036	0.21468	0.115	0.062
West*1963	-0.06061	0.125	0.629	-0.05569	0.126	0.658
West*1964	-0.05562	0.109	0.611	-0.05615	0.110	0.611
West*1965	0.08186	0.108	0.447	0.0794	0.108	0.462
West*1966	-0.10113	0.090	0.264	-0.10773	0.091	0.237
West*1967	-0.03115	0.117	0.790	-0.03202	0.117	0.784
West*1968	0.04098	0.116	0.723	0.04293	0.117	0.713
West*1969	0.01732	0.108	0.872	0.0186	0.108	0.864
West*1970	0.00764	0.119	0.949	0.00994	0.119	0.934
West*1971	0.05857	0.107	0.586	0.05925	0.108	0.583
West*1972	-0.03395	0.112	0.761	-0.03194	0.112	0.775
West*1973	-0.05395	0.114	0.637	-0.05285	0.115	0.645
West*1974	0.03854	0.116	0.741	0.03401	0.116	0.770
West*1975	-0.0225	0.121	0.852	-0.02356	0.121	0.846
West*1976	0.08884	0.113	0.433	0.08431	0.113	0.457
West*1977	0.03401	0.107	0.750	0.02957	0.106	0.781
West*1978	0.03118	0.116	0.788	0.02493	0.116	0.829
West*1979	0.03579	0.121	0.767	0.02806	0.121	0.817
West*1980	0.08704	0.119	0.464	0.07638	0.119	0.520
Intercept	0.42006	0.217	0.053	0.56084**	0.217	0.010

Sample Size	261100	261100
F	121.829	118.657
p	0.000	0.000
Source: NHIS 1963-1980. SE account for sample design strata and clusters.. *p<0.05; **p<0.01; ***p<0.001. AFDC Moderator 1 and 2 are described in the text.		

Table D1. Birth Weight Regressions				
	Low Birth Weight		Log Birth Weight	
	Coef.	SE	Coef.	SE
Medicaid Prior to Birth Month	0.041	0.1785	0.001	0.0083
Low Income	-0.32	0.2223	-0.005	0.0105
Medicaid*Low Income	-0.602*	0.2639	0.005	0.0159
Age of Mother	-0.073	0.052	0.003	0.003
Age of Mother*Age if Mother	0.002	0.0009	0	0.0001
Mother Non-White	0.852***	0.0878	-0.074***	0.0071
Mother Completed HS	-0.404***	0.0703	0.024***	0.0041
1 Sib	-0.161	0.0952	0.020**	0.0062
2 Sibs	-0.146	0.1457	0.034***	0.0082
3+ Sibs	-0.520*	0.1921	0.059***	0.0122
Abortortion Legal	-0.573	0.291	0.034***	0.0052
AFDC Case Loads	-0.012	0.0118	0	0.0005
AFDC Max Ben	0	0.0005	0	0
MDs	0.004	0.003	-0.000***	0.0001
Hospitals	0.042	0.0476	-0.005*	0.0024
PA Spending	0.001	0.0018	0	0.0001
Head Start Share	-0.138	0.3444	0.002	0.0173
Job Share	0.53	0.4435	-0.021	0.0186
Oth Health Share	-0.258	0.4355	0.017	0.0198
CHC Share	0.432	0.469	-0.031	0.0175
MIC Share	-0.453	0.4871	0	0.0266
MCH Share	-0.08	0.2406	0.005	0.0136
FP Share	0.39	0.3545	-0.008	0.0181
FSP Share	-0.215	0.1607	0.006	0.0079
1b.allstgrp	Ref		Ref	
CA	-0.288	0.4223	0.04	0.0199
CO	0.431	0.2606	-0.027	0.0138
CT	0.002	0.2926	0.011	0.0171
DC	-1.642	0.8191	0.156***	0.0315
FL	-0.241	0.1822	0.014	0.0087
GA	0.07	0.0799	0.011	0.0063
IL	-0.271	0.2178	0.024	0.0138
IN	-0.105	0.2265	0.011	0.0125
IA	0.218	0.2662	0.008	0.0152
KY	-0.205	0.1279	0.016	0.0086
LA	0.238	0.1629	-0.040***	0.0071
MI	-0.071	0.227	0.013	0.0139
MS	-0.21	0.2995	-0.009	0.0128
NJ	0.283	0.3516	-0.004	0.0197

NM	1.360***	0.328	-0.076***	0.017
NY	-0.089	0.3518	0.032	0.0214
NC	0.28	0.1399	-0.021*	0.0081
ND	-0.211	0.2017	0.001	0.0126
OH	-0.356	0.2386	0.009	0.0121
OK	-0.053	0.2435	0.018	0.0137
PA	-0.17	0.1298	-0.013	0.007
SC	-0.631***	0.1428	0.021*	0.0081
WV	0.173	0.2769	0.018	0.0149
WI	-0.248	0.3165	0.016	0.0183
St Grp 1	-0.073	0.3343	0.045*	0.0189
St Grp2	-0.476**	0.135	0.026**	0.0075
St Grp3	-0.139	0.2049	0.01	0.0107
St Grp 4	-0.16	0.2943	0.004	0.0158
St Grp 5	-0.764**	0.2384	0.042**	0.015
St Grp 6	-0.456	0.3198	0.024	0.0182
CA*Low Inc	0.786***	0.1461	-0.009	0.0094
CO*Low Inc	0.439***	0.0963	-0.038***	0.0055
CT*Low Inc	-0.377**	0.1283	0.033***	0.0072
DC*Low Inc	1.201***	0.1641	-0.086***	0.0064
FL*Low Inc	0.431***	0.0373	-0.011***	0.002
GA*Low Inc	0.309***	0.0659	-0.021***	0.0041
IL*Low Inc	0.661***	0.1518	-0.011	0.0093
IN*Low Inc	0.480***	0.0752	-0.011*	0.0052
IA*Low Inc	0.351**	0.1143	-0.027***	0.0073
KY*Low Inc	-0.098	0.1172	-0.001	0.0081
LA*Low Inc	-0.107	0.1416	0.056***	0.0073
MI*Low Inc	0.719***	0.1284	-0.026**	0.0077
MS*Low Inc	0.375***	0.0493	0.019***	0.0025
NJ*Low Inc	-0.395***	0.047	0.019***	0.0028
NM*Low Inc	-0.586***	0.1182	0.048***	0.0086
NY*Low Inc	0.506**	0.1473	-0.020*	0.0088
NC*Low Inc	-0.608***	0.0536	0.044***	0.0035
ND*Low Inc	0.955***	0.1309	-0.047***	0.0079
OH*Low Inc	1.026***	0.1421	-0.01	0.0093
OK*Low Inc	1.098***	0.144	-0.022*	0.0089
PA*Low Inc	0.530***	0.0623	0.044***	0.0033
SC*Low Inc	1.019***	0.0329	-0.010***	0.002
WV*Low Inc	1.579***	0.1699	-0.053***	0.0095
WI*Low Inc	0.838***	0.1387	-0.021*	0.0078
St Grp 1*Low Inc	-1.265***	0.128	0.019*	0.008
St Grp2*Low Inc	0.381***	0.0767	-0.024***	0.0044
St Grp3*Low Inc	0.609***	0.1101	-0.014*	0.0061
St Grp 4*Low Inc	0.769***	0.1209	0.019*	0.0071

