THREE ESSAYS ON CHILDREN'S HEALTH CARE USE AND HEALTH

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THREE ESSAYS ON CHILDREN’S HEALTH CARE USE AND HEALTH

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The early years of children’s lives are crucial to their future health and development. Disparities in health and skills that emerge during children’s first few years increase with age. Many factors affect children’s health. At an individual level, mother’s education is an influential factor. At a societal level, public policies affect children’s surrounding environment that influences their health. Therefore it is critical that public policies and other determinants of children’s health be studied carefully.

As a nation, U.S. has made significant improvements in children's health over the past century. However, there is a significant increase in the number of children in the U.S. today that suffer from conditions and diseases that have emerged in recent years, including asthma and obesity. These conditions are impediments to children's healthy development and have long lasting effects.

Investment in children’s health yields long term payoffs at the individual as well as societal levels. Healthy children have more opportunities to succeed in schools and more likely to become healthy, productive adults. Benefits extend to society as a whole including reduced dependency and disability, a healthier future workforce, and consequently a stronger economy. Due to these reasons, it is important to understand how health care use and health among children in the U.S. have been affected by some of their key determinants in recent decades.

This dissertation is divided into three chapters. The first chapter examines the feasibility of using compulsory schooling policies as instruments for mother's schooling to examine the causal effect of mother's schooling on children's health care use and health. The second chapter examines the causal effect of insurance coverage on children's health care use and health using evidence from the Medicaid and SCHIP expansions. The third chapter examines the causal effect of welfare reform on children's health care use and health. Findings from this dissertation provide informative insights on key factors that shape children's health and wellbeing and highlight important methodological issues involving such empirical research.

BIOGRAPHICAL SKETCH

Maki Ueyama was born in Osaka, Japan. She has a bachelor’s degree in Economics from Keio University located in Tokyo, Japan. She has master's degrees in Policy Analysis and Management and in Public Administration, both from Cornell University. Her current research interests are in the fields of health and health care in developing countries. She currently resides in Chennai, India.

This dissertation is dedicated to my grandparents:

Yoshinori and Yoshie Ueyama, Kimiko Nakata

and to the loving memory of Daisuke Nakata

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CHAPTER ONE:
CAUTIONARY TALE ON THE USE OF COMPULSORY SCHOOLING POLICIES AS INSTRUMENTS FOR GRADE ATTAINMENT

1.1. Introduction

An important potential non-market benefit of greater schooling is better health for oneself as well as one's children. Theories of the role played by schooling in household and health production functions explained by Becker (1991) and Grossman (1972b) have led to several empirical attempts to quantify its magnitude. Recent literature on the effect of mother's schooling on child health is disparate (e.g. Currie and Moretti 2003 find a positive impact on child health from college schooling spurred by college openings, whereas McCrary and Royer 2006 find no impact from extra schooling spurred by school entry age policies in California and Texas).

As it is possible that schooling both causes and is caused by one's own health status, the literature on the effect of schooling on one's own health status has used three methods to correct for this problem: the inclusion of past health measures (Wagstaff 1993, Bolin et al 2002, Hurd and Kapteyn 2003); differencing between siblings or twins (Wolfe and Behrman 1987, Strauss 1990, Behrman and Rosenzweig 1999); and instrumental variables (IV).[1]

[1] Many instruments have been explored in the past, but they include variables that are potentially endogenous themselves: ancestry, per capita income, and expenditures on education in state of residence in childhood (Berger and Leigh1989); individual's IQ, knowledge of work test scores and parents' schooling (Berger and Leigh1989); parents' schooling, number of siblings, and rural residence and region of residence at age 16 (Sander 1995a, 1995b); parents' schooling, parents' income and state of residence in childhood (Leigh and Dhir 1997); quarter of birth and family background (Adams 2002). More recent studies use policy variables as instruments for education that are more exogenous: compulsory attendance laws, child labor laws, and state characteristics at age 14 in the US (Lleras-Muney 2005); compulsory education laws in Britain and Norway (Chevalier 2004, Black, Devereux, and Salvanes 2004), college openings in a woman's 17th year (Currie and Moretti 2003); primary school construction programs in Taiwan and Indonesia (Chou, Grossman, and Liu 2003, Breierova and

Estimation concerns also affect the study of mother's schooling and child health because of omitted variables that may be captured by the mother's level of schooling such as endowment and time preference (Arendt 2001) both of which will bias the estimate of the effect of mother's schooling upwards. While there is a significant interest in the intergenerational effect of schooling on health, especially in the developing country context (Grossman and Kaestner 1997, Grossman 2005), literature utilizing an IV approach to address these questions is scarce. Some recent exceptions in the U.S. are Currie and Moretti (2003) and McCrary and Royer (2006). Studies in a non-US context are Chou et al. (2004), Breierova and Duflo (2004), Blunch (2005) and Ahmad and Iqbal (2005). Currie and Moretti (2003) show that exogenous increases in college schooling induced by college openings have a beneficial impact on a white infant's health. In contrast, McCrary and Royer (2006) show that school entry policies in California and Texas do not have a significant impact on infant health.

To the author's knowledge, no study to date has examined how schooling of low educated mothers who most likely will not get education beyond compulsory education affects infant, child or adolescent health in the U.S. using nationally representative data. Existing studies only use a few states (Texas and California in McCrary and Royer 2006; subset of states from 1970-1991 and all states from 1992-1999 in Currie and Moretti 2003) or use a subset of mothers (white mothers in Currie and Moretti 2003; native mothers who were born and gave birth in the same state, i.e. Texas or California in McCrary and Royer 2006). With the decline in high school graduation over the last several decades, especially among black females (Heckman and LaFontaine 2007), it is important to understand how this may have affected children's health. To date, McCrary and Royer (2006) is the only study that examined

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Duflo 2004); a measure of draft induction risk during the Vietnam War (DeWalque 2004, MacInnis 2006); school reforms in Denmark (Arendt 2005).

the intergenerational effect of mother's schooling on health in the US. Examining the effect of schooling of low educated mothers on their children's health is also important to better understand the nonmarket returns to schooling.

The aim of this study is to examine the causal effect of mother's schooling on child health using the 1979-2002 National Longitudinal Survey of Youth 1979 (NLSY79), the 1986-2002 NLSY79 Child and Young Adult (NLSY79CY) and the Compulsory Schooling Law (CoSLAW) database collected by Dean Lillard and his colleagues at the Department of Policy Analysis and Management, Cornell University. Since mother's schooling may potentially capture omitted factors in addition to the direct effect of education, this study proposes to use state compulsory schooling policies from the CoSLAW database as instruments for mothers' schooling following earlier studies. To the author's knowledge, the changes in compulsory schooling laws were independently implemented by the states and not as a part of school reform with concurrent changes in other school laws. First, feasibility of using these instruments for mother's schooling is examined, specifically whether these policies are highly predictive of grade attainment and are not in and of themselves related to maternal or child health. The findings suggest that state compulsory schooling laws are not good instruments for mother's schooling for the sample mothers in this study. They also suggest that there were only a few changes in compulsory schooling policies during the time period the sample mothers were affected and therefore only a small proportion of the sample mothers were affected by the policy changes. The compliance rate of compulsory schooling policies was also low. Perhaps due to the effect of compulsory schooling policies on academic achievement and performance, these policies were not good predictors of mother's grade completion and high school diploma receipt.

This study is unique in its use of detailed data. Unlike most of the previous studies that used compulsory schooling laws as instruments, the CoSLAW database provides exact dates of the changes in different provisions of the laws and thus the study is able to exploit the full variation in these laws. Previous studies use data on fewer provisions of compulsory schooling laws taken every several years and assign the same laws to all children in the same cohort (Acemoglu and Angrist 2000, Lleras-Muney 2005, Lochner and Moretti 2004). For example, Acemoglu and Angrist (2000) used compulsory schooling laws that were collected every 3 to 6 years and therefore data for the missing years were interpolated using data from older data (e.g. data for 1924-1928 all come from information collected for 1924). In this study, there is information on compulsory schooling laws for each year and therefore it is more accurate and detailed than the previous studies. The only exception is Bedard and Dhuey (2007) who also use yearly information on compulsory schooling laws. Even with the rich compulsory schooling dataset, however, the study finds that there are not enough variations in the compulsory schooling laws from early 1960s to early 1970s to estimate the causal effect of schooling faced by the NLSY79 cohort. Moreover, this study investigates the possible issues of using the compulsory schooling laws for grade attainment (compliance, various causal pathways in which the laws affect outcomes, high selectivity of the children on which the laws have their main impact) and some of these issues have not been thoroughly examined in the previous studies.

The remainder of the paper proceeds as follows. Section 2 reviews existing literature on mother's schooling and child health using IV approach. Section 3 explains the basic model and conceptual framework underlying the study. Section 4 outlines the proposed identification strategy for examining the causal relationship between mother's schooling and child's health and health care use. Section 5 introduces the data. Section 6 describes the testing done for the IV method, Section 7 provides the OLS results, Section 8 provides discussions and Section 9 offers conclusions.

1.2. Literature

1.2.1. The Effect of Mother's Schooling on Children's Health

Many studies on the effect of mother's schooling on child health treat mother's education as exogenous. While there is rarely a problem of reverse causality when examining the intergenerational effect of education[2], the spurious relationship between mother's education and child health may be caused by omitted third variables such as ability (Behrman and Rosenzweig 2002) and time preference (Fuchs 1982, de Walque 2004, 2005). The previous studies on schooling and health addressed these endogeneity issues by including past health measures (Wagstaff 1993, Bolin et al 2002, Hurd and Kapteyn 2003); exploiting differences between siblings or twins (Wolfe and Behrman 1987, Strauss 1990, Behrman and Rosenzweig 1999); and using instrumental variables (IV) approach (Berger and Leigh 1989; Currie and Moretti 2003; Kenkel, Lillard and Mathios 2006). However, most of the literature has not used instrumental variables to examine the causal relationship of parental education on child health. Some recent exceptions in the U.S. are Currie and Moretti (2003) and McCrary and Royer (2006) and in other countries are Chou et al (2007), Breierova and Duflo (2004), Blunch (2005) and Ahmad and Iqbal (2005).

IV studies using US data yield mixed evidence of the intergenerational effect of schooling on health. Currie and Moretti (2003) examine the effect of mother's education on birth outcomes using Vital Statistics Natality data from 1970 to 1999. They instrument for a mother's college educational attainment with data on college openings, measured as the number of two and four-year colleges in the mother's

[2] In most studies, mother's education is measured by her years of formal schooling, which is considered to be the most relevant measure of education (Grossman 2005). However, a few studies, especially in the developing country context, use informal schooling and in this case, the problem of reverse causality may arise if parents receive schooling after the child is born.

county when she was 17 years old divided by the estimated number of 18 to 22 year olds in the county in that year. Since Vital Statistics Natality data do not collect mother's education from all states until 1992, they used data for all states only from 1992. Due to the nature of their data, they assume that the mother's county of residence at age 17 years is the same as that at the time of her child's birth. According to the author's calculations, in the full NLSY79 sample of mothers from 1979-2002, only about 55% of the mothers resided in the same county at the time of her child's birth and when she was age 17. Although the time period of their study is different from the time period of this study, this calls for further investigation to ensure credibility of their assumptions. The sample consists of 10% of all first-time white mothers aged 24 to 45 years old in the data. Since their identification strategy did not work for black mothers who are less likely to go on to college, they limit their sample to white mothers. They find that mother's education decreases the incidence of low birth weight and premature birth. The magnitude of their IV estimates exceeds their OLS estimates. They suggest that this may be due to greater than average effect of additional year of college education for marginal woman on infant health.

McCrary and Royer (2006) examine the effect of mother's education on infant health using Texas Natality data for 1989 to 2001 and California Natality data for 1989 to 2002. They instrument mother's education with the school entry policies in the state of her child's birth. They assume that the state in which the mother started her schooling is the same as the state of her child's birth.[3] Their sample includes first-time native mothers 23 years old or younger in Texas and California who were born in the state in which they gave birth. They exclude non-singleton births and find no

[3] This assumption is a standard assumption in this literature (e.g. Lleras-Muney 2005, Bedard and Dhuey 2007). McCrary and Royer (2006) mention that close to 90% of children who were born in TX or CA were still living in the same state according to 2000 Census. The mobility rate is roughly the same for all states for all children in 2000 (Census 2003). Although it is not the best assumption, most previous studies base their analyses on these assumptions.

evidence that mother's schooling affects the incidence of low birth weight, premature birth and child mortality. While all their estimates are statistically insignificant, in all specifications used in the study, the coefficients of mother's education for low birth weight and premature birth equations consistently have unexpected signs (i.e. mother's education increases incidence of low birth weight and premature birth) and the coefficient of mother's education for child mortality consistently have expected sign (i.e. mother's schooling decreases incidence of child mortality). The studies by Currie and Moretti (2003) and McCrary and Royer (2006) seem to suggest that schooling of mothers in the upper tail of educational distribution matter more than those in the lower tail. This is contrary to what one would expect: that lower levels of education should matter more than higher education due to diminishing marginal returns to education.

IV studies from other countries find that schooling tends to improve child health. Chou et al. (2007) find that mother's schooling reduces low birth weight and premature births but has no effect on child mortality by using compulsory schooling reform in Taiwan as an instrument. Breierova and Duflo (2004) find that mother’s schooling reduces child mortality by using primary school construction program in Indonesia as an instrument. Blunch (2005) finds that mother's formal schooling decreases the incidence of child mortality but her adult literacy program participation has no effect on child mortality in Ghana by using interaction between her birth cohort and region of birth as an instrument. Ahmad and Iqbal (2005) find that mother's schooling beyond high school increases child’s height-for-age Z score by using primary school construction program and introduction of universal free primary schooling in Nigeria as instruments.

1.2.2. The Effect of Compulsory Schooling Laws on Schooling

There is a set of research that looks into the effect of school entry laws on grade attainment and academic performance. These studies find that overall, entering school early has an adverse effect on academic performance (Bedard and Dhuey 2006, Datar 2006) including increased grade retention (Elder and Lubotsky 2006, Dobkin and Ferreira 2007) but ambiguous effect on grade attainment.[4] There are studies, however, that found evidence for a small positive effect of entering school younger on IQ test scores taken at age 18 (Black, Devereux and Salvanes 2008).

The changes in age and cutoff dates of compulsory school entry affect the age of the youngest child that is compelled to enter school. These policy changes ultimately have several effects on children's schooling (see Bedard and Dhuey 2007 for more details). Theoretically, changes in the age and cutoff dates of compulsory school entry:

1. Forces children who are affected to stay in school for longer/shorter number of years by law. Their grade attainment will be affected if their educational aspirations were to only finish the minimum required years (total years of compulsory schooling effect)
2. Changes the average age of the cohort (cohort effect)
3. Changes the absolute age of the directly affected children when they enter schools (absolute age effect)
4. Changes the relative age of the children who are directly affected and indirectly affected by the cutoff date changes (relative age effect)

Previous literature seems to agree that in the short term, children benefit (or are disadvantaged) academically from entering school later (or earlier). Since those who enter later are older than their peers, they learn the academic materials at a higher

[4] However, controlling for the age at test, children who enter school at a younger age are found to have higher test scores (Black, Devereux and Salvanes 2008).

developmental stage which may affect their academic performance and grade retention. Studies found that due to the higher level of school readiness for older compared to younger children, older children have higher academic achievement but this benefit fades away with grade progression (Stipek 2002, Bedard and Dhuey 2006, Datarr 2006). Consistent with benefits associated with entering school later, there seems to be a disadvantage associated with entering school early; younger children are more likely to repeat a grade (Lincove and Painter 2006). From these evidences, increase in compulsory school entry age or moving the school entry date back seems to favor children's academic achievement and ultimately grade attainment, despite the fact that they will be legally bound to stay in school for shorter number of years if school exit age and cutoff date remain the same.

However, previous studies produce inconclusive results on the longer term effects. On one hand, some find that the benefit for older age goes beyond compulsory schooling years; older students are more likely to participate in college preparatory courses during high school and consequently are more likely to enroll in colleges (Bedard and Dhuey 2006). On the other hand, some suggest that benefit of older age disappears in the early years in elementary school (Stipek 2002), that older children do not have any long term advantages over younger children and in fact, the advantage reverses at some point; younger children perform at least as well as older children throughout high school, college and in the labor market and there is even some evidence that younger children have higher grade attainment (Angrist and Krueger 1991, Dobkin and Ferreira 2007), and are more likely to attend college and have higher wages (Lincove and Painter 2006, Angrist and Krueger 1991, Mayer and Knutson 1999). These studies suggest that despite the academic disadvantages in the earlier years, younger children may end up getting more schooling if the decision to drop out is a function of biological age (i.e. those who enter school at a younger age take more time to attain certain age, consequently receiving more education) or if peer effects are strong (i.e. younger children are less likely to drop out if their peers who are younger are still in school) (Dobkin and Ferreira 2007).

Therefore the net effect of compulsory schooling laws on grade attainment is ambiguous. Some previous studies that examined the direct (reduced form) effect of compulsory schooling policies on grade attainment (as opposed to the effect of being older as a result of the change in compulsory schooling policies) found no effects (Bedard and Dhuey 2007) or inconsistent results (Oreopoulos 2006). The only effect that can be expected is that after the policy change, if the compliance rate is high, there should be a change in the number of children who have at least the minimum number of compulsory schooling years that was in place before the policy change (e.g. if the school entry age goes down and the minimum years of compulsory schooling were nine before the policy change, then there should be more children who have at least nine years of schooling after the policy change.)

It is also found that compliance with school entry laws and grade retention rates vary by sex, race and parental schooling (Dobkin and Ferreira 2007). Females, minorities, and children with parents who have less than a college degree are more compliant with school entry laws and Hispanics and parents who have less than a college degree are less likely to repeat a grade than whites and children with parents who have a college degree or more, respectively (Dobkin and Ferreira 2007). The effect of compulsory schooling laws may vary by subgroups because the minority parents or parents with low education are less likely to make the decision to repeat a grade for their academically struggling children or there may be differences in the type of schools that children are attending (i.e. white children may be attending more rigorous schools than the minority children and therefore their grade retention rate may be higher than the minority children) (Dobkin and Ferreira 2007).

While school entry laws seem to have ambiguous effect on grade attainment for the average child, one recent study by Cascio and Schanzenbach (2007) suggest that the effect of age at school entry on long term educational achievement is clearly present when the sample is disaggregated by socioeconomic status and in fact, the effects for low and high socioeconomic status children go in the opposite directions. They found that controlling for relative age, disadvantaged children (defined by children who received free or reduced-price lunch in kindergarten) who are older (in terms of biological age) are less likely to take college entrance exams than their counterparts who are younger with the same relative age. On the other hand, they found that more advantaged children (defined by children who did not receive free or reduced-price lunch in kindergarten), who are older (in terms of biological age) are more likely to take college entrance exams than their counter parts who are younger with the same relative age. Cascio and Schanzenbach (2007) attribute this difference to the difference in how these two groups spend their time before entering school; disadvantaged children are more likely to receive less quality care while more advantaged children are more likely to receive high quality care in place of formal schooling.

These studies suggest that there may be several problems in using school entry laws as instruments for grade attainment. First, school entry laws affect children's health care use and health status in various causal pathways other than through educational attainment such as grade retention and academic performance. This violates the exclusion restriction assumption of the instrumental variable method. The instrument (i.e. school entry laws) will most likely be correlated with the error term from the outcome equation (i.e. regression of children's health care use and health status on mother's schooling measured by her grade attainment) since there are omitted variables from the outcome equation (i.e. grade retention and academic performance) that are correlated with both the outcome (i.e. children's health care use and health status) and the instrument (i.e. school entry laws).

Second, since school entry laws are much more likely to affect certain segments of the population, affect grade retention rates (i.e. high proportion of students who enter school early repeat grades) and produce varying responses in how the extra time out of school is used by children of different socioeconomic status, those students who actually get additional years of schooling due to changes in compulsory schooling laws are highly selected. Therefore using school entry laws as instruments provide estimates for a highly selected group.

Another set of research looks into the effect of school exit laws on educational attainment. Overall, raising school exit age increases grade attainment and reduces dropout rate for the average child (Oreopoulos 2005, 2008 for U. S. and Oreopoulos 2005, Bono and Galindo-Rueda 2004 for U. K.). The evidence on the effect of school exit laws on educational attainment is consistent unlike those for the school entry laws.[5]

In sum, with the complexities associated with compulsory schooling laws and grade attainment, a thorough investigation of the validity of compulsory schooling laws as instruments for grade attainment is necessary before moving on to examining the relationship between mother's schooling and child's health and health care use.