St Grp 5*Low Inc	0.263*	0.1242	0.01	0.0081
St Grp 6*Low Inc	1.404***	0.1282	-0.053***	0.0083
1965	-0.079	0.3367	0.002	0.0091
1966	-0.26	0.5217	-0.003	0.0161
1967	-0.336	0.4983	0.006	0.0189
1968	-0.305	0.4254	-0.005	0.0175
1969	-0.762	0.3971	0.005	0.0198
1972	-0.434	0.5034	0.012	0.0201
1965*Low Inc	-0.19	0.2921	0.011	0.0141
1966*Low Inc	0.18	0.3233	0.008	0.0154
1967*Low Inc	0.346	0.2879	0.013	0.0173
1968*Low Inc	0.338	0.4294	0.013	0.0209
1969*Low Inc	0.48	0.4517	0.002	0.0229
1972*Low Inc	0.521	0.4899	-0.015	0.0245
Midwest*1965	0.142	0.3667	0.011	0.014
South*1965	-0.006	0.4105	0.006	0.0125
West*1965	-0.157	0.3521	0.001	0.0106
Midwest*1966	0.156	0.4487	0.013	0.0135
South*1966	0.056	0.4833	0.016	0.0151
West*1966	-0.055	0.438	-0.002	0.0188
Midwest*1967	0.023	0.4511	0.007	0.0188
South*1967	0.426	0.4305	-0.027	0.0162
West*1967	-0.137	0.5219	-0.014	0.0156
Midwest*1968	0.224	0.3373	0.023	0.0156
South*1968	0.26	0.4125	0.001	0.0163
West*1968	0.128	0.4769	-0.003	0.0185
Midwest*1969	0.671	0.412	0.013	0.0192
South*1969	0.656	0.3771	-0.01	0.0151
West*1969	0.893**	0.2838	-0.005	0.0183
Midwest*1972	0.421	0.3275	0.01	0.012
South*1972	0.657	0.4199	-0.005	0.0167
West*1972	0.426	0.315	-0.022*	0.0099
Intercept	-1.904*	0.8205	8.080***	0.0466
Sample Size	13834		13834	

Source: 1964-1969 and 1972 NNS. SE clustered on state. All estimates are weighted. F statistics are not estimable because the number of parameters exceeds the number of clusters. Reference categories not shown. *p<0.05; **p<.01; ***p<0.001

Table E1. OLS: Condition Index

	Low Income			Low Education			Moderate Income			High Education		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	-0.355	0.165	0.037	-0.182	0.178	0.312	0.050	0.144	0.732	0.011	0.117	0.923
Age	-0.017	0.028	0.557	-0.046	0.042	0.281	-0.037	0.029	0.210	-0.032	0.022	0.149
Age*Age	0.000	0.000	0.434	0.000	0.000	0.041	0.000	0.000	0.016	0.000	0.000	0.121
Male	0.006	0.055	0.910	-0.057	0.050	0.257	0.007	0.029	0.801	0.065	0.019	0.002
White	-0.151	0.072	0.042	-0.016	0.050	0.756	-0.119	0.059	0.050	-0.174	0.056	0.003
Married	0.053	0.048	0.281	-0.028	0.037	0.460	-0.047	0.031	0.140	0.018	0.024	0.458
State Abortion Flag	0.120	0.148	0.422	0.299	0.167	0.081	0.077	0.049	0.119	0.054	0.090	0.553
State AFDC Caseload	-0.003	0.007	0.707	0.000	0.007	0.968	-0.016	0.005	0.002	-0.007	0.004	0.111
State Benefit Standard	0.001	0.001	0.220	0.001	0.000	0.025	0.001	0.000	0.004	0.000	0.000	0.272
Sate Unemployment Rate	0.015	0.050	0.767	0.017	0.044	0.704	0.068	0.032	0.042	0.025	0.025	0.328
Cnty per cap Pub Asst	0.001	0.000	0.027	0.000	0.000	0.037	0.000	0.000	0.698	0.000	0.000	0.815
Cnty MD per cap	0.000	0.001	0.417	0.001	0.000	0.000	0.000	0.000	0.594	0.000	0.000	0.916
Cnty Hosp per cap	0.001	0.002	0.583	-0.008	0.004	0.052	-0.004	0.003	0.245	-0.001	0.002	0.645
Cnty Foodstamp Share	-0.062	0.082	0.452	-0.175	0.082	0.039	-0.153	0.044	0.001	-0.066	0.065	0.316
Cnty Headstart Share	0.039	0.159	0.806	-0.002	0.113	0.988	0.083	0.135	0.543	-0.008	0.071	0.913
Cnty Other Health prgm Share	0.170	0.160	0.294	0.106	0.119	0.379	0.035	0.091	0.702	0.025	0.055	0.646
Cnty Jobs Program Share	-0.034	0.106	0.747	-0.132	0.103	0.208	-0.086	0.135	0.526	-0.017	0.060	0.779
Cnty CHC Share	-0.107	0.095	0.265	-0.047	0.086	0.582	-0.126	0.052	0.020	-0.065	0.034	0.066
Cnty MCH Share	0.015	0.148	0.920	-0.443	0.114	0.000	-0.202	0.127	0.118	-0.050	0.051	0.335
Cnty Family Planning Flag	-0.235	0.068	0.001	-0.103	0.051	0.051	-0.077	0.045	0.098	0.038	0.037	0.317

Joint Tests for Fixed Effects				
Birth Year	0.000	0.000	0.000	0.000
Birth State	0.000	0.000	0.000	0.000
Survey Year	0.130	0.020	0.810	0.310
State Specific Trend	0.000	0.000	0.000	0.000
Model Statistics				
Sample Size	5926	6960	5695	10802
Mean of Y	0.09	0.1	-0.05	-0.09
R-Squared	0.2	0.18	0.15	0.08

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.

Table E2. Linear Probability Results: Fair Health or Worse

	Low Income			Low Education			Moderate Income			High Education		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	0.034	0.074	0.650	0.014	0.054	0.802	0.021	0.039	0.589	0.041	0.035	0.258
Age	-0.026	0.012	0.036	-0.024	0.012	0.055	-0.002	0.008	0.799	0.002	0.007	0.797
Age*Age	0.000	0.000	0.141	0.000	0.000	0.110	0.000	0.000	0.896	0.000	0.000	0.514
Male	-0.027	0.019	0.156	-0.031	0.014	0.027	-0.015	0.012	0.207	-0.007	0.007	0.275
White	-0.018	0.022	0.420	-0.031	0.016	0.058	-0.032	0.021	0.132	-0.039	0.013	0.004
Married	-0.063	0.013	0.000	-0.062	0.011	0.000	-0.040	0.008	0.000	-0.028	0.005	0.000
State Abortion Flag	-0.061	0.058	0.298	-0.110	0.049	0.030	-0.037	0.026	0.160	0.038	0.031	0.227
State AFDC Caseload	-0.008*	0.004	0.066	-0.004	0.004	0.274	0.001	0.001	0.461	-0.001	0.001	0.348
State Benefit Standard	0.000	0.000	0.479	0.000	0.000	0.365	0.000	0.000	0.111	0.000	0.000	0.983
Sate Unemployment Rate	0.007	0.015	0.622	0.005	0.021	0.818	-0.003	0.011	0.770	0.006	0.008	0.413
Cnty per cap Pub Asst	0.000	0.000	0.162	0.000	0.000	0.021	0.000	0.000	0.163	0.000	0.000	0.443
Cnty MD per cap	0.000	0.000	0.963	0.000	0.000	0.359	0.000	0.000	0.570	0.000	0.000	0.250
Cnty Hosp per cap	-0.003	0.001	0.001	-0.002	0.002	0.160	-0.001	0.001	0.531	0.000	0.001	0.827
Cnty Foodstamp Share	-0.007	0.036	0.847	-0.058	0.042	0.175	-0.004	0.028	0.899	0.022	0.018	0.230
Cnty Headstart Share	0.140	0.071	0.054	0.041	0.039	0.299	-0.017	0.021	0.408	0.002	0.019	0.894
Cnty Other Health prgm Share	-0.072	0.054	0.189	0.019	0.035	0.582	0.010	0.025	0.683	-0.030	0.012	0.015
Cnty Jobs Program Share	-0.091	0.075	0.230	-0.053	0.042	0.210	0.015	0.014	0.266	-0.008	0.016	0.603
Cnty CHC Share	-0.076	0.033	0.027	-0.086	0.034	0.014	-0.047*	0.027	0.085	-0.023	0.015	0.133
Cnty MCH Share	0.079	0.086	0.365	-0.020	0.054	0.717	-0.039	0.036	0.278	0.014	0.027	0.614
Cnty Family Planning Flag	0.009	0.036	0.806	0.036	0.032	0.257	0.003	0.015	0.850	0.022	0.009	0.018

Joint Tests for Fixed Effects								
Birth Year		0.000		0.000		0.000		0.000
Birth State		0.000		0.000		0.000		0.000
Survey Year		0.150		0.210		0.090		0.210
State Specific Trend		0.000		0.000		0.000		0.000
Model Statistics								
Sample Size	17301		20467	0.13	13358		23501	
Mean of Y	0.14				0.07		0.05	
R-Squared	0.1		0.08		0.06		0.03	

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.