[5] Some critics suggest that forcing children who might otherwise have dropped out to stay in school will not only lower their welfare (if they did actually make the correct decision to drop out), but will also disrupt other children's learning experiences (White 1996) and will incur extra cost to the tax payers to keep unmotivated children in school. The proponents of raising school exit age suggest that forcing them to stay in school will make them realize the benefits of schooling and prevent them from developing myopic behaviors (Spear 2000, Laibson 1997). Therefore while raising school leaving laws increase grade attainment, whether they are actually 'educated' during the extra time in school is debatable. This is also true for the effect of school entry laws. Because of the complexities in the effect of school entry laws on educational attainment, academic performance, and grade retention, simply looking into the effect of the quantity of mother's schooling on children's health may not bring out the entire picture of the effect of her schooling.

1.3. Conceptual Framework

Health production and health demand models suggest that mother's schooling affects child health through changes in allocative efficiency (Rosenzweig and Schultz 1982, Rosenzweig and Schultz 1989, Kenkel 1991, Kenkel 2000, Rosenzweig 1995, Goldman and Lakdawalla 2002, Glied and Lleras-Muney 2003) and productive efficiency (Grossman 1972a, 1972b, 2000). Allocative efficiency suggests that educated mothers choose more efficient combination of inputs to produce child health. More educated mothers may visit doctors more often, use preventive medical care, and reduce risky behaviors, such as smoking and alcohol consumption that may have negative consequences on the health of the child. Productive efficiency suggests that educated mothers produce better child health from a given set of inputs. They may have more up-to-date information about health, better understand doctor's prescriptions and more likely to carefully follow doctor's prescriptions.

Mother's schooling may also affect children's health through her income; her income may directly increase by getting access to higher paying jobs or indirectly by increasing paternal quality and resources through assortative mating in the marriage market. The effect of mother's schooling on children's health can also be explained by quantity-quality model of fertility which suggests that the changes in income induces a shift in mother's preference from quantity to quality of children (Becker 1960, Becker 1991, Becker and Lewis 1973, Behrman and Rosenzweig 2002, Angrist, Lavy and Schlosser 2005) thereby altering her fertility decisions. As mother's schooling increases the opportunity cost of her time, she desires fewer but higher quality children.

It is hypothesized is that schooling may increase a mother's allocative and productive efficiency of improving her child's health, may increase a mother's access to health care, but may also increase the opportunity cost of her time. Because of the increase in mother's opportunity cost of time, she may decrease the time spent with children and or time taken to visit doctors. Therefore although the predicted impact of mother's schooling on the child's health is positive, the net impact on health care use is theoretically ambiguous.

1.4. Proposed Identification Strategy to Examine the Causal Relationship between Mother's Schooling and Child's Health and Health Care Use

Earlier sections explained that compulsory schooling policies may not be very good instruments for mother's schooling because they affect academic achievement and performance in complex ways and more likely impact highly selected group of children. However, let's suppose that these state policies are good instruments for mother's schooling. Following sections show that even with the absence of these problems of compulsory schooling policies as instruments for mother's schooling, the policies may not serve as good instruments due to lack of variations in the policies for the NLSY79 cohort. First, how compulsory schooling policies can empirically be used to examine the causal relationship between mother's schooling and children's health and health care use if they are good instruments will be explained.

The effect of mother's schooling on child health can be estimated using an IV approach. In the first stage, mother's schooling is estimated using state's schooling policies as instruments:

$$E = \alpha_0 + \alpha_1 IV + \varepsilon \tag{1}$$

where E is mother's schooling (highest grade completed and an indicator variable for high school diploma receipt) and IV is a vector of state policy variables that capture the state's compulsory schooling environment including: the age of compulsory entry and the age of permitted school exit. Data is merged with the instruments using mother's state of residence and relevant year. The algorithm used to assign the instruments to the mothers is explained in the Appendix 1.10. The source of identification is the variation in the compulsory schooling laws across states over time that is uncorrelated with children's health and health care use.

In the second stage, effect of mother's schooling on child health is estimated using her predicted schooling from (1) and other exogenous variables:

$$H = \beta_0 + \beta_1 \hat{E} + \beta_1 C + \beta_2 S + \varepsilon \tag{2}$$

where H is one of the several child health outcomes including: an indicator variable for whether the last checkup was a year ago or more, an indicator variable for whether the child had any illness that required medical attention or treatment in past 12 months, the number of such illnesses, an indicator variable for whether the child had any fractured or dislocated bones that required medical attention or treatment in past 12 months, percentile for height-for-age, percentile for weight-for-age, percentile for body mass index-for-age, an indicator for whether the child is at risk of overweight, and an indicator for whether the child is overweight.[6] For the most part, these are measures that reflect both the use of health care conditional on health status, as well as health status itself.

$\hat{E}$ is mother's predicted education. C is a vector of child's and mother's characteristics including: child's sex, race, age, family size, mother's age and marital status. One must keep in mind that some of these control variables such as family size, mother's age and marital status could themselves be causally affected by schooling and therefore part of the effect of mother's schooling could be acting

[6] I used percentile for body mass index-for-age to define at risk of overweight (85th to less than the 95th percentile) and overweight (95th percentile and above).

through the coefficients of these variables. This issue can be addressed by running the regressions with and without these possible endogenous variables. Fixed effects are included for 1) state of residence when child health measures are taken to capture any time invariant differences in child health outcomes across states with different educational policies, 2) year when child health measures are taken, to capture any trend in child health outcomes for each year across the time span, and 3) child's age to capture any trend in children's health for each cohort. We also include *S*, a vector of time varying county level characteristics of the child's geographical location of residence when the child health and health care use measures were taken including total active non-federal MDs, total patient care by non-fed MDs, total number of hospitals, total number of hospital admissions and total number of hospital beds. For 8% of the sample mothers, entire history of state of residence is not known (i.e. at birth, at age 14, current residence, and the most recent past residence), whereas for others, all of this information is known. An indicator variable is included when schooling policies were merged assuming that she lived in the same state as her first reported state of residence to allow for the possibility that these women systematically differ from the rest of the sample in this study. *S* captures state or county by year differences that may otherwise be confounded with the variation in state educational policies and child health outcomes. Finally, ε is the error term that captures the remaining unobserved factors that are not captured in the equation.

Equations (1) and (2) will be estimated using a linear probability model for dummy dependent variables and a simple linear model for continuous dependent variables. Since the error term is not normally distributed for categorical dependent variables, the use of a linear probability model will produce inefficient coefficient estimates. However, this is not a major problem because estimates are generally similar to those produced by nonlinear models when evaluated at the sample means

(Greene 1993). Also, Angrist (2001) suggests that the use of linear probability models in the first stage do not produce fundamentally different results as the use of nonlinear probability models in the first stage. The standard errors in these equations are clustered at mother's state of residence when she was affected by the school exit laws.

1.5. Data

The data for this study comes from the 1979-2002 National Longitudinal Survey of Youth 1979 (NLSY79) and the 1986-2002 National Longitudinal Survey of Youth 1979 Child and Young Adult (NLSY79CY). The NLSY79 is a nationally representative dataset that consists of 12,686 individuals who were aged 14-21 as of December 31, 1978. They had been interviewed annually from 1979 to 1994 and biennially since 1994. In 1986, a survey of children born to female NLSY79 respondents began and has been conducted biannually since then. In 1994, information on children aged 15 and older was collected separately and has been conducted biannually since then. The NLSY79CY includes a variety of information on child's current health conditions and health history from mother's reports for younger children and from self reports for older children except for weight and height information that were partly measured by interviewers. In this study's sample, 33.9%, 38.1% and 47.2% of the percentile for weight-for-age, percentile for height-for-age and obesity measures (percentile for body mass index-for-age, an indicator for whether the child is at risk of overweight, and an indicator for whether the child is overweight) were calculated using measured height and weight information.

The information on the key variable in this study, mothers' schooling, is available in both NLSY79 and NLSY79CY. Mother's highest grade completed from NLSY79CY and mother's high school diploma receipt from NLSY79 are used. There were considerable amount of measurement and reporting errors in the schooling variables in the NLSY79 and the NLSY79CY. A detailed discussion on the issues with the schooling variables in NLSY79 and NLSY79CY is provided in the Appendix 1.11.

The sample consists of children who are aged 0-19 whose mothers lived in the US when the compulsory schooling laws most likely took effect. We exclude those with mothers who lived abroad when the compulsory schooling laws took effect and with missing information on variables used in each regression. Each observation is at child-year level. Sample sizes vary by dependent variables used for the analysis due to missing observations (for example, 44,401 observations for whether the child had any illness that required medical attention or treatment in past 12 months). The sample size of the mothers in this study is 4,524 of which 3,583 mothers stayed in the same state from birth to at least the year in which she was permitted to leave school legally (hereafter referred to as non movers) and 941 are the mothers who moved during this time period (hereafter referred to as movers).

It is worth noting that when determining the effect of compulsory schooling policies on mother's schooling,[7] the main concern is non mover mothers and how they were affected by the compulsory schooling policy changes because the effect of policies on these mothers captures the pure effect of the policies. For mover mothers, interstate moves may be intentional and may be driven by choice; mover mothers may have moved to other states with higher school entry age to escape from the legal binding to enter school early. Moreover, there may be certain state characteristics that drive the mothers to move to other states for some other schooling related objectives. In either of the cases, the interstate moves by the mover mothers will be endogenous. Therefore, the key comparison should be between the schooling of non mover mothers

[7] This is what I would have used as a first stage in the initially proposed identification strategy, 2SLS method, but as I explain below, 2SLS using compulsory schooling policy variables as instruments does not seem to be a good identification strategy for the sample mothers in this study.

before and after the policy change in each state. In the empirical analyses, by including the state fixed effects, all effects on mother's schooling at the state level are controlled for and therefore essentially the comparison is between mothers within each state.

1.5.1. Compulsory Schooling Law (CoSLAW) Database

Data on state schooling policies regarding compulsory schooling laws on school entry and school exit comes from the Compulsory Schooling Law (CoSLAW) database collected by Dean Lillard and his colleagues at the Department of Policy Analysis and Management, Cornell University. The CoSLAW dataset covers the complete history of compulsory schooling laws for each state. The main source of the data comes from state statutes and laws. Unlike the compulsory schooling laws that most previous studies used for instruments, this dataset has the exact dates of the changes in different provisions of the laws.[8] The key variables used from this dataset are compulsory school entry age and permitted school exit age standardized at September 1st of that school year. In other words, the age of the youngest child that is compelled to enter school in that school year and the age of the youngest child that is allowed to leave school in that school year are included. For example, in California in 1967, children must enter school if they are seven years old by October 1st in that school year. The value for the compulsory school entry age is 6.92 which means that children who are 6.92 years old on September 1st in 1967 must enter school and they are the youngest children that are compelled to enter school in that year. There are some discrepancies with the CoSLAW dataset and the datasets used by earlier studies including Angrist and Krueger (1992) and Bedard and Dhuey (2007). This issue is explained in detail in

[8] Previous studies use data on fewer provisions of compulsory education laws taken every several years with the exception of Bedard and Dhuey (2007).

Data Appendix 1.12. In this study, the CoSLAW dataset collected by Lillard and colleagues are assumed to be free from errors.

Figures 1.1 and 1.2 illustrate the changes in the state compulsory schooling policies from 1930 to 2002 graphically and show changes in policies in the average state during the time period the sample mothers from the study were affected by the policies governing school entry and exit. While there is a general downward trend in school entry age (a slight decrease in the school entry age from age seven by about two to three months) and an upward trend in the school exit age, during the time period the sample mothers were affected by the policies, there were not many changes in schooling laws.

During this time period, there were five school entry age changes in five states, one school exit age change in one state, 14 entry cutoff date changes in seven states and 11 school exit cutoff date change in five states. Tables 1.1 through 1.4 provide the details of the compulsory schooling policy changes. Tables 1.1 and 1.2 list the states that experienced policy changes in the school entry or exit age and the details of the policy changes including: the school year in which the change took effect, entry and exit ages, affected birth cohort, total number of compulsory years of schooling by birth cohort if stayed in the same state throughout the years in which the person was affected by the schooling policies, the total number of mover and non mover mothers that were affected and total number of non mover mothers that were affected. For example, California changed its school entry age from eight to six in the 1967-1968 school year. The birth cohort affected by this change was 1960-1964 and the people in this cohort faced a school exit age of 16 if they remained in California throughout their compulsory schooling years. Therefore due to this school entry age change, those who were born in 1957-1959, 1960, and 1961-1964 were compelled to stay in school for eight years, nine years, and ten years, respectively.

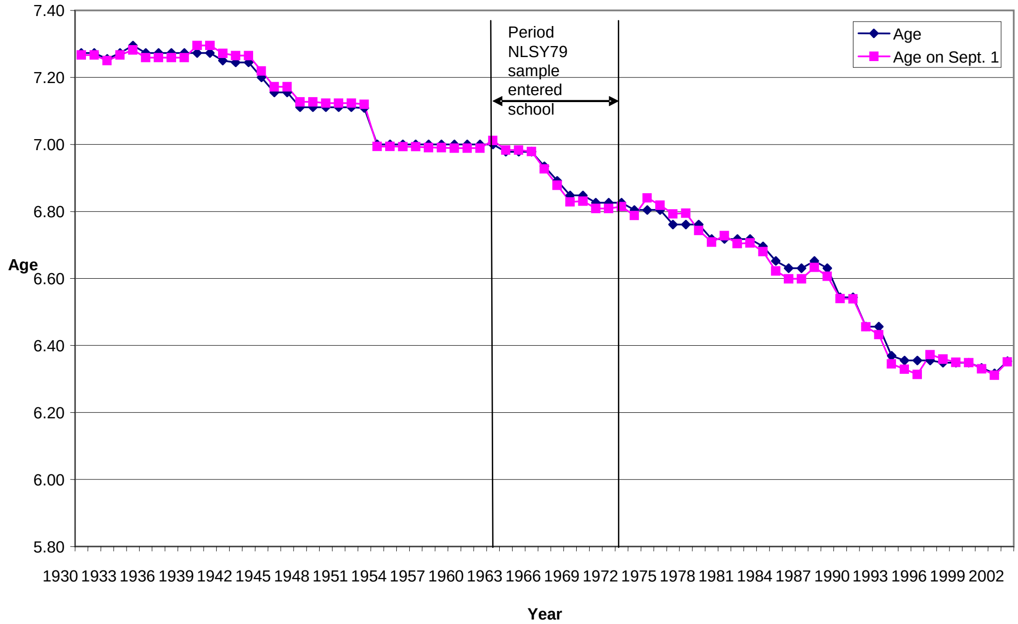

Figure 1.1. Average State-Mandated Age of First Enrollment 1930-2002
Source: Lillard and colleagues from compulsory schooling statutes (Lillard 2007)

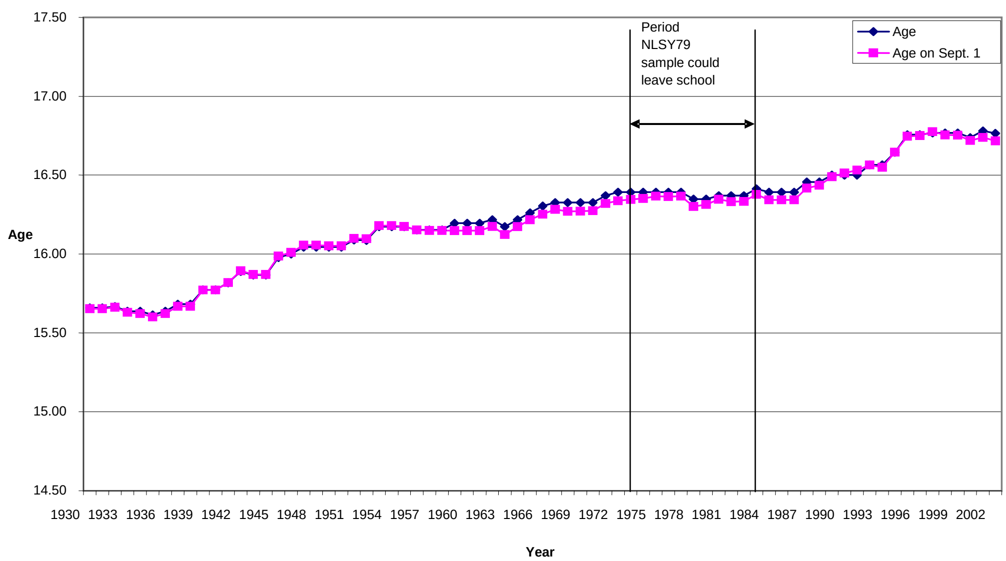

Figure 1.2. Average State Mandated Minimum Age of Permitted School Leaving 1930-2002
Source: Lillard and colleagues from compulsory schooling statutes (Lillard 2007)

Table 1.1. Policy Changes in the Compulsory School Entry Age

						# of mothers affected	
State	**SY of ΔEntry Age**	**ΔEntry Age**	**Exit Age**	**Affected Birth Cohort**	**Compulsory Yrs of Ed by Birth Cohort**	**M&NM**	**NM**
CA	1967-1968	8 to 6	16	1960-1964	1957-1959: 8 yrs 1960: 9 yrs 1961-1964: 10 yrs	352	276
DE	1969-1970	7 to 6	16	1963-1964	1957-1962: 9 yrs 1963-1964: 10 yrs	3	0
NJ	1965-1966	7 to 6	16	1959-1964	1957-1958: 9 yrs 1959-1964: 10 yrs	120	86
NY	1968-1969	7 to 6	16	1962-1964	1957-1961: 9 yrs 1962-1964: 10 yrs	118	84
VA	1968-1969	7 to 6	16	1962-1964	1957-1961: 9 yrs 1962-1964: 10 yrs	48	33

Table 1.2. Policy Changes in the Compulsory School Exit Age

						# of mothers affected	
State	**SY of ΔExit Age**	**Entry Age**	**ΔExit Age**	**Affected Birth Cohort**	**Compulsory Yrs of Ed by Birth Cohort**	**M&NM**	**NM**
MS	1977-1978	7	16 to 13	1962-1964	1957-1961: 9 yrs 1962: 8 yrs 1963: 7 yrs 1964: 6 yrs	18	11

Table 1.3. Policy Changes in the Compulsory School Entry Cutoff Date

						# of mothers affected	
State	**SY of ΔEntry Cutoff**	**ΔEntry Cutoff**	**Entry Age**	**Exit Age**	**Affected Birth Years and Months**	**M&NM**	**NM**
IL	1967-1968	No specified date to December 1st	7	16	1960-1964; Sep-Nov: oldest to youngest in cohort (enter a year early)	25	19
KS	1965-1966	January 1st to September 1st	7	15	1958-1964; Sep-Dec: youngest to oldest in cohort (enter a year later)	1	1
KS	1966-1967	September 1st to January 1st	7	15	1959-1964; Sep-Dec: oldest to youngest in cohort (enter a year early)	1	1
KS	1967-1968	January 1st to December 1st	7	15	1960-1964; Dec: youngest to oldest in cohort (enter a year later)	0	0
KS	1968-1969	December 1st to November 1st	7	15	1961-1964; Nov: youngest to oldest in cohort (enter a year later)	0	0
KS	1969-1970	Novermber 1st to October 1st	7	15	1962-1964; Oct: youngest to oldest in cohort (enter a year later)	0	0
KS	1970-1971	October 1st to September 1st	7	15	1963-1964; Sep: youngest to oldest in cohort (enter a year later)	0	0
NM	1967-1968	No specified date to January 1st	6	17	1961-1964; Sep-Dec: oldest to youngest in cohort (enter a year early)	10	2
OH	1965-1966	September 1st to October 31st	6	18	1959-1964; Sep-Oct: oldest to youngest in cohort (enter a year early)	38	32
SD	1971-1972	No specified date to November 1st	7	16	1964; Sep-Oct: oldest to youngest in cohort (enter a year early)	1	1
TN	1966-1967	December 31st to November 30th	7	16	1959-1964; Dec: youngest to oldest in cohort (enter a year later)	2	2

Table 1.3. (Continued)

						# of mothers affected	
State	**SY of ΔEntry Cutoff**	**ΔEntry Cutoff**	**Entry Age**	**Exit Age**	**Affected Birth Years and Months**	**M&NM**	**NM**
TN	1967-1968	Novermber 30th to October 31st	7	16	1960-1964; Nov: youngest to oldest in cohort (enter a year later)	2	1
TN	1968-1969	October 31st to September 30th	7	16	1961-1964; Oct: youngest to oldest in cohort (enter a year later)	1	1
VA	1968-1969	No specified date to September 30th	7 to 6	16	1961-1964; Sep: oldest to youngest in cohort (enter a year early)	8	6

Table 1.4. Policy Changes in the Compulsory School Exit Cutoff Date

						# of mothers affected	
State	**SY of ΔExit Cutoff**	**ΔExit Cutoff**	**Entry Age**	**Exit Age**	**Affected Birth Years and Months**	**M&NM**	**NM**
MS	1977-1978	January 1st to December 1st	7	16 to 13	1961-1964; Dec: youngest to oldest in cohort (drop a year later)	2	1
KY	1978-1979	No specified date to December 31st	7	16	1962-1963; Sep-Dec: oldest to youngest in cohort (drop a year early)	4	3
NM	1974-1975	January 1st to December 1st	6	17	1957-1964; Dec: youngest to oldest in cohort (drop a year later)	0	0
NM	1975-1976	December 1st to November 1st	6	17	1958-1964; Nov: youngest to oldest in cohort (drop a year later)	2	2
NM	1976-1977	Novermber 1st to October 1st	6	17	1959-1964; Oct: youngest to oldest in cohort (drop a year later)	2	2
NM	1977-1978	October 1st to September 1st	6	17	1960-1964; Sep: youngest to oldest in cohort (drop a year later)	9	4
SC	1976-1977	No specified date to November 1st	7	16	1960-1964; Sep-Oct: oldest to youngest in cohort (drop a year early)	5	3
SC	1979-1980	Novermber 1st to September 1st	7	16	1960-1964; Sep-Oct: youngest to oldest in cohort (drop a year later)	5	5
VA	1974-1975	September 30th to December 31st	<=1967: 7 >=1968: 6	16	1958-1964; Sep-Dec: oldest to youngest in cohort (drop a year early)	29	22
VA	1979-1980	December 31st to November 30th	6	16	1963-1964; Dec: youngest to oldest in cohort (drop a year later)	5	4
VA	1980-1981	Novermber 30th to October 31st	6	16	1964; Nov: youngest to oldest in cohort (drop a year later)	1	1

As evident from the table, there were not many changes in school entry or exit age during the time period when the sample mothers went to school. Most mothers who were affected by school entry changes lived in California followed by New Jersey and New York.