Table E3. Linear Probability Results: High Blood Pressure

	Low Income			Low Education			Moderate Income			High Education		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	-0.2320	0.1010	0.0270	-0.2430	0.1110	0.0340	-0.0700	0.0870	0.4260	0.0210	0.0720	0.7750
Age	-0.0140	0.0290	0.6230	0.0000	0.0280	0.9880	-0.0260	0.0150	0.0980	-0.0300	0.0090	0.0020
Age*Age	0.0000	0.0000	0.4800	0.0000	0.0000	0.1470	0.0000	0.0000	0.0010	0.0000	0.0000	0.0010
Male	-0.0090	0.0330	0.7780	-0.0300	0.0290	0.3150	0.0280	0.0150	0.0670	0.0330	0.0110	0.0040
White	-0.0760	0.0390	0.0590	-0.0430	0.0300	0.1590	-0.0700	0.0310	0.0320	-0.0820	0.0260	0.0030
Married	-0.0030	0.0320	0.9360	-0.0150	0.0240	0.5210	-0.0280	0.0210	0.1930	-0.0050	0.0180	0.8010
State Abortion Flag	0.0950	0.0900	0.2990	0.0600	0.0850	0.4830	0.0830	0.0320	0.0140	0.0560	0.0590	0.3490
State AFDC Caseload	-0.0020	0.0060	0.7180	-0.0050	0.0030	0.1010	-0.0060	0.0020	0.0020	-0.0030	0.0020	0.1220
State Benefit Standard	0.0000	0.0000	0.4870	0.0000	0.0000	0.2420	0.0000	0.0000	0.0010	0.0000	0.0000	0.4770
Sate Unemployment Rate	0.0230	0.0230	0.3220	0.0230	0.0280	0.4200	-0.0090	0.0170	0.6050	-0.0030	0.0100	0.7800
Cnty per cap Pub Asst	0.0000	0.0000	0.0040	0.0000	0.0000	0.2310	0.0000	0.0000	0.8990	0.0000	0.0000	0.8340
Cnty MD per cap	0.0000	0.0000	0.3330	0.0000	0.0000	0.4940	0.0000	0.0000	0.0510	0.0000	0.0000	0.2640
Cnty Hosp per cap	0.0050	0.0020	0.0010	0.0000	0.0020	0.8140	-0.0010	0.0010	0.6090	0.0000	0.0010	0.9020
Cnty Foodstamp Share	-0.0350	0.0720	0.6320	-0.0020	0.0740	0.9780	-0.1340	0.0510	0.0120	-0.0630	0.0390	0.1130
Cnty Headstart Share	0.1110	0.0940	0.2460	0.0490	0.0620	0.4410	0.0980	0.0500	0.0580	0.0210	0.0330	0.5190
Cnty Other Health prgm Share	0.0560	0.0850	0.5120	0.0190	0.0660	0.7770	0.0180	0.0360	0.6170	0.0010	0.0280	0.9610
Cnty Jobs Program Share	-0.0910	0.0770	0.2430	-0.0290	0.0550	0.6070	-0.0850	0.0550	0.1320	-0.0220	0.0260	0.3970
Cnty CHC Share	-0.0610	0.0570	0.2900	-0.0300	0.0350	0.3930	-0.0270	0.0310	0.3920	-0.0320	0.0260	0.2350
Cnty MCH Share	0.2490	0.0750	0.0020	-0.1970	0.0450	0.0000	-0.1980	0.0510	0.0000	-0.0380	0.0310	0.2300
Cnty Family Planning Flag	-0.0860	0.0400	0.0340	-0.0140	0.0340	0.6710	-0.0440	0.0260	0.1010	0.0060	0.0220	0.7840

Joint Tests for Fixed Effects				
Birth Year	0.0000	0.0000	0.0000	0.0300
Birth State	0.0000	0.0000	0.0000	0.0000
Survey Year	0.3200	0.4400	0.1900	0.0100
State Specific Trend	0.0000	0.0000	0.0000	0.0000
Model Statistics				
Sample Size	6591	7824	6439	12147
Mean of Y	0.21	0.2	0.13	0.11
R-Squared	0.16	0.14	0.11	0.07

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.

Table E4. Linear Probability Results: Heart Attack/Heart Disease

	Low Income			Low Education			Moderate Income			High Education		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	-0.012	0.046	0.798	-0.033	0.038	0.392	-0.043	0.048	0.366	-0.022	0.025	0.376
Age	-0.011	0.012	0.369	-0.015	0.009	0.088	-0.008	0.008	0.319	-0.010	0.004	0.023
Age*Age	0.000	0.000	0.818	0.000	0.000	0.202	0.000	0.000	0.167	0.000	0.000	0.037
Male	-0.011	0.011	0.330	-0.014	0.010	0.167	-0.008	0.009	0.387	0.002	0.006	0.742
White	0.001	0.017	0.969	0.019	0.013	0.150	0.009	0.012	0.432	-0.003	0.006	0.691
Married	-0.003	0.010	0.802	-0.014	0.008	0.091	-0.015	0.008	0.081	-0.005	0.004	0.227
State Abortion Flag	-0.005	0.038	0.897	-0.055	0.033	0.101	0.016	0.014	0.287	0.017	0.017	0.334
State AFDC Caseload	-0.001	0.002	0.440	0.002	0.002	0.290	-0.001	0.001	0.451	-0.001	0.000	0.008
State Benefit Standard	0.000	0.000	0.383	0.000	0.000	0.000	0.000	0.000	0.020	0.000	0.000	0.995
Sate Unemployment Rate	0.005	0.013	0.719	0.020	0.011	0.062	0.007	0.008	0.383	0.000	0.003	0.914
Cnty per cap Pub Asst	0.000	0.000	0.522	0.000	0.000	0.016	0.000	0.000	0.298	0.000	0.000	0.029
Cnty MD per cap	0.000	0.000	0.691	0.000	0.000	0.025	0.000	0.000	0.043	0.000	0.000	0.355
Cnty Hosp per cap	0.001	0.001	0.340	-0.002	0.001	0.081	0.000	0.000	0.860	0.000	0.000	0.086
Cnty Foodstamp Share	0.001	0.016	0.928	-0.027	0.026	0.302	0.028	0.025	0.276	0.019	0.025	0.469
Cnty Headstart Share	0.017	0.028	0.539	-0.010	0.027	0.707	-0.019	0.017	0.268	-0.009	0.004	0.044
Cnty Other Health prgm Share	-0.010	0.023	0.678	0.034	0.029	0.256	0.026	0.013	0.049	0.005	0.005	0.319
Cnty Jobs Program Share	0.021	0.016	0.211	-0.019	0.018	0.300	-0.003	0.014	0.855	-0.002	0.005	0.772
Cnty CHC Share	-0.008	0.017	0.635	-0.008	0.021	0.717	-0.026	0.014	0.075	-0.011	0.008	0.155
Cnty MCH Share	0.003	0.045	0.939	-0.062	0.039	0.123	-0.023	0.028	0.425	-0.004	0.007	0.571

Cnty Family Planning Flag	-0.029	0.011	0.016	-0.028	0.010	0.010	-0.005	0.008	0.567	0.008	0.005	0.135
Joint Tests for Fixed Effects												
Birth Year			0.000			0.000			0.000			0.000
Birth State			0.000			0.000			0.000			0.000
Survey Year			0.040			0.600			0.450			0.030
State Specific Trend			0.000			0.000			0.000			0.000
Model Statistics												
Sample Size	6595			7829			6439			12149		
Mean of Y	0.04			0.04			0.03			0.02		
R-Squared	0.06			0.08			0.08			0.04		

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.

Table E5. Linear Probability Results: Adult Onset Diabetes

	Low Income			Low Education			Moderate Income			High Education		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	-0.052	0.055	0.353	0.03	0.041	0.467	0.067	0.045	0.141	0.027	0.054	0.626
Age	-0.011	0.008	0.172	-0.013	0.013	0.324	-0.002	0.008	0.769	-0.003	0.005	0.569
Age*Age	0	0	0.913	0	0	0.016	0	0	0.476	0	0	0.974
Male	0.027	0.014	0.059	-0.005	0.009	0.559	-0.006	0.008	0.425	0.015	0.006	0.02
White	0.004	0.022	0.866	0.009	0.013	0.482	-0.008	0.013	0.546	-0.007	0.008	0.399
Married	0.015	0.01	0.144	-0.001	0.011	0.897	-0.012	0.007	0.107	0.001	0.006	0.849
State Abortion Flag	-0.059	0.039	0.135	0.026	0.022	0.246	0.009	0.011	0.382	-0.012	0.014	0.414
State AFDC Caseload	0.002	0.002	0.283	-0.001	0.002	0.786	-0.004	0.001	0.003	-0.001	0.001	0.544
State Benefit Standard	0	0	0.004	0	0	0.163	0	0	0.297	0	0	0.711
Sate Unemployment Rate	-0.004	0.008	0.652	-0.014	0.011	0.214	0.007	0.007	0.294	0.003	0.008	0.711
Cnty per cap Pub Asst	0	0	0.031	0	0	0.111	0	0	0.435	0	0	0.339
Cnty MD per cap	0	0	0.112	0	0	0.19	0	0	0.997	0	0	0.828
Cnty Hosp per cap	-0.002	0.001	0.028	-0.003	0.001	0	0	0.001	0.731	0	0.001	0.556
Cnty Foodstamp Share	0.002	0.026	0.947	-0.015	0.019	0.424	-0.01	0.022	0.63	-0.005	0.02	0.8
Cnty Headstart Share	-0.024	0.023	0.301	-0.008	0.02	0.692	0.017	0.015	0.252	0.007	0.016	0.668
Cnty Other Health prgm Share	0.055	0.031	0.079	0.012	0.021	0.564	-0.015	0.022	0.494	0.011	0.015	0.472
Cnty Jobs Program Share	-0.013	0.016	0.445	-0.023	0.016	0.148	-0.003	0.022	0.884	-0.013	0.017	0.478
Cnty CHC Share	-0.003	0.027	0.926	-0.001	0.017	0.967	-0.009	0.007	0.258	0	0.012	0.991
Cnty MCH Share	0.041	0.037	0.282	-0.002	0.047	0.97	-0.063	0.025	0.015	-0.013	0.011	0.247
Cnty Family Planning Flag	-0.054	0.025	0.034	-0.005	0.008	0.563	-0.017	0.01	0.083	-0.008	0.011	0.474

Joint Tests for Fixed Effects				
Birth Year	0.000	0.000	0.000	0.000
Birth State	0.000	0.000	0.000	0.000
Survey Year	0.000	0.010	0.050	0.290
State Specific Trend	0.000	0.000	0.000	0.000
Model Statistics				
Sample Size	6593	7826	6438	12148
Mean of Y	0.03	0.03	0.02	0.02
R-Squared	0.13	0.13	0.1	0.06

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.

Table E6. Linear Probability Results: Obese

	Low Income			Low Education			Moderate Income			High Education		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	-0.198	0.136	0.153	-0.127	0.158	0.428	0.014	0.147	0.925	-0.007	0.066	0.922
Age	0.005	0.027	0.856	-0.015	0.026	0.568	-0.014	0.017	0.415	0.003	0.016	0.851
Age*Age	0	0	0.002	0	0	0.035	0	0	0.957	0	0	0.045
Male	0.005	0.033	0.87	-0.005	0.031	0.867	0.032	0.019	0.1	0.042	0.013	0.002
White	-0.118	0.06	0.057	-0.064	0.022	0.006	-0.12	0.046	0.013	-0.165	0.045	0.001
Married	0.041	0.029	0.174	0.002	0.03	0.936	0.018	0.032	0.578	0.025	0.02	0.22
State Abortion Flag	0.151	0.103	0.149	0.525	0.091	0	-0.039	0.06	0.521	-0.021	0.057	0.718
State AFDC Caseload	-0.001	0.006	0.85	0.003	0.004	0.485	-0.007	0.004	0.08	-0.004	0.004	0.269
State Benefit Standard	0	0	0.431	0.001	0	0.095	0	0	0.259	0	0	0.058
Sate Unemployment Rate	-0.005	0.028	0.87	-0.034	0.028	0.24	0.073	0.022	0.002	0.04	0.021	0.071
Cnty per cap Pub Asst	0	0	0.296	0	0	0.073	0	0	0.342	0	0	0.977
Cnty MD per cap	0	0	0.154	0	0	0.07	0	0	0.913	0	0	0.203
Cnty Hosp per cap	-0.005	0.003	0.088	-0.004	0.003	0.086	-0.005	0.003	0.124	-0.001	0.003	0.731
Cnty Foodstamp Share	-0.033	0.092	0.724	-0.118	0.069	0.096	-0.13	0.052	0.015	-0.066	0.042	0.124
Cnty Headstart Share	-0.139	0.109	0.21	-0.018	0.09	0.843	0.091	0.122	0.463	-0.021	0.056	0.715
Cnty Other Health prgm Share	0.092	0.104	0.38	0.034	0.105	0.751	0.028	0.093	0.769	0.011	0.055	0.838
Cnty Jobs Program Share	0.116	0.082	0.166	-0.037	0.091	0.684	-0.081	0.098	0.415	0.019	0.045	0.675
Cnty CHC Share	-0.09	0.046	0.057	-0.002	0.076	0.981	-0.087	0.049	0.082	-0.039	0.03	0.195
Cnty MCH Share	-0.277	0.11	0.016	-0.279	0.085	0.002	0.063	0.144	0.665	-0.023	0.042	0.59
Cnty Family Planning Flag	-0.044	0.062	0.486	-0.067	0.06	0.265	-0.049	0.04	0.234	0.04	0.029	0.169
Joint Tests for Fixed Effects												

Birth Year	0.000	0.000	0.000	0.000
Birth State	0.000	0.000	0.000	0.000
Survey Year	0.580	0.040	0.430	0.000
State Specific Trend	0.000	0.000	0.000	0.000
Model Statistics				
Sample Size	6899	8103	6283	11765
Mean of Y	0.34	0.33	0.24	0.2
R-Squared	0.21	0.16	0.12	0.09

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.