Tables 1.3 and 1.4 list the states that experienced policy changes in the school entry or exit cutoff dates and the details of the policy changes including: the school year in which the change took effect, the change in cutoff date, entry and exit ages, affected birth months, the total number of mothers that were affected and total number of non mover mothers that were affected. For example, Kansas changed the entry cutoff date from January 1st to September 1st in the 1965-1966 school year. From this policy change, those who were born from September to December were affected; after the policy change, those who were born in these months entered a year later and therefore they became the oldest (from the youngest) in their cohort. Compared to the changes in school entry and exit ages, more states changed their cutoff dates during the time period of interest. However, many cutoff date policy changes only affected very few mothers and some changes did not affect any sample mothers.

The number of states with the policy changes that affected the sample mothers is extremely small. Only five states (four states for school entry and one state for school exit) changed compulsory schooling ages during the time period. Similarly, only 12 states (seven states for school entry and five states for school exit) changed compulsory schooling cut off dates during the time period. Since some states changed both school age and cutoff date, there were basically only 13 states that experienced either school age or cutoff date changes during the time period. The key here is that there is not much variation in compulsory schooling policies within states over time that affected the sample mothers in this time period.

Table 1.5. Number of Mothers Affected by Policy Changes in the Compulsory School Entry and Exit Ages

# of Affected Mothers	Total	Non-Movers	Movers
School Entry Policy Only	641	479	162
School Exit Policy Only	17	11	6
Both Polices	1	0	1
Neither	3865	3093	772
Total # of Mothers	4524	3583	941

Table 1.6. Number of Mothers Affected by Policy Changes in the Compulsory School Entry and Exit Cutoff Dates

# of Affected Mothers	Total	Non-Movers	Movers
School Entry Policy Only	78	58	20
School Exit Policy Only	53	39	14
Both Polices	11	8	3
Neither	4382	3478	904
Total # of Mothers	4524	3583	941

Tables 1.5 and 1.6 summarize the number of mothers that were affected by the school entry and exit policies. As mentioned before, the main concern is the educational experiences of the non mover mothers. The tables show that only very few mothers were affected by the laws. Only 11% (i.e. (479+11)/4524=.11) of the mothers were non-movers and were affected by the school entry or exit age policy changes. Moreover, only 2% (i.e. (58+39+8)/4524=.02) of the mothers were non movers and were affected by the school entry or exit cut off date policy change. Those movers who moved to states with the same compulsory schooling age or cutoff date can technically be counted as non movers since it is unlikely that their relocation to different states is driven by compulsory schooling related issues. However, these types of movers were very small in number; among the movers, only about 5 mothers moved to a state with the same entry and exit ages and cutoff dates. For the rest of the

mothers, about a half went to states with the same age but different cutoff date and a half went to states with both different age and cutoff date.

1.5.2. Descriptive Statistics of Sample Children and Their Mothers

Before proceeding with the tests for the IV method, descriptive statistics of the final sample of children and their mothers are presented. Table 1.7 shows selected descriptive statistics for children and mothers for all children and by mother's education. The observations are at the child–year level. The average age of the children is about seven years and children whose mothers do not have a high school diploma are older by about 0.8 years. This is consistent with the usual trend of educated mothers delaying their child bearing and therefore having younger children. Mothers without a high school diploma are slightly younger, less likely to be married and more likely to be Hispanic or Black. Children with mothers without a high school diploma have larger families and more likely to have a condition that require treatment.

Table 1.7. Child's and Mother's Characteristics: All Children and by Mother's Schooling (Dependent Variable: Ever Ill)

Variable	All	HS Diploma or more	No HS Diploma
child's age	7.175 (4.086)	6.904*** (4.034)	7.778*** (4.137)
child's sex: female	0.492 (0.500)	0.489 (0.500)	0.496 (0.500)
child's race: hisp	0.192 (0.394)	0.151*** (0.358)	0.278*** (0.448)
child's race: black	0.307 (0.461)	0.293*** (0.455)	0.335*** (0.472)
child's race: non-black, non-hisp	0.501 (0.500)	0.556*** (0.497)	0.386*** (0.487)
mother's years of education	12.479 (2.510)	13.457*** (1.942)	10.407*** (2.309)
mother's age	32.711 (4.858)	33.329*** (4.818)	31.405*** (4.680)

Table 1.7. (Continued)

Variable	All	HS Diploma or more	No HS Diploma
mother's marital status: never married	0.148	0.113***	0.223***
	(0.356)	(0.317)	(0.416)
mother's marital status: married	0.650	0.713***	0.519***
	(0.477)	(0.453)	(0.500)
mother's marital status: separated	0.076	0.059***	0.112***
	(0.265)	(0.235)	(0.315)
mother's marital status: divorced	0.117	0.110***	0.133***
	(0.322)	(0.313)	(0.340)
mother's marital status: widowed	0.008	0.005***	0.014***
	(0.089)	(0.072)	(0.117)
Family size	4.415	4.273***	4.716***
	(1.480)	(1.272)	(1.809)
# of children in the family	1.966	1.728***	2.470***
	(1.377)	(1.142)	(1.666)
Have condition that requires treatment	0.055	0.054**	0.058**
	(0.228)	(0.225)	(0.234)
N	44649	30320	14329

Note: * significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.8 shows children's health measures for all children and by mother's education. Children with mothers without a high school diploma are more likely to have reported of having his/her last checkup more than a year ago. However, contrary to the expectations, children with mothers with a high school diploma are more likely to have reported having at least one illness that required a doctor's care in the past year and the number of such illnesses is also greater for this group. They are also more likely to report having at least one bone fracture or dislocated bone that required a doctor's care in the past year. Moreover children with mothers with a high school diploma have higher weight and height for age but the overweight measures (i.e. BMI for age, at risk of being overweight and overweight) do not show any statistically significant differences between the two groups.

Table 1.8. Children's Health Outcomes for All Children and by Mother's Schooling

Variable	All	HS Diploma or more	No HS Diploma
last check up: 1 yr ago or more	0.322 (0.467)	0.311*** (0.463)	0.343*** (0.475)
ever been ill past year	0.349 (0.477)	0.380*** (0.485)	0.28*** (0.450)
# of times ill past year	0.825 (1.942)	0.896*** (1.928)	0.673*** (1.957)
ever had bone fractures past year	0.025 (0.155)	0.026*** (0.159)	0.021*** (0.144)
percentile: weight-for-age	56.196 (30.549)	56.872*** (30.497)	55.123*** (30.602)
percentile: height-for-age	54.946 (31.634)	56.562*** (31.567)	52.597*** (31.586)
percentile: BMI-for-age	55.534 (33.308)	55.732 (33.324)	55.245 (33.285)
% at risk of overweight	0.267 (0.442)	0.268 (0.443)	0.266 (0.442)
% overweight	0.137 (0.344)	0.137 (0.344)	0.137 (0.344)

Note: * significant at 10%; ** significant at 5%; *** significant at 1%

These simple bivariate relationships seem to indicate that children with less educated mothers have less access to preventive care but in terms of health status, they do not seem to be that worse off. In fact, children with more educated mothers reported having more illnesses and being heavier for their age. While positive association between mother's schooling and illnesses and bone fractures seem contrary to what one would expect, this may imply that higher educated mothers are more aware of the symptoms of illness and therefore more likely to report children as ill (this will probably not apply to fractures since bone fractures are obvious to anyone regardless of the educational level) or that children of higher educated mothers are more likely to engage in activities that have a higher risk of illness or bone fractures such as outdoor sports activities and traveling to places with different climate, etc. Children of higher educated mothers may also seek more medical attention for their illnesses (and dislocated or fractured bones to some extent although it is less likely that any mother will forgo medical attention for these injuries) compared to less educated mothers. The children of more educated mothers may also be heavier for age (without being at risk overweight or being overweight) because they are more well nourished than their counterparts. To find out if these associations are causal, multivariate analyses with corrections for endogeneity of mother's schooling are necessary.

1.6. Testing IV Method

Using the proposed identification strategy to correct for endogeneity of mother's schooling is possible only if compulsory schooling policy variables provide enough variation for mother's education and satisfy other criteria. Instrumental variable method is a valid method when two conditions are met:

1. Instrument relevance: The instrument must be correlated with the treatment of interest. In the regression of outcome Y on instrument Z, $Y = \alpha_0 + \alpha_1 Z + \varepsilon$, the instrument relevance condition says that $\alpha_1 \neq 0$.
2. Exclusion restriction: The instrument must not be correlated with the error term, i.e. omitted variables in the outcome equation. In the context of the regression above, the exclusion restriction says that $Cov(Z, \varepsilon) = 0$.

In the context of this study, compulsory schooling must be correlated with mother's schooling but cannot be correlated with the error term from the child health regression or in other words, omitted variables from the regression of child health on mother's schooling. As explained in the previous section, however, there are a few key issues to note about the compulsory schooling laws. First, there were only a few changes in the compulsory schooling laws during the period when the sample mothers were affected by these laws. In fact, only five states changed the school entry or exit age laws and 12 states changed their school entry or exit cutoff dates during this time. Second, mainly due to the few changes in compulsory schooling laws during this time period, only very few mothers were actually affected by the law changes. Only 11% of the mothers were non movers affected by the school entry or exit age and 2 % of the mothers were non movers affected by cutoff date changes. These two issues cast a doubt on whether the policy changes could have really affected mother's schooling. Therefore whether the policy changes had the expected effects on mothers are examined to determine the feasibility of using compulsory schooling policies as instruments for mother's schooling.

As one may recall, California, New Jersey, New York, and Virginia changed their school entry age and Mississippi changed its school exit age during the time period of interest. Table 1.9 compares the mean highest grade completed by mother, the proportions of mothers with a high school diploma, 8th grade completion, 9th grade completion, and minimum years of compulsory schooling for those who were affected and not affected by the policy changes. The data is from 1979 to 2002 and includes only those mothers who were non movers. Since only non movers are examined, it was possible to calculate the minimum number of years in which the mothers were legally bound to stay in school. The first column shows the means for all states, columns 2 and 3 show the means for all states that were affected by the school entry age changes, columns 3 to 10 are the means of each state that were affected by the school entry age changes and the last two columns are the means of Mississippi which was the only state that changed the policy on school exit age. Recall that all states that changed policies on school entry age lowered the school entry age (Table 1).

Table 1.9. Comparison of Highest Grade Completed, High School Diploma Receipt, 8th Grade completion, 9th Grade Completion and Minimum Years of Compulsory Schooling for All States and by Selected States for Mothers Affected and Not Affected by Compulsory School Entry or Exit Age Changes

	All States	CA, NJ, NY, VA	
		not affected	affected
HGC	12.844 (2.608)	12.995 (3.096)	12.854 (2.532)
HS dip	0.691 (0.462)	0.658 (0.475)	0.634 (0.482)
8th grade	0.981 (0.138)	0.958** (0.201)	0.981** (0.136)
9th grade	0.958 (0.201)	0.936** (0.245)	0.967** (0.180)
Min yrs	0.938 (0.242)	0.943 (0.232)	0.927 (0.261)

	CA (FIPS=6)		NJ (FIPS=34)		NY (FIPS=36)		VA (FIPS=51)		MS (FIPS=28)	
	not affected	affected	not affected	affected	not affected	affected	not affected	affected	not affected	affected
HGC	12.435 (3.786)	12.634 (2.595)	13.972* (2.602)	13.081* (2.595)	13.293 (2.732)	13.083 (2.760)	12.723 (2.123)	13.515 (2.108)	12 (2.498)	11.727 (2.724)
HS dip	0.594 (0.493)	0.598 (0.491)	0.75 (0.439)	0.776 (0.419)	0.678* (0.469)	0.571* (0.498)	0.696 (0.465)	0.727 (0.452)	0.654 (0.485)	0.545 (0.522)
8th grade	0.906*** (0.293)	0.971*** (0.168)								
9th grade			0.972 (0.167)	0.977 (0.152)	0.957 (0.204)	0.964 (0.187)	0.979 (0.146)	1 (0)	0.923 (0.272)	0.818 (0.405)
Min yrs	0.905 (0.293)	0.917 (0.277)	0.972 (0.167)	0.953 (0.212)	0.957* (0.204)	0.905* (0.295)	0.979 (0.146)	1 (0)	0.923 (0.272)	1 (0)

Note: * significant at 10%; ** significant at 5%; *** significant at 1%

Since the states that lowered their school entry ages did not make any changes in the school exit age during the time period when the sample mothers were affected by the school exit laws, the number of years that the mothers were legally bound to stay in school increased after the policy change. If the new policy was effectively enforced, after the policy change, there should have been more mothers that completed the minimum years of compulsory schooling that was in effect before the policy change, i.e. eight years for California and nine years for the rest of the states. Similarly, Mississippi lowered its school exit age but the school entry age remained the same during the sample period. Therefore, if the new policy was effectively enforced, after the policy change, there should have been fewer mothers that completed the minimum years of compulsory schooling that was in effect before the policy change, i.e. nine years. The last row shows whether the mother has completed the minimum years of compulsory schooling that had legally bound her. This variable captures the compliance rate of the policies in place. The compliance rate should remain the same regardless of whether there were changes in compulsory schooling laws. In fact, if there are changes in compliance rates before and after the compulsory schooling policy change, it is a problem. This is because the change in compliance rate may be an indication of other changes that simultaneously occurred at the time when the compulsory schooling policy changed in that state.

The effect of policy change is ambiguous for mother's grade completion and high school diploma receipt. Compulsory schooling policies dictate the amount of time children must stay in school but not the grade they must complete. Therefore the policy change may change the number of years the mothers attended school but may or may not have changed their grade completion since some may have repeated or skipped grades (Oreopolous 2006). Moreover, whether or not compulsory schooling policies affect mother's schooling decisions depend on where they lie in the educational distribution. For those who lie in the lower tail of the spectrum, compulsory schooling policies may affect high school completion if their perceived cost of high school graduation changes due to the policy changes (Orerpolous 2006). For example, if the minimum years of compulsory education increases (by a decrease in the school entry age or an increase in the school exit age), the remaining number of years until high school completion is lower thereby decreasing the perceived cost of high school graduation. If the perceived cost of high school graduation decreases, more students are likely to stay in school until graduation. However, for those who lie in the upper tail of the spectrum, change in compulsory schooling policy will not be binding since they are likely to continue their education beyond compulsory schooling anyway.

There are certain effects of compulsory schooling policy change on mother's schooling that are unlikely to happen if people respond rationally to the policy changes, however. For example, going back to the previous example, if the minimum years of compulsory education increases, students in the lower tail of educational distribution will most likely increase years of schooling and those in the upper tail of educational distribution will most likely not change their years of schooling. This means that it is highly unlikely for the overall years of schooling to decrease if the minimum years of compulsory education increase since no one will decrease years of schooling if they are making rational decisions. Similarly, it is unlikely for the overall years of schooling to increase if the minimum years of compulsory education were to decrease. In this study, for states that changed their compulsory schooling entry age, mother's grade completion and high school diploma receipt are expected to either stay the same or increase whereas for states that changed their compulsory schooling exit age, mother's grade completion and high school diploma receipt are expected to either stay the same or decrease. In conclusion, while it is possible to eliminate some effects, the exact effects of compulsory schooling policies on mother's grade completion and high school diploma receipt are ambiguous.

Table 1.9 shows that for all mothers, the average highest grade completed is higher than 12th grade. Approximately 70% of the mothers have a high school diploma. 98% and 96% of the mothers have completed 8th grade and 9th grade, respectively. The overall compliance rate is about 94%. When only the states that were affected by school entry age policies are examined, there is a statistically significant difference between the affected and unaffected mothers only for 8th and 9th grade completion. As briefly explained before, mothers should have more minimum years of schooling before policy change than after policy change. Effects on grade completion and high school diploma receipt should either remain the same or increase after policy change. Compliance rate is also expected to remain the same even after policy change.

The effect of the policy change on the proportion of mothers with 8th or 9th grade completion is positive, consistent with the expectations. The compliance rate decreased after the policy change which is not expected but this change is statistically insignificant. Mother's highest grade completed and the proportion of mothers with high school diploma decreased after the policy change which is also unexpected although they are both statistically insignificant. This may be an indication that the mothers in the sample are somehow not bound by the compulsory schooling policy, especially after the policy change. This is a problem as policies need to be binding to be valid instruments. To find out how the effects vary by state, comparisons are conducted by state.

California, which had the most number of mothers that were affected by the policies, showed that regardless of which schooling variables used, all schooling variables indicated that mothers who were affected had more schooling than those who were not affected although most differences were not statistically significant. Recall that in 1967, California changed its compulsory entry age from eight to six. This should have meant that for mothers who were born between 1957 and 1959, the minimum years of compulsory schooling were eight years, for those who were born on 1960, the minimum years of compulsory schooling were nine years and for those who were born between 1961 and 1964, the minimum years of compulsory schooling were ten years. When mothers who were born before and after the policy change are compared (those who were born in 1957-1959 vs. those who were born in 1960-1964), mothers who were affected by the policy change (i.e. those who were born in 1960-1964) are indeed more likely to have at least 8 years of education (97.1% vs. 90.6%), higher grade completion (12.634 vs. 12.995) and higher rate of high school diploma receipt (59.8% vs. 59.4%). The compliance rate was quite low (around 90%) throughout the sample period in California, however, and although the rate increased after the policy change from 90.5% to 91.7%, this is still very low and may be problematic since it indicates that the policy may not have been complied with.

Unlike California, many effects of policy change in New Jersey and New York were in unexpected directions (indicating that affected mothers had less schooling than those who were not affected) although again most effects were statistically insignificant. Again recall that New Jersey lowered its entry age from seven to six in 1965 and therefore mothers who were born in 1957-1958 had minimum of nine years of compulsory schooling whereas those who were born in 1959-1964 had minimum of ten years of compulsory schooling. New York lowered its entry age from seven to six in 1968 and therefore mothers who were born in 1957-1961 had minimum of nine years of compulsory schooling whereas those who were born in 1962-1964 had minimum of ten years of compulsory schooling. This should have meant that mothers who were born in 1959-1964 in New Jersey and in 1962-1964 in New York should have been exposed to greater minimum number of years in school and therefore these mothers are expected to have higher completion rate of nine years of compulsory schooling, higher (or comparable but not lower) grade attainment and higher (or comparable but not lower) rate of high school completion than the earlier cohorts.