Table E7. OLS: Economic Index

	Low Income			Low Education			Moderate Income			High Education		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	-0.111	0.209	0.597	-0.184	0.188	0.332	0.142	0.288	0.624	-0.287	0.162	0.083
Age	0.029	0.033	0.386	0.069	0.046	0.142	0.06836*	0.039	0.087	0.048	0.034	0.170
Age*Age	0.000	0.000	0.185	0.000	0.000	0.877	0.000	0.000	0.729	0.000	0.000	0.617
Male	0.047	0.035	0.186	-0.038	0.044	0.395	0.000	0.056	0.995	0.039	0.039	0.315
White	0.060	0.076	0.432	0.263	0.079	0.002	0.237	0.055	0.000	0.282	0.098	0.006
Married	0.309	0.034	0.000	0.393	0.051	0.000	0.344	0.061	0.000	0.359	0.045	0.000
State Abortion Flag	0.209	0.159	0.195	-0.069	0.141	0.629	0.088	0.113	0.442	-0.040	0.068	0.557
State AFDC Caseload	-0.001	0.007	0.838	0.002	0.009	0.847	-0.004	0.007	0.516	0.009	0.005	0.085
State Benefit Standard	0.000	0.000	0.787	-0.001	0.001	0.353	0.000	0.000	0.523	-0.001	0.000	0.101
Sate Unemployment Rate	-0.038	0.059	0.523	-0.018	0.048	0.708	0.057	0.058	0.331	0.030	0.033	0.363
Cnty per cap Pub Asst	0.000	0.000	0.070	-0.001	0.000	0.000	0.000	0.000	0.930	0.000	0.000	0.003
Cnty MD per cap	0.001	0.000	0.131	0.000	0.001	0.714	0.000	0.001	0.916	0.000	0.000	0.220
Cnty Hosp per cap	0.011	0.002	0.000	0.001	0.005	0.887	-0.017	0.005	0.003	-0.015	0.005	0.009
Cnty Foodstamp Share	-0.064	0.129	0.620	0.320	0.133	0.021	0.172	0.137	0.217	-0.156	0.110	0.162
Cnty Headstart Share	-0.361	0.168	0.038	-0.298	0.147	0.050	-0.069	0.116	0.554	-0.020	0.118	0.868
Cnty Other Health prgm Share	0.306	0.155	0.054	0.129	0.142	0.368	-0.022	0.088	0.803	-0.121	0.079	0.130
Cnty Jobs Program Share	0.028	0.205	0.892	0.093	0.144	0.522	0.074	0.124	0.554	0.198	0.124	0.116
Cnty CHC Share	0.040	0.099	0.687	0.345	0.124	0.008	0.247	0.123	0.050	-0.019	0.076	0.808
Cnty MCH Share	-0.331	0.165	0.051	-0.470	0.225	0.042	-0.012	0.276	0.964	0.024	0.110	0.829
Cnty Family Planning Flag	0.136	0.090	0.138	0.002	0.067	0.981	-0.073	0.093	0.437	-0.065	0.073	0.378

Joint Tests for Fixed Effects				
Birth Year	0.000	0.000	0.080	0.160
Birth State	0.000	0.000	0.000	0.000
Survey Year	0.710	0.030	0.000	0.000
State Specific Trend	0.000	0.000	0.000	0.000
Model Statistics				
Sample Size	5973	7181	5739	10579
Mean of Y	-0.24	-0.1	0.23	0.44
R-Squared	0.33	0.3	0.14	0.16

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.

Table E8. OLS: Years of Education (Top Coded at 17)

	Low Income			Low Education			Moderate Income			High Education		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	-0.038	0.517	0.941	-0.280	0.603	0.644	0.365	0.696	0.603	0.117	0.503	0.817
Age	0.077	0.088	0.384	0.065	0.113	0.569	-0.129	0.091	0.161	-0.029	0.095	0.764
Age*Age	-0.001	0.000	0.028	0.000	0.000	0.185	0.000	0.000	0.477	0.000	0.000	0.324
Male	0.144	0.116	0.219	-0.068	0.118	0.565	-0.179	0.140	0.208	-0.075	0.087	0.393
White	-0.389	0.206	0.066	0.192	0.241	0.430	0.323	0.201	0.115	0.599	0.284	0.041
Married	0.208	0.111	0.069	0.255	0.128	0.052	0.172	0.085	0.049	0.109	0.075	0.156
State Abortion Flag	0.528	0.527	0.322	-0.062	0.460	0.894	-0.285	0.467	0.545	-0.053	0.211	0.804
State AFDC Caseload	0.006	0.032	0.846	0.016	0.023	0.495	-0.012	0.020	0.533	-0.007	0.014	0.646
State Benefit Standard	0.000	0.001	0.919	-0.001	0.001	0.673	-0.001	0.001	0.353	-0.002	0.001	0.117
Sate Unemployment Rate	0.011	0.216	0.958	-0.068	0.166	0.683	0.148	0.172	0.394	0.136	0.077	0.085
Cnty per cap Pub Asst	-0.001	0.001	0.043	-0.001	0.000	0.000	0.000	0.000	0.364	0.000	0.000	0.023
Cnty MD per cap	0.001	0.001	0.160	0.000	0.001	0.846	-0.001	0.002	0.558	0.001	0.001	0.404
Cnty Hosp per cap	0.038	0.008	0.000	0.002	0.017	0.898	-0.036	0.013	0.010	-0.035	0.009	0.000
Cnty Foodstamp Share	-0.255	0.362	0.486	0.693	0.305	0.028	0.588	0.447	0.196	-0.321	0.278	0.255
Cnty Headstart Share	-0.724	0.629	0.256	-0.886	0.402	0.033	-0.555	0.384	0.156	-0.184	0.285	0.523
Cnty Other Health prgm Share	0.527	0.536	0.331	0.590	0.353	0.102	0.299	0.259	0.253	-0.077	0.281	0.786
Cnty Jobs Program Share	0.245	0.607	0.689	0.067	0.330	0.841	0.284	0.419	0.501	0.528	0.315	0.101
Cnty CHC Share	0.331	0.368	0.373	0.824	0.373	0.033	0.280	0.403	0.491	-0.148	0.203	0.469
Cnty MCH Share	-0.360	0.876	0.683	-1.368	0.594	0.026	-0.119	1.082	0.913	0.106	0.473	0.824
Cnty Family Planning Flag	0.417	0.308	0.183	0.232	0.223	0.304	0.035	0.245	0.887	-0.011	0.208	0.960