When mothers who were born before and after the policy change are compared (i.e. not affected vs. affected), the only statistically significant change was decrease in mother's HGC after the policy change by about 0.9 years, an effect in the unexpected direction. Both the proportion of mothers with high school diploma and 9th grade completion increased, but they were not statistically significant. Moreover, while statistically insignificant, the compliance rate decreased slightly which again weakens the validity of compulsory schooling laws as predictors of mother's schooling. In New York, the only expected effect is seen for the proportion of mothers with 9th grade completion although the effect is statistically insignificant. Both mother's highest grade completed and the proportion of mothers with a high school diploma decreased (statistically significant effect only for the proportion of mothers with a high school diploma) and these effects are not in the expected direction. The compliance rate is also decreasing considerably and again, this poses a big problem on the study's identification method.

As for Virginia, again recall that Virginia lowered its entry age from seven to six in 1977 and therefore mothers who were born in 1957-1961 had minimum of nine years of compulsory schooling whereas those who were born in 1962-1964 had minimum of ten years of compulsory schooling. This should have meant that mothers who were born in 1962-1964 in Virginia should have been exposed to greater minimum number of years in school and therefore these mothers are expected to have higher completion rate of nine years of compulsory schooling, higher (or comparable but not lower) grade attainment and higher (or comparable but not lower) rate of high school completion than the earlier cohorts. Similar to California, mothers in Virginia who were affected by the policy changes (i.e. mothers who were born in 1962-1964) had more schooling than those who were not affected which is consistent with the expectations although none of the effects are statistically significant.

Mississippi experienced a change in the school exit age. Again recall that Mississippi lowered its exit age from 16 to 13 in 1977 and therefore mothers who were born in 1957-1961 had a minimum of nine years of compulsory schooling whereas those who were born later had fewer minimum years of compulsory schooling (1962 birth cohort had eight years, 1963 birth cohort had seven years, and 1964 birth cohort had six years). This should have meant that mothers who were born in 1962-1964 in Virginia should have been exposed to fewer minimum number of years in school and therefore these mothers are expected to have lower completion rate of nine years of compulsory schooling, lower (or comparable but not higher) grade attainment and lower (or comparable but not higher) rate of high school completion than the earlier cohorts. These negative effects are found for those mothers who were affected by the policy change as compared to those who were not, although none of the effects are statistically significant. Figures 1.3 to 1.7 provide more detailed graphical pictures of the change in mother's schooling by birth cohort for the above states. The vertical lines are there to indicate the minimum years of compulsory schooling to which each birth year cohort was legally bound. Even in the states that had the expected effects in the descriptive comparison above, there are no visible changes in mother's schooling before and after the policy change. Figure 1.8 shows the change in mother's schooling by birth cohort for all the states that did not change their policies. Here, no changes in the schooling variables are expected before and after the change.

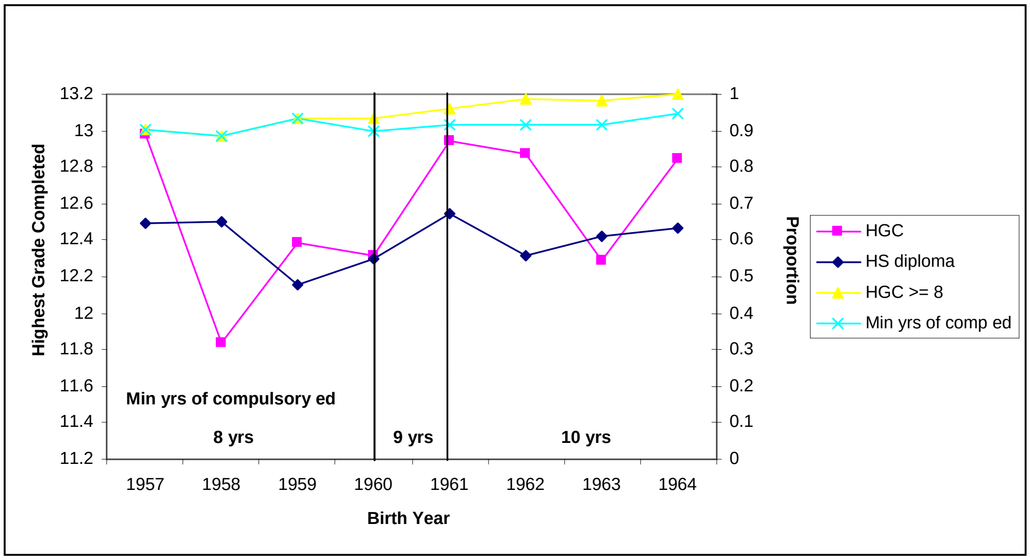

Figure 1.3. Changes in the Average Years of Mother's Schooling by Birth Year for California

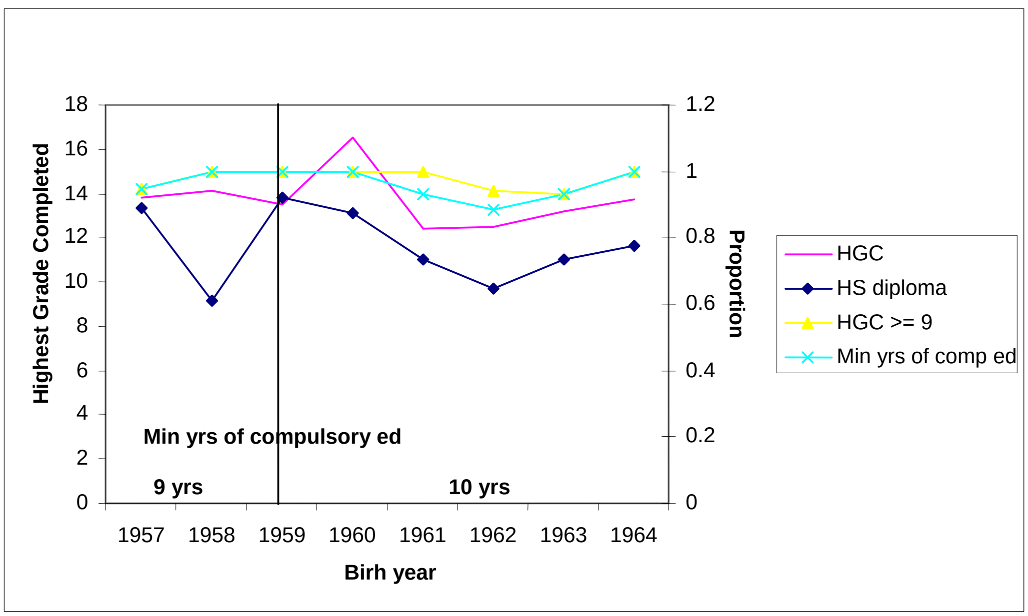

Figure 1.4. Changes in the Average Years of Mother's Schooling by Birth Year for New Jersey

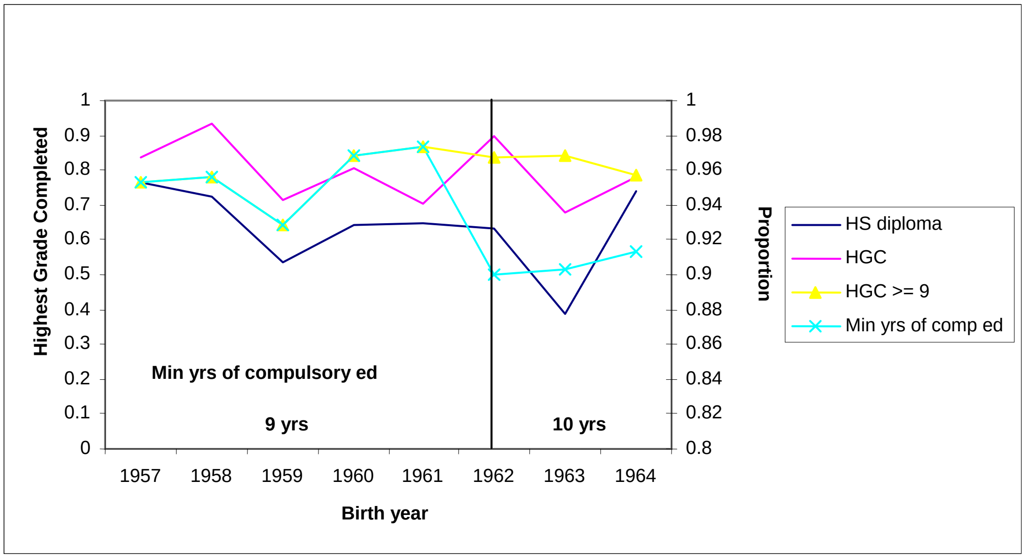

Figure 1.5. Changes in the Average Years of Mother's Schooling by Birth Year for New York

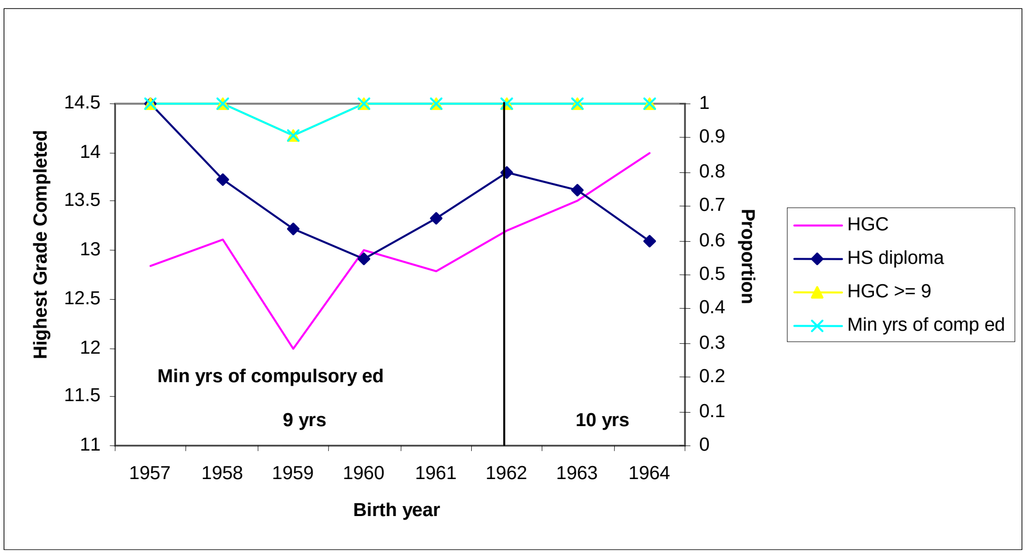

Figure 1.6. Changes in the Average Years of Mother's Schooling by Birth Year for Virginia

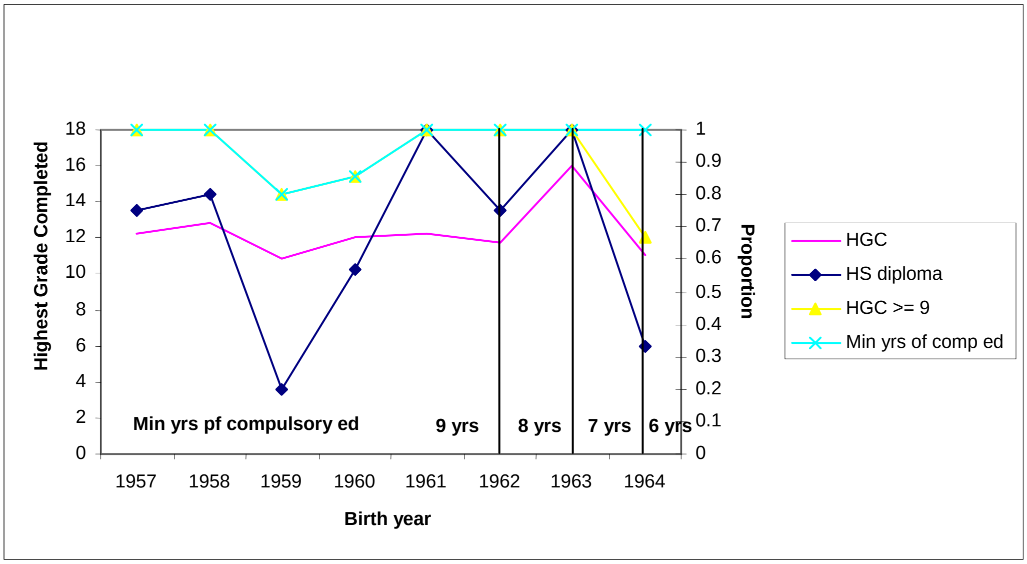

Figure 1.7. Changes in the Average Years of Mother's Schooling by Birth Year for Mississippi

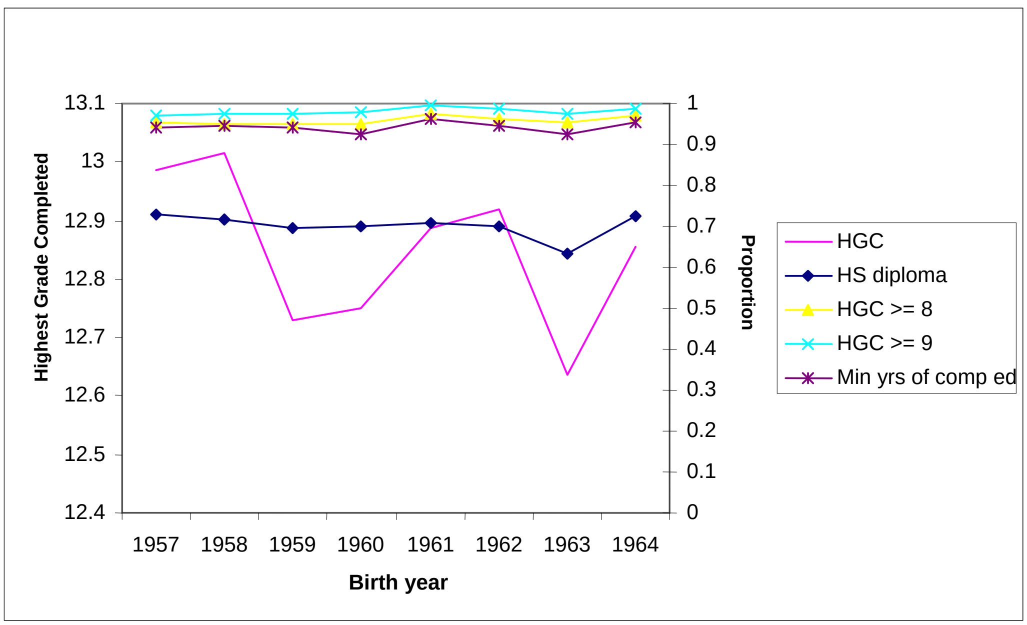

Figure 1.8. Changes in the Average Years of Mother's Schooling by Birth Year for All Other States

While the proportion of mother's 8th grade and 9th grade completion and minimum years of compulsory schooling remain stable throughout all birth cohorts, mother's highest grade completed changes quite drastically by birth cohort which suggest that this variable may not be the best one to use for mother's schooling. The effects of the policy changes are not clearly seen probably due to the small sample size for each cell (i.e. each state/before or after change for the tables or each state/birth year for the figures). This casts a clear doubt on the validity of the compulsory schooling policies as instruments for mother's educational attainment in this data set.

Table 1.10 conducts a similar comparison as Table 1.9 but for cutoff date changes. Only those non mover mothers who were born in the affected months are compared before and after the change and for those states that went through a cutoff date change only once in the sample period. Therefore descriptive analyses are not shown here for Kansas and Tennessee for the effect of entry cutoff date change and New Mexico, South Carolina, and Virginia for the effect of exit cutoff date change because these states went through multiple changes and the effect on affected mothers are most likely too complicated to see in simple descriptive statistics. All the states examined here for the effect of school entry cut off date change moved their dates back making those who were affected to enter school early. Therefore the mothers who were affected are expected to have more years of schooling than those with the birth months who were not affected (i.e. those who entered school before the policy change). As for the effect of school exit cut off date change, Mississippi and Kentucky were examined. Mississippi moved its cutoff date forward making those who were affected eligible to exit from school later whereas Kentucky moved its cutoff date back making those who were affected eligible to exit from school earlier. Therefore the mothers who were affected are expected to have more years of schooling in Mississippi and fewer years of schooling in Kentucky than those with the birth months who were not affected (i.e. those who exited from school before the policy change).

Table 1.10. Comparison of Highest Grade Completed, High School Diploma Receipt, 8th Grade completion, 9th Grade Completion and Minimum Years of Compulsory Schooling for All States and by Selected States for Mothers Affected and Not Affected by Compulsory School Entry or Exit Cutoff Date Changes

	IL (FIPS=17)		NM (FIPS=35)		OH (FIPS=39)		SD (FIPS=46)		VA (FIPS=51)	
	not affected	affected	not affected	affected	not affected	affected	not affected	affected	not affected	affected
HGC	12.363 (3.749)	12.737 (2.281)	13.222 (3.420)	12 (4.243)	12.929 (2.615)	13.156 (2.127)	12 (--)	16 (--)	13.125 (1.727)	13.667 (1.366)
HS dip	0.818 (0.405)	0.632 (0.496)	0.778 (0.441)	0.5 (0.707)	0.857 (0.363)	0.813 (0.397)	1 (--)	1 (--)	0.75 (0.463)	1 (0)
8th grade	0.909 (0.302)	0.947 (0.229)					1 (--)	1 (--)	1 (0)	1 (0)
9th grade			0.778 (0.441)	0.5 (0.707)						
Min yrs					0.857 (0.363)	0.938 (0.246)				

	MS (FIPS=28)		KY (FIPS=21)	
	not affected	affected	not affected	affected
HGC	10.333 (2.887)	12 (--)	14 (2.309)	12 (2)
HS dip	0.333 (0.577)	1 (--)	1 (0)	0.667 (0.577)
9th grade	0.667 (0.577)	1 (--)	1 (0)	0.667 (0.577)

Note: * significant at 10%; ** significant at 5%; *** significant at 1%

While most effects are in the expected direction except for New Mexico where all variables have the effect in the unexpected direction, all effects are statistically insignificant from zero. Since the sample sizes are even smaller for the cutoff date changes, more statistically insignificant effects are found for the cutoff date changes than for the school entry and exit age changes. No attempt is made here to show the change in mother's schooling by birth year to show the effect of the cutoff date changes because the sample size is too small to even detect an effect.

Taking all of these findings together, the number of mothers who were affected was too small to detect the expected effects of compulsory schooling law changes and therefore they provide a strong evidence against the validity of these policies as instruments.

One might argue that many of the effects of compulsory schooling policies are not in the expected direction because of the small number of mothers that were affected. To find out whether these trends are unique to the sample mothers in this study, the analyses were extended for all NLSY79 respondents (12686 people) and for all female NLSY79 respondents (6283 people). Since these samples include people who are not mothers of the children in NLSY79CY, not everyone has the mother's HGC variable from NLSY79CY.

Therefore for this exercise, HGC Revised and high school diploma receipt variables from NLSY79 were used for all non movers as well as for non movers excluding those who indicated having a GED since HGC Revised variable adjusts for GED receipts. Similar to the sample of mothers, only around 10% of the people were affected by the policy changes. Overall results were also very similar to the sample mothers. Therefore the results in the unexpected direction found for the sample mothers are not likely to be because of the small sample size.

Since the compulsory schooling policies do not seem to be binding educational attainment of all sample mothers, the sample mothers were restricted to only those mothers who are high school dropouts. As expected, the effects of policy changes are stronger and more consistent with expectations, but the number of affected mothers became even smaller and will therefore not have enough variation for identification. Despite the low compliance rate for compulsory schooling policies and inconsistent effects of the policy changes on mother's highest grade completed and high school diploma receipt, the effect of the policy changes on mother's schooling were examined using simple OLS regressions. This was done by state and with and without the basic control variable, race. First, the results from the previous descriptive statistics were replicated using regression approach including only an indicator variable for whether the individual was affected by the policy change. Next the ages of compulsory school entry or permitted school exit were added instead of a policy change indicator variable. One would expect to find a negative coefficient on the age of compulsory school entry and a positive coefficient on the age of permitted school exit if the policies had a binding effect on grade attainment.