Joint Tests for Fixed Effects				
Birth Year	0.000	0.000	0.240	0.010
Birth State	0.000	0.000	0.000	0.000
Survey Year	0.000	0.010	0.000	0.020
State Specific Trend	0.000	0.000	0.000	0.000
Model Statistics				
Sample Size	14658	17612	11211	19558
Mean of Y	12.18	12.33	13.29	14
R-Squared	0.21	0.16	0.15	0.1

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.

Table E9. OLS: Continuous Income to Poverty Ratio

	Low Income			Low Education			Moderate Income			High Education		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	-1.059	0.591	0.080	-0.908	0.813	0.270	-0.895	1.543	0.565	-0.624	0.937	0.509
Age	0.085	0.119	0.478	-0.203	0.207	0.332	0.205	0.334	0.543	0.256	0.195	0.197
Age*Age	0.000	0.001	0.975	0.006	0.004	0.139	0.006	0.005	0.238	0.002	0.001	0.271
Male	0.256	0.099	0.013	0.068	0.243	0.781	0.429	0.359	0.238	0.422	0.210	0.050
White	0.671	0.167	0.000	1.143	0.238	0.000	1.249	0.192	0.000	1.145	0.491	0.024
Married	0.861	0.076	0.000	1.295	0.289	0.000	0.826	0.349	0.023	1.158	0.267	0.000
State Abortion Flag	0.046	0.304	0.880	-0.178	0.323	0.584	0.991	0.452	0.034	-0.196	0.482	0.686
State AFDC Caseload	0.021	0.014	0.150	0.006	0.036	0.879	-0.015	0.047	0.757	0.042	0.031	0.175
State Benefit Standard	0.000	0.001	0.821	-0.001	0.002	0.489	0.000	0.002	0.954	-0.006	0.002	0.001
Sate Unemployment Rate	-0.001	0.108	0.992	-0.053	0.182	0.771	0.399	0.432	0.361	0.171	0.240	0.480
Cnty per cap Pub Asst	-0.001	0.001	0.049	-0.004	0.000	0.000	0.000	0.000	0.987	0.000	0.000	0.966
Cnty MD per cap	0.002	0.001	0.002	-0.001	0.003	0.567	0.001	0.003	0.841	0.002	0.001	0.124
Cnty Hosp per cap	0.008	0.008	0.299	0.002	0.024	0.937	-0.053	0.031	0.093	-0.054	0.030	0.076
Cnty Foodstamp Share	0.179	0.236	0.452	1.501	0.473	0.003	0.629	0.650	0.338	-0.606	0.450	0.185
Cnty Headstart Share	-0.320	0.339	0.349	-0.814	0.438	0.070	0.566	0.664	0.398	0.257	0.654	0.697
Cnty Other Health prgm Share	0.488	0.348	0.168	0.295	0.438	0.504	-0.531	0.398	0.189	-1.364	0.602	0.029
Cnty Jobs Program Share	-0.234	0.556	0.676	0.271	0.567	0.635	-0.024	0.525	0.963	1.459	0.586	0.017
Cnty CHC Share	0.038	0.305	0.902	1.412	0.642	0.033	1.853	0.472	0.000	0.193	0.446	0.668
Cnty MCH Share	-0.472	0.643	0.467	-2.264	1.495	0.137	1.028	1.162	0.381	-1.041	0.987	0.297

Cnty Family Planning Flag	0.199	0.375	0.598	-0.213	0.287	0.463	-0.383	0.590	0.519	-0.574	0.301	0.063
Joint Tests for Fixed Effects												
Birth Year			0.000			0.000			0.080			0.000
Birth State			0.000			0.000			0.000			0.000
Survey Year			0.000			0.020			0.000			0.010
State Specific Trend			0.000			0.000			0.000			0.000
Model Statistics												
Sample Size	15094			18105			11559			20356		
Mean of Y	2.65			3.4			4.6			5.37		
R-Squared	0.21			0.14			0.05			0.06		

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.