Tables 1.11 to 1.15 show the results from the simple regression of mother's grade attainment on compulsory schooling policies by state. Each column represents a regression and each row represents an independent variable included in the regression. First, columns (1) to (6) do not include any controls and the columns (7) to (12) include race as a control. Columns (1) to (3) of Tables 1.11 to 1.15 show the results from the regression using an indicator variable for whether the individual was affected by the policy change and as expected, the results mirror exactly those found earlier. Columns (4) to (6) of Tables 1.11 to 1.15 show the results from the regressions with the age of compulsory school entry and similar to the findings from the regressions with an indicator variable, most effects are statistically insignificant.

The only statistically significant effects are found for California and New York where the effect is in the expected direction for California and in the unexpected direction for New York, both consistent with the results found in the descriptive statistics. After including race, with the exception of California where the effect remains in the same direction in the same magnitude, all effects disappear for both the indicator variable and the age of compulsory school entry. These OLS regressions reinforces the findings from the earlier descriptive tables that compulsory schooling policies are not good predictors of mother's schooling, one more piece of evidence against the use of compulsory schooling laws as instruments.

In summary, this section builds up an argument for why the compulsory schooling policy variables are not good instruments for the grade attainment of the mothers in NLSY79. To summarize, the reasons are as follows:

1. Very few mothers were affected by the laws. Only 11% of the sample mothers were non movers and affected by the cut off age policy change and only 2% of the sample mothers were non movers and affected by the cut off date policy change.
2. Very few states changed the compulsory schooling laws during the sample period. Only five states (four states for school entry and one state for school exit) changed compulsory schooling ages during the study period that affected non mover mothers. Only 12 states (seven states for school entry and five states for school exit) changed compulsory schooling cut off dates during the study period that affected non mover mothers.
3. The rate of compliance for the compulsory schooling law was low at about 94%.

Table 1.11. Effect of Compulsory Schooling Policy on Mother's 8th Grade Completion, High School Diploma Receipt and Highest Grade Completed for California

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	HGC>=8	HS dip	HGC	HGC>=8	HS dip	HGC	HGC>=8	HS dip	HGC	HGC>=8	HS dip	HGC
Affected	0.065***	0.004	0.199				0.061***	-0.003	0.142			
	(0.023)	(0.051)	(0.317)				(0.022)	(0.051)	(0.313)			
Entry age				-0.036***	0.034	-0.071				-0.034***	0.037	-0.043
				(0.013)	(0.029)	(0.179)				(0.013)	(0.029)	(0.175)
With Race	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	414	414	414	414	414	414	414	414	414	414	414	414
R-squared	0.02	0	0.001	0.019	0.003	0	0.054	0.022	0.043	0.054	0.026	0.042

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.12. Effect of Compulsory Schooling Policy on Mother's 9th Grade Completion, High School Diploma Receipt and Highest Grade Completed for New Jersey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC
Affected	0.005	0.026	-0.891*				0.000	0.049	-0.648			
	(0.031)	(0.085)	(0.458)				(0.000)	(0.084)	(0.455)			
Entry age				-0.036	0.141	0.574				0.000	0.120	0.307
				(0.040)	(0.108)	(0.595)				(0.000)	(0.107)	(0.585)
With Race	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	122	121	122	122	121	122	122	121	122	122	121	122
R-squared	0.000	0.001	0.031	0.007	0.014	0.008		0.057	0.093		0.064	0.080

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.13. Effect of Compulsory Schooling Policy on Mother's 9th Grade Completion, High School Diploma Receipt and Highest Grade Completed for New York

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC
Affected	0.008	-0.106*	-0.210				0.007	-0.089	-0.105			
	(0.026)	(0.063)	(0.361)				(0.016)	(0.063)	(0.356)			
Entry age				-0.015	0.090	0.626*				-0.015	0.067	0.530
				(0.024)	(0.059)	(0.334)				(0.015)	(0.059)	(0.332)
With Race	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	268	267	268	268	267	268	268	267	268	268	267	268
R-squared	0.000	0.011	0.001	0.001	0.009	0.013	0.021	0.039	0.047	0.023	0.037	0.055

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.14. Effect of Compulsory Schooling Policy on Mother's 9th Grade Completion, High School Diploma Receipt and Highest Grade Completed for Virginia

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC
Affected	0.021	0.032	0.792				0.022	0.044	0.834*			
	(0.025)	(0.105)	(0.481)				(0.025)	(0.102)	(0.472)			
Entry age				-0.024	-0.011	-0.599				-0.027	-0.034	-0.686
				(0.023)	(0.096)	(0.442)				(0.023)	(0.094)	(0.434)
With Race	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	80	79	80	80	79	80	80	79	80	80	79	80
R-squared	0.009	0.001	0.034	0.014	0.000	0.023	0.020	0.065	0.083	0.027	0.064	0.076

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.15. Effect of Compulsory Schooling Policy on Mother's 9th Grade Completion, High School Diploma Receipt and Highest Grade Completed for Mississippi

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC	HGC>=9	HS dip	HGC
Affected	-0.105	-0.108	-0.273				-0.005	-0.103	-0.013			
	(0.113)	(0.178)	(0.922)				(0.103)	(0.183)	(0.889)			
Exit age				0.010	-0.040	0.027				-0.014	-0.043	-0.055
				(0.036)	(0.056)	(0.292)				(0.032)	(0.058)	(0.281)
With Race	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	37	37	37	37	37	37	37	37	37	37	37	37
R-squared	0.024	0.010	0.002	0.002	0.014	0.000	0.012	0.012	0.118	0.018	0.019	0.119

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

4. The direction of the effect of compulsory school entry age laws on mother's grade attainment may be more complicated than simply the number of years in school due to the complicated ways in which these laws may have affected mother's schooling.
5. Compulsory schooling laws most likely affect highly selected group of children.

Consequently, there were no evidence of the effect of compulsory schooling policies on mother's schooling. This is consistent with the finding by Bedard and Dhuey (2007) who also found no effect of compulsory schooling policies in the U. S. between 1964 and 1985 for the sample of white males regardless of the level of education.[9] Therefore the proposed identification strategy outlined in the previous section will not work for the sample of this study.

1.7. OLS Results

Since the compulsory schooling policies were not good instruments for mother's education, examining causal effects of mother's schooling on child health using the proposed identification strategy cannot be done. Therefore this section examines the association of mother's schooling and child's health and health care use using OLS to describe the correlation that exists and to consider other possible ways to tease out causal effects. Specific focus is placed on finding the level of schooling that is associated with the greatest improvement in children's health. As already pointed out

[9] While the validity of compulsory schooling laws as instruments is questionable in the U.S. context, there is no evidence so far against the use of these instruments for other countries (Bedard and Dhuey 2007). Oreopolous (2006) also estimated the effect of compulsory schooling policies on educational attainment but the study was performed to examine the compulsory schooling policies in Canada between 1920 and 1990 for all Canadian individuals born around the time period. He found that the compulsory schooling policies significantly affected grade attainment. However, the direction and the magnitude of the effect of each compulsory education policy were not consistent. Not withstanding, he went on to estimate the effect of educational attainment on wages using these compulsory schooling policies as instruments mentioning that regardless of the varying signs and magnitude of each policy, taken all together, the policies were good predictors of the educational attainment.

in the earlier sections, there is a vast literature documenting the positive relationship between mother's schooling and children's health (e.g. Grossman 1976, 2006). However, not much is known about how the magnitude of this relationship varies with the level of schooling, especially in the U.S. context. The economic theory of diminishing marginal returns implies that the returns to mother's schooling is greater for lower levels of schooling and the returns decrease as mothers get more schooling. The optimal level of schooling (that has the greatest association with the improvements in children's health) is not very well understood empirically.

Instead of instrumenting mother's schooling with the compulsory schooling policies as proposed in section 4, here, mother's schooling was directly included in the OLS equation disregarding the problem of endogeneity. Instead of clustering the standard errors at the mother's state of residence when she was affected by school exit laws as proposed before for the IV regressions, in these OLS regressions, they are clustered at the state in which children's health measures were taken. Table 1.16 and 1.17 show the results from OLS regressions.[10] Table 1.16 shows results from regressions using mother's highest grade completed as a key independent variable. Table 1.17 shows results from regressions that include indicator variables for different levels of schooling (i.e. an indicator variable for high school completion (including both diploma receipt and GED), an indicator variable for GED receipt, an indicator variable for some college or more, an indicator variable for college graduate and an indicator variable for beyond 4 years of college). The reference category is high school dropouts. Therefore, for example, the association between mother's more than 4 years of college schooling and children's health is given by adding the coefficients for high school completion, some college or more college graduate and beyond 4 years

[10] Here, I control for state fixed effects as I explained in section 4. However, since I would like to control as many unobserved variables as possible, I also try estimating the same regression with county fixed effects and clustering the standard errors by county. Since the results are similar qualitatively and in magnitude to those with state fixed effects, I only present the results using state fixed effects here.

of college). The coefficient on each level of schooling essentially captures the incremental increase (or decrease) in the association between mother's schooling and children's health. Panel A includes all control variables mentioned in section 4 but panel B excludes family size, mother's marital status and mother's age from the regression since they may be capturing the effect of mother's education.

Table 1.16 from both panels suggest that increase in mother's schooling is associated with greater use of prevalence care, greater weight and height for age but more illnesses and bone fractures in the past year that required medical attention. The magnitudes are small for all child measures. In panel A, one grade increase in mother's grade completion is associated with 0.6 percentage point decrease in the probability of a child having the last checkup 1 year ago or more, less than a percentile increase in children's weight and height for age, 1.5 percentage point increase in the probability of having at least one illness in the past year that required medical attention, an increase in the number of such illnesses in the past year by 0.04 percentage points and 0.1 percentage point increase in the probability of having bone fractures in the past year that required medical attention. There was no relationship for the prevalence of overweight children. These results are similar with or without the possible endogenous covariates of mother's education. This is similar to the findings from the simple descriptive tables earlier.

Table 1.17 provides insight as to how different levels of schooling are associated with children's health. Some college schooling seems to be associated with the greatest increase in children's weight and height-for-age. Schooling level beyond some college does not seem to have any statistically significant incremental effect on children's health.

Table 1.16. OLS Regression of Various Child's Health and Health Care Use Outcomes on Mother's Highest Grade Completed

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Checkup	Wt-for-age	Ht-for-age	BMI-for-age	At risk overwt	Overwt	Ever ill	# of illnesses	Bone fractures
A.									
HGC	-0.006***	0.519***	0.815***	0.094	-0.000	-0.003	0.015***	0.041***	0.001***
	(0.001)	(0.177)	(0.229)	(0.165)	(0.002)	(0.002)	(0.001)	(0.006)	(0.000)
Endog controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44460	22153	18084	17990	17990	17990	44707	44646	36847
R-squared	0.125	0.031	0.037	0.026	0.020	0.024	0.118	0.107	0.011
B.									
HGC	-0.008***	0.718***	0.849***	0.413**	0.003	-0.000	0.016***	0.041***	0.001***
	(0.001)	(0.164)	(0.216)	(0.163)	(0.002)	(0.002)	(0.002)	(0.006)	(0.000)
Endog controls	No	No	No	No	No	No	No	No	No
Observations	44524	22179	18108	18014	18014	18014	44771	44709	36902
R-squared	0.122	0.029	0.035	0.022	0.016	0.019	0.113	0.106	0.011

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: Each regression in panel A includes controls for child's sex, race, age, family size, mother's age and marital status, county level characteristics of the child's state of residence when the child health measure was taken (total active non-federal MDs, total patient care by non-fed MDs, total number of hospitals, total number of hospital admissions and total number of hospital beds). Each regression in panel B includes all controls in panel B except family size, mother's age and marital status. Standard errors are clustered at the state level (i.e. mother's state of residence when she was affected by school exit laws). All regressions include fixed effects for state of residence and year when child health measures are taken and child's age.

Table 1.17. OLS Regression of Various Child's Health and Health Care Use Outcomes on Mother's High School Diploma Receipt

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Checkup	Wt-for-age	Ht-for-age	BMI-for-age	At risk overwt	Overwt	Ever ill	# of illnesses	Bone fractures
A.									
HS comp	-0.001	2.252**	4.184***	0.581	0.004	-0.005	0.052***	0.106**	0.005**
	(0.009)	(1.105)	(0.940)	(1.075)	(0.015)	(0.007)	(0.009)	(0.041)	(0.002)
GED	-0.006	0.623	-2.174	1.040	0.013	0.013	-0.022**	0.013	-0.002
	(0.014)	(1.116)	(1.326)	(1.150)	(0.017)	(0.012)	(0.011)	(0.044)	(0.002)
Some college	-0.012	3.972***	3.884**	2.015	0.003	0.002	0.030***	0.042	0.005
	(0.012)	(1.130)	(1.489)	(1.290)	(0.018)	(0.012)	(0.011)	(0.055)	(0.003)
College graduate	-0.020	-1.843	-3.014	-1.030	0.004	0.019	0.026*	0.134	-0.006
	(0.015)	(2.166)	(2.810)	(1.622)	(0.025)	(0.020)	(0.015)	(0.083)	(0.005)
More than college	-0.001	-0.748	0.229	0.468	-0.005	-0.032*	0.011	0.025	0.003
	(0.012)	(1.909)	(2.022)	(1.734)	(0.024)	(0.019)	(0.013)	(0.064)	(0.003)
Endog controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	43508	21788	17799	17707	17707	17707	43755	43695	35982
R-squared	0.125	0.033	0.039	0.027	0.020	0.024	0.119	0.107	0.011
B.									
HS comp	-0.007	2.886**	4.411***	1.470	0.013	0.003	0.062***	0.127***	0.005**
	(0.009)	(1.113)	(0.929)	(1.092)	(0.015)	(0.008)	(0.010)	(0.041)	(0.002)
GED	-0.005	0.249	-2.168	0.338	0.005	0.006	-0.023**	0.012	-0.002
	(0.015)	(1.193)	(1.398)	(1.162)	(0.017)	(0.013)	(0.011)	(0.045)	(0.002)
Some college	-0.014	4.184***	3.776**	2.445*	0.008	0.006	0.031***	0.042	0.005
	(0.012)	(1.139)	(1.507)	(1.309)	(0.018)	(0.013)	(0.011)	(0.054)	(0.004)
College graduate	-0.021	-1.612	-3.112	-0.455	0.008	0.022	0.024	0.127	-0.006
	(0.015)	(2.120)	(2.723)	(1.635)	(0.025)	(0.020)	(0.016)	(0.083)	(0.005)
More than college	-0.005	-0.433	0.225	0.933	0.001	-0.027	0.006	0.015	0.003
	(0.012)	(1.921)	(2.038)	(1.727)	(0.024)	(0.019)	(0.014)	(0.065)	(0.003)

Table 1.17. (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Checkup	Wt-for-age	Ht-for-age	BMI-for-age	At risk overwt	Overwt	Ever ill	# of illnesses	Bone fractures
Endog controls	No	No	No	No	No	No	No	No	No
Observations	43567	21812	17821	17729	17729	17729	43814	43753	36032
R-squared	0.122	0.031	0.037	0.024	0.017	0.019	0.114	0.106	0.011

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

See note from Table 12

For example, in panel A, compared to high school drop outs, high school completion (regardless of whether they have high school diploma or GEDs) is associated with approximately two percentile increase in children's weight-for-age and four percentile increase in height-for-age. Compared to high school drop outs, some college schooling is associated with approximately six percentile increase (2.252+3.972) and eight percentile increase (4.184+3.884) in children's weight and height-for-age. There is no statistically significant incremental effect for schooling levels beyond this level although there seems to be a small negative association with mother's schooling beyond college and prevalence of overweight among children (i.e. by about three percentage points).

As seen before, greater schooling is associated with more illness that required medical attention among children but this association is mainly for high school completion. The only exception is the probability of any illness in the past year that required medical attention where the association is greatest for some college (increase by eight percentage points (0.052+0.030) as compared to high school dropouts). GED receipt is actually associated with smaller increase in the probability of any illness in the past year that required medical attention (increase by three percentage points (0.052-0.022) as compared to high school dropouts) than high school diploma receipt (increase by 5 percentage points as compared to high school dropouts). As discussed in the earlier sections, these illness or bone injury measures may reflect the fact that mothers with more schooling are more likely to seek care due to her ability to more accurately evaluate the symptoms or due to greater access to care. It may also reflect the fact that these children are more active and thus are more likely to get ill or injured. The magnitude of the effects is small for all children's health measures in general and there were no statistically significant associations for preventive care use.

These OLS results most likely overestimate the true effect of mother's schooling on children's health. For example, children with mothers with more schooling are also more likely to come from higher income families, more likely to be insured and more likely to have mothers that value health. All of these will most likely affect children's health in the same direction and therefore without accounting for these covariates (and many more that are unobserved), the estimates on mother's schooling variables from OLS regressions will be positively biased. It is possible to control for some of the observable covariates such as income although, like schooling, they are endogenous to children's health. Nevertheless, some of the observed omitted variables are included in the OLS regressions, specifically, children's health insurance status, an indicator variable of whether the mother is working, mother's work hours and mother's wage income.[11] Magnitudes of the effects are similar for almost all measures with slightly smaller magnitudes for weight and height-for-age measures.[12] This provides suggestive evidence that the true effect of mother's schooling on children's health has the same magnitude as the OLS estimates at most and quite likely is smaller due to positive bias of OLS estimates explained earlier. Considering the fact that the OLS estimates are very small in magnitude to begin with, the true effect of mother's schooling on children's health is most likely negligible.

Moreover, it is not clear whether these results are good or bad. Increase in the percentiles for weight and height-for-age found here is difficult to evaluate since they are mainly used to determine acute or chronic under-nutrition for low weight and height-for-age children which is rare in the U.S (WHO 1995). The problem that is more prevalent among American children is obesity which these measures are not very good at identifying (Stettler, Zomorrodi and Posner 2007). In fact, the findings do not

[11] Child's health insurance status, mother's labor force participation, work hours and income may be endogenous because they all capture unobserved mother's characteristics such as her commitment to her child's good health and health awareness.
[12] These results are not shown in this paper but are available upon request.

suggest any association with mother's schooling and prevalence of overweight among children other than a small decrease among children with mothers who have schooling beyond college education. The illness and injury measures are also difficult to interpret since these measures reflect many things including morbidity, access to care, the ability of the mothers to accurately diagnose the symptoms of illness or injury, etc. More objective measures are necessary to understand the true association between mother's schooling and the prevalence of illnesses and injuries among children.

1.8. Discussion

This study elicited the problems of using compulsory schooling laws as instruments for grade attainment of the mothers in the NLSY79 cohort. During the time period which the NLSY79 cohort were affected by the compulsory schooling policies, there were not many changes in the policies to provide enough variation for identification. The time period in which NLSY79 respondents were affected by school entry or exit policies was very short (less than 10 years) and within that time period, there were only a few policy changes. Previous studies that used compulsory schooling laws as instruments for schooling were probably more successful because they used a longer time period that had more policy changes that were binding. For example, Acemoglu and Angrist (2000) used compulsory schooling laws and child labor laws from 1920-1960. Lleras-Muney (2005) used compulsory schooling laws and child labor laws from 1915-1939. Lochner and Moretti (2004) used compulsory schooling laws from 1914-1974. McCrary and Royer (2006) used compulsory entry laws from approximately 1973-1993.[13] These studies used earlier years (1915-1940) or more

[13] Unlike other studies that used IV methods, McCrary and Royer (2006) used regression discontinuity to examine the effect of mother's schooling on infant health. They found that schooling discontinuity seen around school entry cut off date was strongest for younger mothers and weakest for older mothers which reinforces the argument here that compulsory schooling laws were less effective in 1963-1972 (time period for which NLSY79 respondents were affected by school entry laws) and more effective in recent years (after 1975).

recent years (after 1975) which were the time periods where the policies were most effective, binding and had most variation (Lleras-Muney 2005, Bedard and Dhuey 2007). Earlier years are more likely to be a better time frame to use these laws as instruments. In general, people had fewer years of schooling than now. This is because there was less demand for skilled or educated workers in the work force then than it is today, and the gains from an extra year of schooling were not as great as it is today (Oreopolous 2008). There were no incentives for people to get schooling beyond compulsory years. Therefore the laws were probably more binding during earlier years compared to more recent years. The years 1963-1972, the time period for which NLSY79 respondents were affected by school entry laws, were the years when the compulsory schooling laws saw little change, falling between the two time periods that experienced compulsory schooling law changes. Therefore despite the fact that CoSLAW data are more detailed than the previous studies, this study is not able to take advantages of it because the time period used for this study does not have many policy changes that are necessary for the identification to work.

However, as discussed in this paper, even if the compulsory schooling policies have relevant variations and are binding, their effect on grade attainment is complicated because they also affect academic achievement and grade retention that may have their own independent effect on the outcome. Therefore if the compulsory schooling policies are used as instruments for grade attainment, not only would the IV estimates be difficult to interpret due to the ambiguity of the effect of the policies on grade attainment, but they will also violate one of the conditions for a valid instrument. None of the IV studies that use compulsory schooling policies as instruments for grade attainment are immune from these problems although to the author's knowledge, these problems are not discussed in any of the studies.[14]

[14] Bedard and Dhuey (2007) provide similar discussion about the potential problems of compulsory schooling laws as instruments although they do not use IV methods in their study. In a separate study

One possibility of using this sample of mothers in NLSY79 to examine the relationship between mother's schooling and children's health care use and health status is to use different sets of instruments for mother's schooling. Some instruments that have been proven to work for this time period are graduation requirements, GED policies and per capita education spending (Kenkel, Lillard and Mathios 2006). They used the same NLSY79 cohort to examine the effect of schooling (i.e. high school completion and GED receipt) on own health behavior. Their instruments had more variations than the compulsory schooling policies during the relevant time period and their first stage confirmed the validity of the instruments. If these instruments are used for mother's schooling instead of own schooling, the study will provide an interesting extension to Kenkel, Lillard and Mathios' (2006) study by exploring how schooling affects the health of the next generation. However, similar to the compulsory schooling policies, the graduation requirement and GED policies may also change other aspects of schooling that may affect health outcomes independently from grade attainment. If this is the case, the use of these policies may also be problematic as instruments for grade attainment. Instead, these instruments (i.e. graduation requirements, GED policies and per capita education spending) may be suited to capture the quality (instead of quantity) of schooling since it is likely that the content of high school education may have been affected by these policies. For example, graduation requirements, GED policies and per capita education spending may be used as instruments for the type of high school curriculum that the mother experienced to find out whether the quality of mother's schooling matters for children's health care

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(Bedard and Dhuey 2006), they use relative age constructed from compulsory schooling entry laws and birth date to instrument for observed age to examine the effect of observed age on various schooling outcomes including college enrollment and in that study, they do not mention about these issues related to the use of compulsory schooling laws as instruments.

use and health status outcomes.[15] Since most literature on the returns to schooling focus on the quantity of schooling, this will provide an important extension to the existing literature.

Other methods may also be considered to examine the effect of mother's schooling on children's health care use and health status. One such method is regression discontinuity, where children who are born right before and after the school entry cutoff date are compared (e.g. McCrary and Royer 2006). Since this method does not depend on the changes in compulsory schooling policies, it should work for this time period as well. Since NLSY79 is a nationally representative sample, the study will build on McCrary and Royer' (2006) study by providing new evidence of mother's schooling on children's health care use and health status for all states. However, if being born right before or after the cutoff date affects children's health through causal pathways other than grade attainment, then one must be cautious about using this method as well.

1.9. Conclusions

Using 1979-2002 NLSY79 and 1986-2002 NLSY79CY, this study documented that the compulsory schooling policies are unsatisfactory instruments for mother's schooling to examine the causal effect of mother's schooling on child's health and health care use. There were only a few changes in compulsory schooling policies during the time period the sample mothers were affected and therefore only a small proportion of the sample mothers were affected by the policy changes. The compliance rate of compulsory schooling policies was also low. Perhaps due to the complex ways in which the compulsory schooling policies affect academic achievement and performance, they were not good predictors of mother's grade

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[15] NLSY79 has information on type of high school curriculum, i.e. vocational, commercial, college preparatory or general program.

completion and high school diploma receipt.

Since causational analyses were not possible using the sample mothers of this study, simple OLS analyses were conducted to examine the association between mother's schooling and children's health and health care use. Findings suggest that mother's schooling is associated with greater weight and height-for-age but also with more illnesses and bone injuries that require medical attention. Some college schooling is associated with the greatest increases in weight and height-for-age whereas high school completion is associated with the greatest increases in illnesses and bone injuries that require medical attention. Since the magnitudes of the associations are very small, the causal effects are probably smaller or even close to zero since the OLS estimates are most likely positively biased.

Despite the fact that CoSLAW data would have allowed the study to take advantage of the fullest variation in the compulsory schooling policies, these policies turned out to be not very good instruments for mother's schooling because of the time frame of the study. While examining the causal effect of mother's schooling on child's health and health care use was not possible, this study offers future researchers some cautionary tales on the use of compulsory schooling policies as instruments for grade attainment in general. First, compulsory schooling policies affect only a subset of people who would drop out of school if the law does not bind them from leaving school. Previous studies suggest that this is a highly selected group and therefore the policies do not necessarily serve as the best predictors for educational attainment for the average child. Second, because of the complex ways in which compulsory school policies impact academic achievement or performance, compulsory schooling laws may be an invalid instrument. Third, this study also found that the compliance rate for compulsory schooling laws is quite low. Since there is no reason to believe that the low compliance rate seen during the time period of interest is unique to this cohort, it calls for a careful examination into compliance issue even if the laws are valid instruments.

APPENDIX

1.10. Algorithms Used to Merge in Compulsory Schooling Policies

To estimate the IV model, instruments (compulsory schooling policies on school entry and school leaving) were merged with the mother's record by mother's state of residence in the relevant years. The instruments were used in the first stage regression on mother's own education. The way mother's data were merged with each type of instruments is explained below.

Table A.1. Compulsory Schooling Laws in Hypothetical States X and Y

		State X		State Y	
year	age	age of comp entry	age of perm leaving	age of comp entry	age of perm leaving
1960	0	5	14	7	14
1961	1	5	14	7	14
1962	2	5	14	7	14
1963	3	5	14	7	14
1964	4	5	14	7	14
1965	5	6	14	7	14
1966	6	**6**	14	7	14
1967	7	6	14	**7**	14
1968	8	6	14	8	14
1969	9	6	14	8	14
1970	10	6	14	8	14
1971	11	6	14	8	14
1972	12	6	14	8	14
1973	13	6	14	8	14
1974	14	6	15	8	**14**
1975	15	6	**15**	8	16
1976	16	6	15	8	16
1977	17	6	15	8	16
1978	18	6	15	8	16

1. **Compulsory Schooling Policies: School Entry**

Age of compulsory entry was assigned to each mother using her state of birth and the year she was required to enter school in her state of birth. Consider mother A who was born in state X in 1960. She became age 4 in 1964, age 5 in 1965, age 6 in 1966, age 7 in 1967, age 8 in 1968, age 9 in 1969, and age 10 in 1970. The age of compulsory entry in state X in 1964 was 5, in 1965 was 6, in 1966 was 6, in 1967 was 6, in 1968 was 6, in 1969 was 6, and in 1970 was 6. Therefore she was required to enter school in 1966. If the same mother A were born in state Y, she will be required to enter school in 1967.

2. **Compulsory Schooling Policies: School Leaving**
Age of permitted school leaving were assigned to each mother using her state of birth and the year she became eligible to drop out from school in her state of birth. Again consider mother A who was born in state X in 1960. She became age 12 in 1972, age 13 in 1973, age 14 in 1974, age 15 in 1975, age 16 in 1976, age 17 in 1977, and age 18 in 1978. The age of permitted school leaving in state X in 1972 was 14, in 1973 was 14, in 1974 was 15, in 1975 was 15, in 1976 was 15, in 1977 was 15, and in 1978 was 15. Therefore she became eligible to drop out from school in 1975. If the same mother A were born in state Y, she will be eligible to drop out from school in 1974. Note that although state Y increased the age of permitted school leaving to age 16 in 1975 when she is still age 15, she does not need to go back to school since she has already reached age 14 when age 14 was the age of permitted school leaving in state Y.

1.11. Schooling Variables in NLSY79 and NLSY9CY

This study thoroughly examines the schooling variables in two of the most commonly used datasets, NLSY79 and NLSY79CY and finds that there were considerable measurement and reporting errors in the schooling variables. It is possible to check the consistency of the schooling variables because the reports on schooling for a person are longitudinal and there are several schooling variables available in NLSY79 and NLSY79CY data that are independently reported. The most prominent error was grade reversals where the respondent reports having a lower grade completed in the later years. Future researchers who intend to use the schooling variables in NLSY79 and in NLSY79CY should use caution.

There are several schooling variables available in NLSY79 and NLSY79CY that can potentially be used for this study. Here are the ones that were initially considered:

1. Highest Grade Completed (HGC) by Mother as of the Interview from the NLSY79CY
2. Highest Grade Completed (HGC) as of May 1st Survey Year from the NLSY79
3. Highest Grade Completed (HGC) as of May 1st Survey Year (REVISED) from the NLSY79
4. Do you have a high school diploma or have you ever passed a high school equivalency or GED test? Yes or No? from the NLSY79
5. Which do you have, a high school diploma, a GED or both? from the NLSY79

After examining these data, 24% of the mothers had at least one discrepancy between mother's HGC from the NLSY79CY and HGC Revised from the NLSY79 during their educational histories. This prompted the study to investigate the issue further and it found out some of the complexities in the schooling variables in the datasets.

First, before going into the discrepancies among the schooling variables, the study found that there were some missing observations in the schooling variables in the mother-year level data (i.e. one observation per mother per year) that can be imputed with very high accuracy using a simple logic. While these imputations ended up affecting only 3% of the mother-year sample data, imputation was done anyway because a small sample size increase at the mother-year level will translate into an increase of at least as much (if these mothers have more than one child or a child that appear in the data for multiple years) in the final sample at the child-year level. The logic that was used to impute some of the missing values is as follows:

1. If a mother reported that she completed high school or its equivalent in one year, for all the subsequent years, the missing values will be replaced by 'completed high school or its equivalent'. This also applies to high school diploma and GED.
2. Conversely, if a mother reported that she has not completed high school or its equivalent in one year, for all the preceding years, the missing values will be replaced by 'not completed high school or its equivalent'. This also applies to high school diploma and GED.
3. If a mother reports having both a GED and a high school diploma in the same year, she was categorized in a high school diploma category and not in a GED category.
4. If a mother reports having either a GED or a high school diploma but missing in the other in the same year, it is assumed that she only has the one that she reported and does not have the one with missing value.

If a mother's HGC has the same value in both the year preceding and succeeding the year with the missing HGC in the middle years, then the missing years will be filled in with the value that precedes or succeeds that missing year (which should be the same value). This logical refinement of the data did not create many noticeable changes in the means or the variances. For example, before the logical adjustment, mean mother's HGC from NLSY79CY is 12.487 with the standard deviation of 2.446 whereas after logical adjustment, it is 12.471 with the standard deviation of 2.448. The largest change was the probability of GED and high school diploma receipts, where the values from the NLSY79 were 0.095 with the standard deviation of 0.294 and 0.659 with the standard deviation of 0.474 before the logical adjustment but were 0.103 with the standard deviation of 0.303 and 0.693 with the standard deviation of 0.461 after the adjustment.

Second, there were many discrepancies among the schooling variables for each mother in NLSY79 and NLSY79CY. The Bureau of Labor Statistics was contacted and had revealed out that while some inconsistencies originate from the definition of the variables, others are clearly reporting errors (McClaskie 2008). Mother's HGC from NLSY79CY actually comes from a question in NLSY79: "What is the highest grade or year of regular school that you have completed and gotten credit for?" (i.e. Q3-4) which is also the source for HGC Original in NLSY79. This question is asked only for those people who answer 'yes' in the leading question: "At any time since (date of the interview), have you attended or been enrolled in regular school—that is, in an elementary school, a middle school, a high school, a college, or a graduate school?" Therefore mother's HGC from NLSY79CY (e.g. C00611.26 in 2000) should be the same as the HGC Original from NLSY79 (e.g. R70071.00 in 2000) but should be different from HGC Revised from NLSY79 (e.g. R70073.00 in 2000) since NLSY79 revises HGC Original using an algorithm to create NHC Revised variable. The main differences between mother's HGC in NLSY79CY and HGC Revised in NLSY79 are: mother's HGC in NLSY79CY is the highest grade completed as of survey date and there is no allowance for a GED, high school diploma, or grade reversal problems whereas HGC Revised in NLSY79 is the highest grade completed by the respondent as of May 1 survey year and there is a correction for a GED, high school diploma and grade reversal problems (McClaskie 2008). More specifically, HGC Revised from NLSY79 follows the following decision rules (NLSY79 2008, McClaskie 2008):

When there is a grade reversal (i.e. HGC reported in the later year is less than those reported in the earlier year) then HGC was replaced by the highest grade completed previously reported by the respondent:

1. When the respondent reports having no high school diploma or GED but some college attendance (i.e. grade 13 or higher), then HGC was recoded as 12;
2. When the respondent reports having no high school diploma or GED but some college attendance, then HGC was recoded as 12;
3. When the respondent reports having high school diploma or GED but no college attendance, then HGC was recoded as 12;
4. In cases where a four-year college degree had obviously been earned in 5 or more years, then HGC was recoded as 16;
5. When the respondent reports "ungraded" when asked for highest grade completed, then HGC was replaced by the highest grade completed previously reported by the respondent;
6. HGC values were revised to reflect the respondent's status on May 1st of survey year date;
7. When the HGC histories are highly erratic, then HGC was assigned "invalid missing" (Steve McClaskie: mcclaski@chrr.osu.edu)

In this study, the mother's HGC from NLSY79CY is the most relevant measure for mother's schooling since it does not adjust for GEDs.[16] Previous studies have found that GED recipients are more similar to high school drop outs than high school graduates in all dimensions including economic and social outcomes (Heckman and LaFontaine 2006, 2007, 2008, Heckman and Rubinstein 2001, Cameron and

[16] 3% of the mothers had discrepancies between HGC Original from NLSY79 and the mother's HGC from NLSY79CY. Recall that the information in mother's HGC from NLSY79CY comes from the same question as that for HGC Original from NLSY79 which means that both variables should have the same values. An attempt was made to exclude these mothers from the sample but since the results were very similar, they are kept in the final sample to maintain the sample size.

Heckman 1993). Therefore combining GED recipients with those who completed standard high school will be problematic.

In addition to mother's grade completion, mother's high school completion is also of great interest to this study. Compulsory schooling policies will most likely affect those mothers who would otherwise dropout of school if legally not bound. This means that these policies will most likely affect the mother's enrollment decisions for high school rather than any other levels of schooling such as college. For mother's high school completion, high school diploma variable from NLSY79 was used. It was assumed that the mother has high school diploma if she responds that she has a high school diploma or both high school diploma and a GED. This variable is optimal because it eliminates the possibility of including GED recipients in the high school completion category because the question specifically asks whether the respondent has a high school diploma, a GED or both. It is highly unlikely for a respondent to answer that she has a high school diploma when she only has a GED or vice versa. Nevertheless an attempt was made to use another method to impute mother's high school completion. A mother was defined as having completed high school if she reported that her HGC is 12 or more. A mother was assumed to be a GED recipient if one of the following was true:

1. mother's HGC from NLSY79CY is less than or equal to 10 for at least two consecutive years followed by a value that is greater or equal to 12 (e.g. 10, 10, 12, 13)
2. mother's HGC from NLSY79CY is equal to 11 for at least two consecutive years when the mother was at least age 19 in the first of the two consecutive years followed by a value greater or equal to 12 (e.g. 10, 10, 12, 13)

Table A.2. Comparison of High School Variables Imputed from Mother's Highest Grade Completed from NLSY79CY and from High School Variables Taken from NLSY79 for Years 1986-2002

Mom/year observation data	Mother's HGC from NLSY79CY	HS variables from NLSY79
HS completion or its equivalent	.764 (.424)	.842 (.365)
GED	.069 (.254)	.103 (.303)
HS diploma	.710 (.454)	.693 (.461)
N	30557	30252, 30227 [a]

Note: [a] Sample size for HS completion or its equivalent is 30252 and for GED and HS diploma is 30227.

A mother was defined as having a high school diploma only if she reported that her HGC is 12 or more but did not have a GED as defined here. GED recipients were treated as high school dropouts as suggested by previous studies. Table A.2 shows the comparison between the probabilities of mother's high school completion, GED receipt and high school diploma receipt calculated from mother's HGC from the NLSY79CY and reported high school information (i.e. completion, GED, and diploma) from the NLSY79. The observation is at mother-year level (i.e. one observation per mother per year) and they are from years 1986 to 2002 which is the period years for the final sample of this study. As clear from the table, there is a large discrepancy between these two methods of defining high school completion. The probabilities of high school completion and GED receipt are lower but the probability of high school diploma is higher when calculated from mother's HGC from the NLSY79CY. When GED receipts are examined closely, out of 4524 mothers, 143 mothers (3% of all mothers) had discrepancies for all the years (1979-2002) between GED receipt imputed from mother's HGC from the NLSY79CY and the reported GED receipt from the NLSY79 and 661 mothers (15% of all mothers) had discrepancies in at least one year. Considering the fact that high school variables from mother's HGC from the NLSY79CY are imputed whereas those from NLSY79 are reported by the mothers themselves, the variables from the latter dataset is more likely to be reliable than the imputed variables from the former dataset. Therefore this study uses high school diploma receipt from the NLSY79 for mother's high school completion. One point that must be taken away from this is that mother's HGC from the NLSY79CY must be used with caution when determining her high school schooling completion status. These discrepancies provide strong evidence that using mother's HGC from the NLSY79CY to impute her high school completion may be very misleading if her own report about high school education in NLSY79 is correct.

Third, another important finding of this study is that there are many mothers with inconsistent longitudinal schooling trajectories in mother's HGC from the NLSY79CY. Some mothers report a lower HGC in the later years after reporting a higher HGC in the earlier year and this logically does not make sense. In all, 13% of mothers have this discrepancy in their schooling histories. This problem is also recognized by NLSY79 and the appendix in their codebook supplement makes a note of this (NLSY79 2008). As explained earlier, their HGC Revised from NLSY79 accounts for these grade reversals.

Table A.3. Number of Mothers Affected by Grade Reversals in Mother's Highest Grade Completed Variable from NLSY79CY

	# of Mothers	
# of Grade Reversals	All mothers	Only those who are in the sample
1	507	481
2	109	107
3	14	14
4	1	1
5	1	1
Total	632	604

Table A.3 provides the number of mothers affected by grade reversals in mother's HGC from NLSY79CY. The majority of mothers who are affected by this problem have only one grade reversal in their schooling histories. Considering the fact that NLSY79 revises the HGC Original from NLSY79 to create HGC Revised in cases of grade reversals by replacing the affected value by the highest grade completed previously reported by the respondent and the fact that most affected mothers are affected by grade reversal only once in their grade completion histories, the most reasonable way to adjust for the grade reversal is to follow the NLSY79 method. Nevertheless, previous research was examined to find out how the studies have dealt with this problem. Although there were many studies that used HGC variables from NLSY79 or NLSY79CY,[17] there was only one study that did mention in passing the problem of grade reversal, but not how the authors dealt with the issue (Keane and Wolpin 2000).[18] Therefore in this study, it was assumed that if a mother reports having lower HGC in the later years in NLSY79CY, the value was reassigned with the highest grade completed previously as reported by the mother. The mean and variance did not change much due to this adjustment. Before adjusting for grade reversal problem, the mean of mother's HGC was 12.804 with the standard deviation of 2.603 but after the adjustment, the new mean is 12.844 with the standard deviation of 2.608. In sum, researchers must keep in mind that the measurement errors of mother's HGC in NLSY79CY are very large and may bias the research findings one way or the other.

1.12. CoSLAW Database

When these school entry age and cutoff dates in this dataset are compared with data from previous studies including Angrist and Krueger (1992) and Bedard and Dhuey (2007), there are quite a number of discrepancies. For example, in the period of 1964-1972 which coincides with the time period the sample mothers were affected by policies governing school entry, there were 29 states where the cutoff dates differed between the CoSLAW dataset and the dataset used by Bedard and Dhuey (2007) of which 16 states did not experience policy changes in either dataset. Since the main interest in the study is to examine schooling experiences within each state (by running separate analyses for each state or by including state fixed effects when all states are pooled together), these 16 states that did not change their policies in either dataset are not of concern. The more concerning issues are the policy changes that take place during the time period. In the CoSLAW dataset, there are 14 entry cutoff date changes

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[17] There are ten studies that have 'highest grade completed' in the abstract that uses NLSY79 and/or NLSY79CY according to the NLS Annotated Bibliography online database (2008). Although not all the studies may use the HGC, there are 352 studies on education attainment using NLSY79 (NLS Annotated Bibliography online database 2008).