Table E10. OLS: Continuous Family Wealth Decile

	Low Income			Low Education			Moderate Income			High Education		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	-0.110	0.741	0.883	-0.331	0.589	0.577	1.548	0.861	0.079	-0.864	0.518	0.103
Age	0.156	0.169	0.360	0.292	0.175	0.102	0.310	0.147	0.042	0.305	0.118	0.013
Age*Age	-0.002	0.001	0.066	-0.00294***	0.001	0.002	-0.002	0.001	0.037	-0.002	0.001	0.001
Male	0.129	0.171	0.456	-0.043	0.186	0.816	0.092	0.139	0.510	0.159	0.110	0.156
White	0.489	0.250	0.057	0.979	0.265	0.001	1.031	0.251	0.000	0.938	0.250	0.001
Married	1.592	0.155	0.000	1.754	0.153	0.000	1.948	0.170	0.000	1.914	0.134	0.000
State Abortion Flag	0.204	0.562	0.718	0.065	0.614	0.917	0.610	0.351	0.090	-0.093	0.316	0.770
State AFDC Caseload	-0.016	0.027	0.555	-0.002	0.029	0.959	-0.016	0.022	0.490	0.021	0.017	0.229
State Benefit Standard	-0.001	0.002	0.666	-0.001	0.002	0.515	0.000	0.001	0.805	0.000	0.001	0.748
Sate Unemployment Rate	-0.080	0.214	0.708	-0.032	0.190	0.868	0.024	0.138	0.862	0.073	0.120	0.545
Cnty per cap Pub Asst	0.000	0.001	0.965	0.000	0.001	0.604	0.000	0.000	0.608	0.000	0.000	0.001
Cnty MD per cap	0.001	0.002	0.634	-0.001	0.001	0.623	-0.001	0.002	0.721	0.000	0.001	0.594
Cnty Hosp per cap	0.033	0.011	0.006	-0.009	0.016	0.567	-0.030	0.014	0.034	-0.021	0.015	0.157
Cnty Foodstamp Share	0.010	0.453	0.982	0.987	0.426	0.025	0.503	0.411	0.228	-0.287	0.371	0.444
Cnty Headstart Share	-1.758	0.707	0.017	-0.467	0.513	0.368	0.003	0.421	0.995	-0.132	0.347	0.705
Cnty Other Health prgm Share	1.089	0.572	0.064	-0.289	0.514	0.577	-0.376	0.422	0.377	-0.200	0.273	0.469
Cnty Jobs Program Share	0.425	0.661	0.524	-0.017	0.423	0.968	0.065	0.325	0.841	0.494	0.365	0.183
Cnty CHC Share	0.195	0.325	0.553	0.852	0.262	0.002	0.930	0.386	0.020	0.105	0.196	0.595
Cnty MCH Share	-2.150	0.746	0.006	-1.851	0.600	0.004	-0.081	1.100	0.941	0.088	0.372	0.815
Cnty Family Planning Flag	0.385	0.320	0.235	0.101	0.284	0.725	-0.363	0.311	0.249	-0.254	0.194	0.197

Joint Tests for Fixed Effects				
Birth Year	0.000	0.000	0.560	
Birth State	0.000	0.000	0.000	0.000
Survey Year	0.450	0.000	0.000	0.000
State Specific Trend	0.000	0.000	0.000	0.000
Model Statistics				
Sample Size	6255	7522	6045	11284
Mean of Y	4.88	5.35	6.05	6.44
R-Squared	0.33	0.34	0.3	0.3

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.

Table E11. OLS (Triple Difference): Condition and Economic Index

	Condition Index With Contextual Controls			Condition Index Without Contextual Controls			Economic Index With Contextual Controls			Economic Index Without Contextual Controls		
	b	se	p	b	se	p	b	se	p	b	se	p
MCAIDSHARE	0.004	0.090	0.963	-0.042	0.107	0.696	-0.158	0.139	0.263	-0.109	0.139	0.439
PRATE	-66.137	41.654	0.119	-74.635	42.622	0.087	6.568	97.222	0.946	16.950	94.006	0.858
MCAIDSHARE*PRATE	-0.882	0.445	0.054	-0.990	0.436	0.028	-0.074	1.099	0.947	0.120	1.044	0.909
Age	-0.045	0.021	0.033	-0.041	0.021	0.061	0.064	0.023	0.008	0.069	0.022	0.004
Age*Age	0.000	0.000	0.003	0.000	0.000	0.003	0.000	0.000	0.697	0.000	0.000	0.603
Male	0.029	0.022	0.187	0.030	0.023	0.189	0.020	0.030	0.510	0.020	0.031	0.527
White	-0.004	0.018	0.805	-0.002	0.019	0.898	0.360	0.038	0.000	0.354	0.038	0.000
Married	-0.133	0.047	0.007	-0.130	0.049	0.010	0.226	0.108	0.043	0.177	0.111	0.119
Birth Year* PRATE	0.006	0.022	0.786	0.011	0.023	0.634	0.008	0.051	0.872	0.001	0.049	0.977
Survey Year* PRATE	0.028	0.011	0.019	0.027	0.011	0.019	-0.012	0.013	0.356	-0.011	0.013	0.421
State Abortion Flag	0.126	0.049	0.013				-0.101	0.057	0.081			
State AFDC Caseload	-0.006	0.003	0.053				0.003	0.005	0.492			
State Benefit Standard	0.000	0.000	0.016				-0.001	0.000	0.027			
Sate Unemployment Rate	0.026	0.021	0.230				0.032	0.034	0.344			
Cnty per cap Pub Asst	0.000	0.000	0.316				0.000	0.000	0.505			
Cnty MD per cap	0.000	0.000	0.455				0.000	0.000	0.147			
Cnty Hosp per cap	-0.003	0.002	0.171				-0.012	0.004	0.006			
Cnty Foodstamp Share	-0.115	0.040	0.006				0.003	0.091	0.971			
Cnty Headstart Share	-0.003	0.060	0.961				-0.111	0.111	0.325			
Cnty Other Health prgm Share	0.053	0.053	0.329				-0.086	0.062	0.174			

Cnty Jobs Program Share	-0.037	0.045	0.419				0.184	0.120	0.134			
Cnty CHC Share	-0.071	0.034	0.042				0.063	0.065	0.342			
Cnty MCH Share	-0.147	0.048	0.004				-0.095	0.084	0.265			
Cnty Family Planning Flag	0.013	0.033	0.699				-0.070	0.057	0.226			
Joint Tests for Fixed Effects												
Birth Year			0.000			0.000			0.070			0.360
Birth State			0.000			0.000			0.000			0.000
Survey Year			0.120			0.120			0.000			0.000
State Specific Trend			0.000			0.000			0.000			0.000
(CONTINUTED…)												
Model Statistics												
Sample Size	17934			18081			17923			18065		
Mean of Y	-0.04			-0.04			0.27			0.27		
R-Squared	0.1			0.1			0.19			0.19		

Source: PSID 1968-2009. See main text for a description of models and data. Joint tests based on an F-statistic.