[18] Kean and Wolpin (2000) mention that 20% of their observations had inconsistent longitudinal enrollment and highest grade completed data. They note that they carefully scrutinized all observations with inconsistent data and reconstructed a reasonable grade completion history using information available in the NLSY including highest grade attended and highest grade completed. They do not go in detail how they did the 'reconstruction'. Dr. Wolpin was contacted on March 28, 2008, but the author has received no response from him as of December 15, 2008.

in seven states while there are five entry cutoff date changes in four states in the dataset from Bedard and Dhuey (2007). Since the identification from the IV method comes from the policy changes including these cutoff date changes, if these cutoff date changes are incorrect, changes in mother's schooling will be wrongfully attributed to the policy changes and may fail to detect the true changes in mother's schooling due to policy changes. Both Lillard (2008) who is in charge of the CoSLAW dataset and Bedard and Dhuey (2007) claim that the source of the compulsory school entry policies are from state statutes and laws. Lillard (2008) is currently investigating the discrepancies in the datasets.

1.13. More on Identification

The use of compulsory school entry and exit ages as instruments for mother's schooling did not turn out to be a valid identification strategy to examine the causal effect of mother's schooling on children's health care use and health as discussed in the main text. This section explains the source of identification and intuition behind the proposed identification strategy. It examines whether the strategy worked and summarizes the reasons why it did not work in practice. The section also discusses how these issues would bias the estimates if they are used as instruments.

If compulsory schooling laws are effective and binding, it may affect mother's schooling decisions and ultimately affect her grade attainment or high school completion status. When compulsory school entry and exit ages are used as instruments for mother's schooling, the identification comes from states that changed either school entry and/or exit age or cutoff dates during the study period and from mothers who potentially changed grade attainment or high school completion status due to the policy changes. Since the regressions control for state and year fixed effects, the study compares schooling of mothers who got exposed to different schooling policies within each state and correlates these schooling differences with the intensity and timing of the policy changes that happened in each state.

Instrumental variables produce estimates of the effects of mother's schooling for mothers they are affecting, not for the average mothers. From the previous literature, it has been found that females, minorities, children with parents who have less than a college degree or from low income families are more compliant with compulsory schooling policies (Dobkin and Ferreira 2007, Elder and Lubotsky 2008). It has also been found that these policies mostly affect schooling decisions around high school (i.e. whether to complete high school or not) and not other educational margins such as primary schooling or college (McCrary and Royer 2006). Mothers whose schooling decisions depend on compulsory schooling laws are unlikely to be those who already decided to drop out from their primary school or those who go on to college; these laws affect those who precisely are in the margin of dropping out of high school. Therefore instrumental variable estimates using compulsory school entry and exit ages as instruments most likely produce estimates for mother's high school education among the disadvantaged population.

The direction of the effect of school entry age on mother's schooling is ambiguous although that of school exit age is predictable. As explained earlier, school entry law has an ambiguous effect on grade attainment due to its effect on academic performance and grade retention. Previous studies have found that increase in school entry age indeed increased ages of children entering school which allowed them to enter school when they were developmentally prepared for the rigors of schooling and thereby had positive impacts on academic performance and lead to reduction in grade repetitions (e.g. Bedard and Dhuey 2006). However, there is little agreement on how long these benefits last and whether it is long enough to affect grade attainment. In comparison, there are consistent findings for school exit age; increasing school exit age increased grade attainment and reduced drop out rates (e.g. Oreopolous 2005, 2008). Therefore in the first stage regression where mother's schooling is regressed on school entry and exit ages, while direction of the coefficient on school entry age is ambiguous, the coefficient on school exit age should be positive.

However, as explained in the main text, there were problems using compulsory schooling laws as instruments in the study period of this study including: 1) There were only a few changes in compulsory schooling policies during the time period the sample mothers were affected and therefore only a small proportion of the sample mothers were affected by the policy changes; 2) Some states had compliance rates that were consistently low or significantly dropped after the law changes which cast a doubt on the laws' efficacy; 3) Perhaps due to the complex ways in which compulsory schooling policies affect academic achievement and performance, they were not good predictors of mother's grade completion and high school diploma receipt. All of these issues are potential causes for making them weak instruments. When an instrumental variables (IV) regression suffers from a weak instrument problem, 1) IV estimates will have larger standard errors and therefore the precision of the estimates will be low, i.e. asymptotic variance is higher than that of OLS estimator and it will be larger if the correlation between the endogenous regressor and the instrument is lower (asymptotic problem #1); 2) IV estimates may be inconsistent if the instrument is not entirely exogenous (asymptotic problem #2); and 3) in finite samples, IV estimates will be biased in the same direction as the OLS estimates and the magnitude of the bias is inversely related to sample size and the correlation between the instrument and the endogenous regressor (Bound, Jaeger and Baker 1995).

Moreover, the complex relationship between compulsory schooling laws and mother's schooling also suggests potential violation of the exclusion restriction of instrument validity since compulsory schooling laws, specifically school entry laws, affect children's health care use and health through causal pathways other than mother's grade attainment or high school completion such as academic performance and grade retention. If the instruments are not valid, IV estimates will be both biased and inconsistent from the OLS estimates (Hahn and Hausman 2003).

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CHAPTER TWO:
THE EFFECT OF HEALTH INSURANCE ON CHILDREN'S HEALTH CARE USE AND HEALTH: EVIDENCE FROM MEDICAID AND SCHIP EXPANSIONS

2.1. Introduction

Although there was a steady increase in children's health insurance coverage in the 1990s with Medicaid expansions and the implementation of State Children's Health Insurance Program (SCHIP), the uninsured rate among children still remains a big problem (Holahan and Cook 2007). As of 2006, more than 9 million children were without coverage (Holahan and Cook 2007, DeNavas-Walt, Proctor and Smith 2007, Kaiser Family Foundation 2008). With a large number of children still uninsured, addressing the low insurance coverage among children is of great concern to the nation because of its potential for detrimental effect on health care use and health (Dubay et al 2007).

One major concern among the policy makers is the low insurance coverage among certain subgroups of children. While eligibility for Medicaid and SCHIP has increased considerably for the children over the last decade, insurance coverage still remains very low for a certain subset of the children. One such subset is immigrant (non-citizen) children. A quarter of uninsured children who have income below the public health insurance eligibility threshold are legal immigrant children. They are barred from obtaining coverage because of new eligibility rules that banned coverage to recent immigrants to the country (Dubay et al 2007). Compared to the citizen children, immigrant children are more than twice as likely to be uninsured, more than three times likely not to have a usual place of care and more than twice as likely to be in poor reported health (Capps 2001). Currently, 20% of the children in the United

States are either immigrants themselves or from immigrant families and this number is growing rapidly; since the 1990s, the growth rate of the number of children in immigrant families has been seven times faster than that of the children in the native families (Morse 2008).

Another subgroup with low insurance coverage is older adolescents aged 19 and 20. Adolescents who are above 18 are not eligible for SCHIP and have limited eligibility for Medicaid (Almeida and Kenney 2000). At age 19, adolescents also grow out of their parents' employer health insurance (Collins et al 2007). About a half of the older adolescents hold jobs without health insurance coverage (Callahan and Cooper 2004, Quinn, Schoen, Buatti 2000) and are much more likely to be in poverty than the older workers (Bureau of Labor Statistics 2005). They are often too poor to afford private health insurance. Consequently they have the highest rate of uninsurance among any age group of the U. S. population (Risling et al 2007). While there have been uncoordinated efforts by the states and private insurers to extend coverage to this group, there have not been any national strategies to promote these efforts (Callahan 2007). This is of concern given that adolescents have a high risk of unintended pregnancies, sexually transmitted diseases, substance abuse and injuries (Blum 1995, Henshaw 1998, CDC 2001, Anderson 1999).

Insurance is important not for its own sake but for its facilitation to health care and ultimately better health as well as financial protection for unforeseeable health shocks. Insurance is expected to improve insurers' access to health care and indirectly improve health. Insurance will have an effect on health when two conditions are met: 1) insurance increases the use of appropriate and timely health care; and 2) this increased health care translates to improved health. For children, understanding the relationship between health insurance status, health care use and health status is extremely crucial given their high needs of health care and the importance of healthy growth in the formative years. RAND randomized health insurance experiment conducted in the early 1970s showed that some health care uses are indeed affected by insurance among children; children were sensitive to price for outpatient health care but not for inpatient health care. However, the experiment also found that difference in outpatient health care use as a result of different co-payment levels did not ultimately affect health (See Newhouse and the Insurance Experiment Group 1993, Manning et al 1987 for summary of findings). RAND experiment provided an indispensable understanding of the causal link between health insurance, health care use and health. However, although the results are unfortunately outdated, social experiments of this comprehensiveness and magnitude are impossible to implement in today's environment.[1] The next best option to randomized controlled experiment is a quasi-experimental study that addresses endogeneity of health insurance and this is the main attempt in this study.

This study examines the effect of health insurance on children's use of health care and health status by using the 1992-2002 National Health Interview Survey. This is the time period of expansion in public insurance programs and the increase in children's insurance coverage. Variation in Medicaid expansion and SCHIP implementation is used as a source of identification for the analysis. To examine the effect of health insurance on children's use of health care and health status, an instrumental variables (IV) fixed effects model is estimated. Simulated eligibility measures for public insurance for children and their families were calculated from Current Population Survey (CPS) and used as instruments for health insurance coverage (any coverage) which are potentially endogenous to the health outcomes. Simulated measure of children's own eligibility for public health insurance is the

[1] Moreover, the RAND experiment did not have an uninsured group for comparison (group with highest co-payment level paid 95% of the actual medical costs) and therefore strictly speaking, it allowed researchers to only infer the effects of insurance by comparing outcomes of children in different co-payment groups.

fraction of a nationally representative sample of children who are eligible for public insurance (Medicaid or SCHIP) in a given state in a given month and year for each age (see Currie and Gruber 1996a for example). Simulated measure of children's family eligibility for public health insurance is the family mean of simulated eligibilities after assigning children's simulated eligibilities that vary by state, year, month and age and adults' simulated eligibilities that vary by state, year, month, sex and female head of household (see Gruber and Simon 2007 for example). These fractions are the degrees to which one state or year is more generous in its treatment compared to another state or year. Intuitively, the identification method exploits the fact that some states implemented more generous public insurance eligibility rules at different times. It correlates these differences in magnitudes and periods of eligibility expansions with the changes in children's health care use and health outcomes.

This study examines the effect of health insurance coverage using the variations from public health insurance expansions. The paper uses the range of outcomes that capture inpatient and outpatient health care as well as ultimate health status. With different outcomes seen for inpatient and outpatient services by co-payment levels, examining the use of both types of services is essential. It examines the effect on all children and by different age groups since studies show varying effects of public insurance expansion on insurance status by age (Leininger 2007a). It also separately estimates the effect on immigrant children. The effect on older adolescents is explored using graphical and regression discontinuity techniques. All the models control for observable individual and state characteristics and include state, year and child's age fixed effects.

Many studies including this study examine the same fundamental question of whether public health insurance expansions affected health insurance coverage and whether this change in coverage affected outcomes such as health care use and health (e.g. Currie, Decker and Lin 2008, Wang et al. 2007a, Currie 1999). To identify the causal effects, all studies basically correlate change in children's insurance status, from no insurance to public insurance, with public health insurance eligibility expansions. Levy and Meltzer (2001) concluded in their literature review that most evidence points toward a small, positive effect of health insurance on health with the effect concentrating on vulnerable populations including infants and low income children. This study provides additional evidence on the effect of health insurance on health status by using different time periods (1992-2002, 1997-2002), for different subsets of children (all children, by age groups, immigrant children) and for different outcome measures (health care use including preventive care use, curative outpatient care and inpatient care and health).

This study also provides exploratory analyses on older adolescents. Older adolescents aged 19 and above are an age group that is especially important. Because they have been left out of the realm of both private and public health insurance, they have the highest uninsured rate among all age groups. Given that this age group has its unique health vulnerabilities, this study will provide an informative preliminary analysis for further investigation.

The remainder of the paper proceeds as follows. Section 2 introduces some background information on Medicaid expansions and SCHIP implementation. Section 3 reviews existing literature on public health insurance (Medicaid or SCHIP) and children's health and health care use. Section 4 explains the basic model and conceptual framework underlying the study. Section 5 outlines the identification procedures. Section 6 introduces the data. Section 7 tests for IV method. Section 8 presents the results of the descriptive and the regression analyses for all children and by age groups. Section 9 presents results of descriptive and regression analyses for immigrant children. Section 10 presents results of descriptive and regression analyses for older adolescents. Section 11 presents discussion of the results and Section 12 offers conclusions and implications for future research.

2.2. Background Information on Medicaid and SCHIP Expansions

Medicaid was initially created in 1965 to provide heath insurance to welfare receiving population (single parent families, elderly, blind and disabled) and soon after, states were given freedom to extend eligibility to the “Ribicoff children” (children who meet the financial criteria but not ‘categorical’ or non-financial criteria for Medicaid, e.g. low income children in two parent families) and to the medically needy. But the beginning of the mid 1980s was when Medicaid substantially expanded eligibility for children and pregnant women. From 1984 to 1987, eligibility was extended to those who were financially comparable to AFDC recipients but were not eligible because of other reasons such as family structure. From 1987 till today, income cutoff for eligibility increased considerably. The largest expansions for children occurred from 1998 to 2000 when states created State Children's Health Insurance Program (SCHIP) programs. SCHIP was established through the Balanced Budget Act of 1997 to allow states to extend eligibility to low-income children under age 19 who were not eligible for Medicaid. To this date, none of the states cover children beyond age 18. SCHIP typically covers children who have higher incomes than those who are covered by Medicaid and the range of services are typically more comprehensive in Medicaid than SCHIP, especially when administered as a separate SCHIP program and not as a Medicaid expansion program (For more information about the coverage differences, refer to Rosenbaum et al 2004). States began implementing SCHIP as early as 1998 and by September 30, 1999, all states and the District of Columbia had their own SCHIP program in place (Cohen and Bloom 2005). As of July 2005, majority of states had SCHIP eligibility at or above 200% federal poverty threshold (NASHP

2005).[2] By December 2004, 4 million children were covered by SCHIP in all states and the District of Columbia (Smith and Rousseau. 2005). Medicaid eligibility expansions started earlier for younger children and pregnant women in the late 1980s and was extended to older children in the late 1990s (Currie, Decker and Lin 2008). While the eligibility expansions were gradual for younger children, older children experienced a sharper increase in eligibility in the late 1990s (Currie, Decker and Lin 2008).

Expansion of parental eligibility to Medicaid/SCHIP began around the same time frame. Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) delinked welfare cash assistance and Medicaid eligibility. It also allowed states to apply less restrictive income eligibility rules to families as long as they met the categorical standards. In August 1998, Department of Health and Human Services expanded eligibility to all two parent families that met income and resource standards by altering definition of unemployment, one of the categorical standards for Medicaid. In July 2000, Health Care Financing Administration provided guidelines for states to obtain waivers to extend SCHIP coverage to parents. Health Insurance Flexibility and Accountability initiative issued in August 2001 also gave incentives for states to extend parental coverage. It provided enhanced waiver flexibility for designing public insurance programs to states that extend coverage to parents (For more details, see Gruber 2003, Dubay and Kenney 2003, Broaddus et al 2002).

Before 1996, legal immigrants were covered by Medicaid under the same eligibility criteria as the citizens. However, after the passage of PRWORA, there were many changes in federal regulations that made immigrant children less likely to be covered by public health insurance. Legal immigrants became ineligible for Medicaid coverage for the first five years in the country regardless of their financial need.

[2] Federal poverty thresholds for a family of three in 2005 were: $16,090 for 48 contiguous states and District of Columbia, $20,110 for Alaska and $18,510 for Hawaii.

Exemptions were given only to refugees, other humanitarian immigrants and active members or veterans of US Armed Forces and their families. SCHIP was also introduced with the same eligibility criteria as Medicaid for the legal immigrants.[3] The documentation requirements also tightened. For some legal immigrants, income calculations for eligibility now need to include a portion of their sponsor's income and resources regardless of whether their income is shared with the sponsor. While about half of the states use state funds to cover legal immigrant children and pregnant women, these coverages are highly responsive to state's economic conditions.

Once children reach age 19, most of them become ineligible for Medicaid or SCHIP. Although there have been several state proposals that extend public insurance coverage to children beyond age 18, none has been realized so far (Collins et al 2007). Only in special cases, older adolescents of age 19 or above may obtain public health insurance. Medicaid is required to cover older adolescents of ages 19 and 20 if they are either pregnant or parents. SCHIP is required to offer coverage if they are pregnant. There are several other coverage options under Medicaid or SCHIP for older adolescents of ages 19 and 20 including: "Ribicoff Children" (15 states);[4] medically needy children (16 states); low income childless independent adults (15 states); high risk children who require special health care (45 states) (those with disabilities, those in psychiatric facilities or other institutional care, those who were previously in foster care, and those who require family planning services) (Fox et al 2007). Although there is no literature examining the magnitude of coverage in these special cases, most states had not adopted broad coverage options for this age group.

[3] Undocumented immigrants and immigrants with visas for temporary stays are continued to be barred from Medicaid and SCHIP.
[4] As stated earlier, "Ribicoff children" are those who meet the financial criteria but not categorical criteria for Medicaid such as low income children in two parent families.

Therefore most older adolescents did not benefit from the Medicaid expansions and SCHIP implementation in the 1990s.[5]

2.3. Literature

Health is determined not only by use of health care but also by income, health behavior, health awareness and other various environmental factors. All of these also affect insurance status of an individual which impacts his/her use of health care and ultimately health. At the same time, health affects the use of health care (sick seek more health care than the healthy) and the decision for insurance coverage (adverse selection). This complexity where health insurance is affected both by health and by other underlying factors that are correlated with health (both observed and unobserved) necessitates addressing the issue of endogeneity of health insurance. Since a randomized control social experiment cannot be implemented in today's environment, several approaches have been used to correct for endogeneity in quasi-experimental studies. One method used in recent studies is instrumental variables (IV) method. Using the variation created by Medicaid and SCHIP expansions, studies have used simulated public insurance eligibilities (e.g. Currie and Gruber 1996a, Gruber and Simon 2007) or other state and family level characteristics (Wang et al. 2007a) as instruments for health insurance. Other methods that have been used by other researchers are difference-in-differences (e.g. Leininger 2007a) and fixed effects (e.g. Currie and Thomas 1995).

The main analysis in this study follows earlier studies that used simulated public insurance eligibility measures as instruments, such as Currie and Gruber (1996a) and Gruber and Simon (2007), to examine the causal effect of health insurance coverage on children's use of health care and health status outcomes.

[5] To address the low coverage of children beyond age 18, 17 states have passed laws that increased age dependency beyond age 18 for private insurance in the past few years (Collins et al 2007).

Earlier studies point out that family's public health insurance eligibility is an important factor in children's public health insurance coverage due to spillover effects (Cutler and Gruber 1996a, Gruber and Simon 2007). Therefore this study uses simulated eligibilities at both child and family level for instruments.[6] While many earlier studies used simulated public insurance eligibilities as instruments for actual public insurance eligibility (e.g. Currie and Gruber 1996a, Dafny and Gruber 2000, 2005, Currie, Decker and Lin 2008),[7] this study uses the same instruments for children's insurance coverage (i.e. have insurance or not). Since the same instruments are used, identification from both studies comes from the same public insurance eligibility expansions in 1990s and early 2000s. Reasons for this study to examine the effect of any insurance coverage instead of public insurance eligibility are three fold. First, insurance status will have lower reporting error than actual eligibility. Since children's insurance status is available in NHIS whereas actual eligibility is not, eligibility needs to be calculated using various information. The information required to determine eligibility include income that is often reported with great error. Consequently, reporting bias will be larger for eligibility than coverage. Second, the issue of the effect of actual coverage induced by eligibility expansion is as important as the effect of eligibility expansion itself and this has not been explored as extensively in earlier studies. Comparing findings from these two types of studies provides informative insights on the importance of policies that target increasing take up rates. Lastly, although the natural extension of eligibility studies to examine coverage is to study the effects of public insurance coverage (instead of any coverage),

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[6] Earlier studies generally use either one of the instruments (i.e. child or family level) and not both. This study uses both instruments to capture effects of both child and family level expansions of public insurance eligibilities.

[7] These studies use eligibility as opposed to coverage for several reasons. First, eligibility is a policy lever that is easier to maneuver than coverage. Second, eligibility is less likely to be endogenous because it is less likely to reflect behavioral outcomes than coverage which reflects individuals' choices to enroll (or not) in a program (Kaestner, Joyce, and Racine 1999).

this study does not do so because the instruments were not very strong when the effect of insurance coverage were examined by type of insurance. A detailed comparison of the identification strategies between this study and previous eligibility studies are found in the Appendix 2.14.

Most studies that used public insurance eligibility expansions in the 1990s to correct for endogeneity of insurance have looked into the effect of public insurance eligibility and to a much lesser extent, public insurance coverage. Previous studies that focused on eligibility found that public health insurance eligibility increased children's use of preventive care (Currie and Gruber 1996a). They also found that eligibility decreased children's preventable hospitalizations (i.e. hospitalizations that can be avoided by early contact with a primary care physician) but increased overall hospitalizations suggesting increased efficiency in health care (Dafny and Gruber 2005, Kaestner, Joyce, and Racine 1999). However, these changes in health care use did not seem to have affected children's health. A few existing studies found little (both positive and negative) to no effect. They found that Medicaid increased reported activity limitation (Currie and Gruber 1995) and reduced child mortality (Currie and Gruber 1996a) but SCHIP did not impact reported general health status (Currie, Decker and Lin 2008).

One recent study by Currie, Decker and Lin (2008) is important in the context of this paper. They found that public insurance eligibility increased children's use of preventive care (i.e. children with public insurance eligibility are 6.8 percentage points less likely to have gone without doctor visit in the past year) but no effect on their health status. They used 1986-2005 NHIS data and controlled for the endogeneity of public insurance eligibility using the simulated eligibility measure for the child. Whereas their main focus was to understand how public insurance eligibility affected the importance of income in predicting children's health care use and health status, the main focus of this study is to determine how health insurance coverage affected health care use and health status of all children and the subgroups during the period of public insurance expansion. This study builds on their study by using an additional instrument (family level simulated eligibility measure), a wider range of outcome measures and by examining subgroups of children.

As stated earlier, to the author's knowledge, not much work is done on the effect of insurance coverage on children's health care use and health status that uses public insurance eligibility expansions in the 1990s for identification. Existing evidence from national level studies suggest that Medicaid had no impact on the number of doctor visits and reported general health status for white children but a positive effect for Black and Hispanic children (Kaestner, Joyce, and Racine 1999). Medicaid also decreased the incidence of ambulatory care sensitive discharges among low income young children (Kaestner, Joyce, and Racine 1999). SCHIP decreased unmet needs and increased use of both dental and medical care (Wang et al. 2007a, 2007b).

Using data from 1997-2002 NHIS, Wang et al. (2007a) found that public insurance coverage led to a fairly large increase in low income children's medical care access and use.[8] There was however no effect on the unmet need for drug or mental health care. Instrumental variable approach was used to control for the endogeneity of public insurance coverage. They used SCHIP availability, waiting periods, SCHIP or Medicaid income eligibility threshold, Food Stamps receipt and availability of employer health insurance for instruments. They limit the sample to those children who are in the Sample Child file[9] whose family incomes are below state SCHIP eligibility limits. Whereas their main focus is to understand how public insurance

[8] Children with public insurance coverage were approximately 25 and 9 percentage points, about 35% and 100% of the sample mean, more likely than the uninsured to have had a general and a specialty doctor visits in the past year.
[9] Sample Child file includes information for only one child per household.

coverage affected health care access and use (including usual source of care, forgone care or unmet need, use of health care) among low income children, the main focus of this study is to determine how (public) health insurance coverage affected health care use and ultimately health status for all children and by subgroups during the period of public insurance expansion. This study builds on their research by using a different set of instruments, a larger sample of children,[10] a longer time period, a wider range of outcomes including children's health and by examining subgroups of children.[11]

Few studies have focused on immigrant children. Immigrant children are one of the most vulnerable populations in the country and their number is growing rapidly. These studies found some mixed impacts of Medicaid and SCHIP. Currie (1999) found that the Medicaid eligibility expansions in the late 1980s to early 1990s decreased the proportion of children with no doctor visits in the past year by about 35 percentage points. This study adds to the small literature on the effect of public health insurance on immigrant children health care use by providing more recent evidence. It also extends the analysis to their children's health status which has not been explored before.

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[10] As mentioned in the text, Wang et al (2007a) use data from the Sample Child file which includes information for only one child per household. On the other hand, this study mainly uses data from the person file which includes data for all children in the household and therefore the sample size is larger for this study and the variables used in this study come from different questions. For example, the outcomes on general and specialty doctor visits comes from Sample Child file based on the following questions: "During the past 12 months have you seen or talked to the following about [sample child's] health: (1) A general doctor who treats a variety of illnesses (a doctor in general practice, pediatrics, family medicine, or internal medicine) and (2) A medical doctor who specializes in a particular medical disease or problem (other than obstetrician/gynecologist, psychiatrist or ophthalmologist)?" Based on these questions, the proportion of low income children who have had general and specialty doctor visits in the past year is approximately 75-85% and 9-15%, respectively (Wang et al 2007). In this study, the outcomes on doctor visits for 1997-2002 comes from person file based on the following questions: "During those 2 weeks, did {person} see a doctor or other health care professional at a doctor's office, a clinic, an emergency room, or some other place? (Do not include times during an overnight hospital stay) [exclude any baby born during interview week]"; and "How many times did {person} visit a doctor or other health care professional during those 2 weeks?" Based on these questions, the proportion of children who have had doctor visits in the past 2 weeks is approximately 11%.

[11] Although in the earlier versions of the study, the effects of public and private insurance coverage (as opposed to no insurance) on children's health care use and health status were examined using the same instruments for public and private insurance coverage, these analyses were dropped in the later version because the instruments were too weak for public and private insurance coverage.

Studies found that 19 and 20 year olds have higher rates of uninsurance than other children in the same income category (Almeida and Kenney 2000). In fact, they are the fastest growing group in the U. S. population without health insurance in the recent years (Collins et al 2007). Despite this disproportionate risk of being uninsured, there are only a few studies that examine the effect of insurance coverage on older adolescents of which all of them are either correlational or descriptive. These studies find that uninsured older adolescents are more likely to delay or forgo health care. They have fewer doctor visits and are less likely to have a usual source of health care. Consequently, they are more likely to be in poor health (Callahan and Cooper 2005, McManus, Greaney and Newacheck 1989). Moreover, most previous studies group these older adolescents with other age groups (e.g. younger adolescents, other children or other nonelderly adults) making it difficult to understand the problems unique to this age group (McManus, Greaney and Newacheck 1989). This study provides informative analysis on the benefits of insurance coverage for older adolescents' health and health care use.

In sum, this study examines the effect of health insurance on children's health and health care use with an emphasis on subgroups with low coverage. It looks at health insurance coverage as opposed to public insurance eligibility but use the same variation in public insurance eligibility expansion for identification as previous eligibility studies. Knowing the effect of coverage is informative in determining the relevance of the policies that aim to increase enrollment to public and private insurance programs without altering eligibility rules. The findings from this study will complement Currie, Decker and Lin (2008) by examining the effects of public insurance expansion on children who actually took up coverage (either private or public) and by using a greater set of outcome measures.

Lastly, this study will contribute to the small literature that exists on health insurance and immigrant children and older adolescents, a population that historically have had difficulty in the nation's health care system. Covering these children will most likely be one of the most pressing issues in the future of U.S. health care system because of the renewed awareness to the problem of uninsurance and the growing interest in improving health insurance coverage in our nation (Luo 2008).

2.4. Conceptual Framework

Health insurance may affect child health through several complex pathways. Health insurance may affect the marginal cost of health care. Health insurance reduces marginal cost of health care for the individuals by pooling individuals' risks. For private insurance, children's families usually pay annual premiums and a co-payment for every health care use. The more payment families make as premiums, the marginal cost of health care (i.e. co-payment) goes down. Public insurance almost brings down the marginal cost of health care to zero except for a small co-payment in the case of SCHIP if applicable. This reduction in the marginal cost of health care should in turn increase access to care and should ultimately lead to improved child health.

For children who became newly eligible and signed up for Medicaid/SCHIP due to eligibility expansions, marginal cost of health insurance decreased considerably (down to zero, except for time and stigma costs of signing up for eligible children because they can now get coverage for free). If children are responsive to this change in health care cost, then they will most likely increase health care use and this may ultimately affect health. Expansions to parents that began as a part of welfare reform may also have affected children's use of health care and health status by altering children's health insurance coverage. By insuring parents along with children, marginal increase in costs of obtaining and renewing coverage is considerably less than the marginal increase in benefits (Sommers 2006). For example, while the paperwork and procedures to renew coverage do not increase proportionally with family size, financial benefits of health insurance and improved health status increase proportionally. Thus, expanded eligibility to parents should have a non-negative effect on children's Medicaid/SCHIP coverage and this should ultimately affect children's use of health care and health as explained above.

However, child's health insurance coverage may not change if Medicaid/SCHIP expansions crowd out private health insurance, i.e. children who were already covered by private insurance may simply switch to public insurance once they become eligible for public insurance.[12] In this case, their health insurance status (i.e. covered or not) will remain the same. If the effect of insurance on children's health care use and health does not vary by type of insurance, then the ultimate effect on children's use of health care and health may be nill.

2.5. Identification Procedures

The impact of Medicaid/SCHIP expansions on children's health care use and child health is estimated using an instrumental variables (IV) fixed effects model.

In the first stage, a child's health insurance status is estimated using simulated measure of child's and family's public health eligibilities as instruments:

$$AnyIns_{ist} = \alpha_0 + \alpha_1 SimPubElig_{ist} + \alpha_2 SimFamElig_{ist} + \varepsilon_{ist} \quad (1)$$

where $AnyIns_{ist}$ is the health insurance status (i.e. have any insurance or not) of the child *i*. $SimPubElig_{ist}$ is the fraction of a nationally representative sample of children who are eligible for public insurance (Medicaid or SCHIP) in a given state in a given month and year for each age calculated from CPS. This simulated measure of child's

[12] Previous studies that found crowd out rate of about 5-30% for Medicaid and 50-60% for SCHIP (Gruber and Simon 2007).

own public health insurance eligibility varies by state, year, month, and age (see Currie and Gruber 1996a for example).[13] $SimFamilyElig_{ist}$ is the simulated measure of child's family eligibility for public health insurance that is calculated in a similar way using CPS. After assigning each child with his/her own simulated eligibility measure, a simulated measure of public insurance eligibility is assigned to each adult that varies by state, year, month, sex and female head of household. Then the family mean of the simulated eligibilities are taken to be used as an instrument (see Gruber and Simon 2007 for example). These fractions are the degrees to which one state (or year) is more generous in its treatment compared to another state or year. Instruments are valid methods when two conditions are met: 1) Instrument relevance: The instrument must be correlated with the treatment of interest. In the regression of outcome Y on instrument Z, $Y = \alpha_0 + \alpha_1 Z + \varepsilon$, the instrument relevance condition says that $\alpha_1 \neq 0$; and 2) Exclusion restriction: The instrument must not be correlated with the error term, i.e. omitted variables in the outcome equation. In the context of the above regression, the exclusion restriction says that $Cov(Z, \varepsilon) = 0$. Theoretically, simulated eligibilities satisfy both conditions. As mentioned earlier, state's public insurance expansions reduced marginal cost of health insurance. If children are responsive to these cost changes, then eligibility expansions must be correlated changes in overall insurance coverage. This satisfies condition 1. It also fulfills condition 2 since the measures are solely based on policies that were in place in that state/year which were most likely exogenous and/or captured by control variables included in the regression. However, if either one of the conditions is not met, the instruments are not valid. The first

[13] Both Davidoff et al. (2005) and Currie and Gruber (1996) calculated SCHIP and Medicaid eligibility with NHIS income data that are only available in categories. Davidoff et al. (2005) do not discuss in detail about how they calculated eligibility. Currie and Gruber (1996) imputed income by randomly choosing a point in the income bracket and for missing income data, they regressed yearly income on household characteristics in CPS to get the coefficients that were then used to estimate the income in NHIS. This study follows the method of calculating eligibility and simulated eligibility instrument used by Currie and Gruber (1996).

condition can be tested using a conventional rule of first stage F statistic greater than 10. The findings from the study build on the assumption that the second condition is met and that there are no omitted variables that affect public insurance eligibility rules and children's health care use and health.

In the second stage, the effect of child's health insurance status on child's health care access and outcomes is estimated using his/her predicted health insurance status from (1) and other exogenous variables:

$$Y_{ist} = \beta_0 + \beta_1 X_{ist} + \beta_2 Z_{ist} + \beta_3 PredAnyIns_{ist} + \beta_4 \sigma_s + \beta_5 \omega_t + \beta_6 \sigma_{ist} \qquad (2)$$

where Y_{ist} is one of the several access to care measures or the health outcomes of child i in state s in year t: a dummy indicating whether the child is in excellent health,[14] a dummy indicating whether the child has any limitation of activity, a dummy indicating whether the child had doctor visits in the past two weeks and the number of such visits (for 1997-2002), a dummy indicating whether the child had school days lost due to illness or injuries in the past year and number of such days (only for ages 5 to 17 and for 1997-2002), a dummy indicating whether the child had short stay hospital episodes in the past year and the number of such episodes (for 1997-2002), and a dummy indicating whether the child had short stay hospital days in the past year and the number of such days (for 1997-2002).[15] $PredAnyIns_{ist}$ is the predicted health insurance status of the child i in state s in year t.

X_{ist} is a vector of individual characteristics including child's age, sex, race, mother's education, mother's age, family size, mother's marital status, family income as percent of federal poverty level and its square, and mother's work status. Z_{st} is a vector of state characteristics including the seasonally-adjusted unemployment rate

[14] 5 point scale for reported general health was also used to define poor health and was used as outcome. Since results were consistent with the findings for excellent health, the results are not presented here.

[15] Separate analyses are conducted for different periods of years because some outcome measures were not consistently measured over time in NHIS. Further explanation can be found in the data section.

(data from the Bureau of Labor Statistics); real median wages (data from the CPS); the maximum value of the federal and state EITC for a single mother with two children (data from Green Book; Leigh, 2003); the annual employment growth rate (data from the National Bureau of Economic Analysis); the amount of federal housing money spent per 1,000 residents in the state (data from the U.S. Census Bureau); a dummy variable indicating whether the state has AFDC waiver, and a dummy indicating whether the state has TANF. Child's age is controlled as age fixed effects and not with a continuous variable. As discussed earlier, the change in the generosity of public health insurance eligibility varied depending on the child's age and therefore it is more appropriate to compare children in the same age group than to compare with children from all other age groups.

If this model is estimated by OLS, $\beta_{3,}$ could be biased. As mentioned earlier, $AnyIns_{ist}$ captures many characteristics about the person including income (most likely in a nonlinear way), unobserved health care needs and health. Therefore, IV approach is used with simulated eligibility measures as instruments for children's health insurance coverage in a two stage set-up from which the causal effect of children's health insurance status on their health care access and health outcomes are derived. Mathematical explanation of endogeneity bias is provided in the Appendix 2.15. Basically, if there is a correlation between one or more of the regressor variables and the error term, OLS estimator will be biased and inconsistent. The direction of the bias depends on the problem. One may believe that higher income individuals who are more aware about health are more likely to have health insurance. If so, OLS estimates will be positively biased because these individuals are more likely to be healthier due to greater access to high quality health care and healthier lifestyles. On the other hand, one may believe that individuals with greater health care needs due to unobserved health problems (i.e. unobservable to the researchers) are more likely to have health insurance. If this is the case, then there will be a negative bias because individuals will be negatively selected into insurance.

Magnitudes of the estimated effects also depend on case by case bases. OLS estimates produce average treatment effect whereas IV estimates produce local average treatment effect or LATE. Therefore while OLS produces estimates of the effects on the average individual in the sample, IV produces estimates of the effects on the population that is affected by the instrument. Depending on the subgroup of population that the instrument is working on, magnitudes of two estimates vary. For example, in this paper, public insurance eligibility expansions are used to identify the effect of health insurance coverage. Since public insurance eligibility expansions most likely have impacted low income children and not high income children, IV estimates produce effects that are 'local' to low income children. If one believes that the effects of health insurance on a low income child are greater than those on an average child (e.g. a low income child may have greater unmet need for health care), then the magnitude of the IV estimates will be larger than the OLS estimates. On the other hand, if one believes that the effects on a low income child are smaller than those on an average child (e.g. a low income child may not be able to make the full use of health insurance services), then the magnitude of the IV estimates will be smaller than the OLS estimates. Therefore without empirical analyses, it is difficult to predict the direction or the magnitude difference between IV and OLS estimates.

All regressions include state and year fixed effects, σ_s and ϖ_t, that are used to control for unobserved heterogeneity that may be correlated with the policy variable. If there are any differences in the effects that are unique to a state or a year, then fixed effects control for these effects. ε_{ist} is the error term that captures remaining unobservables that are not captured in the equation and are clustered at the state level. Equations are estimated using a linear probability model for categorical dependent variables and an OLS for continuous dependent variables. However, one should keep in mind that linear probability models should be used with caution for dichotomous outcome variables. More details are provided in the Appendix 2.16.

2.6. Data

2.6.1. National Health Interview Survey

The data for this study comes from National Health Interview Survey, which is a cross-sectional household interview survey conducted continuously throughout each year since 1957. It is a representative sample of the U.S. population from all 50 States and the District of Columbia. NHIS monitors the health of the U.S. population through collection of various health characteristics by many demographic and socioeconomic characteristics. It covers the civilian non-institutionalized population of the United States living at the time of the interview. Patients in long-term care facilities, persons on active duty with the Armed Forces, and U.S. nationals living in foreign countries are excluded from the survey. The annual response rate of NHIS is greater than 90 percent (Vital and Health Statistics Summary Health Statistics for U.S. Adults and Children Reports: National Health Interview Survey for various years). Blacks and Hispanics have been oversampled since 1995. NHIS uses stratified multistage probability sampling. NHIS underwent two changes in 1997. The first is a change in the interview procedure. Until 1997, interviews were conducted using paper and pencil. However, starting from 1997, they have been conducted using a computer assisted personal interviewer (CAPI). The second change, which is the crucial one, is the questionnaire redesign that has been in effect starting with the 1997 survey.

This study uses the 1992-2002 NHIS data for the analyses. The main sample for this study consists of children between ages 0-18 years, who lived with their mothers at the time of interview, and are the children of the household reference person.[16] Therefore all children who are the grandchildren of the household reference person are excluded. In addition to the analyses for all children, the analyses are conducted by different age groups (ages 0-6, 7-12, 13-18). This was done to reflect the varying health care needs and effectiveness of medical intervention of health by age. Immigrant children include children who have at least one foreign born parent.[17]

Exploratory analysis on older adolescents use samples from 1997-2002 and includes all adolescents between ages 16-21 regardless of their relationship to the household reference person. Unlike the main sample, this sample is not limited to children of the household reference person. All adolescents that met the age criteria were kept to understand how the proportion of adolescents living with parents change by age. The analysis is also limited to the years between 1997 and 2002. Until 1996, the minimum age limit to qualify as a reference person was age 19. However, from 1997 onwards, NHIS changed the limit to age 18. This is important for the analysis of older adolescents in this study since the source of identification comes from the sharp discontinuity in health insurance coverage at age 18 and 19. Therefore any other sharp changes that may have occurred at the same time will invalidate the method.[18] During

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[16] In the sample child core, one child is randomly selected from each family and the basic information on health status, health care services, and behavior is collected from a responsible adult family member residing in the household.

[17] The results using the sample of immigrant children defined as those who have foreign born mother are qualitatively similar to those presented in this paper.

[18] Until 1996, NHIS defined reference person as "the first household member 19 years or older mentioned by the respondent in answer to question la; i.e., the person who owns or rents the sample unit. If no household member occupying the sample unit owns or rents the unit, the reference person is the first household member mentioned who is 19 years of age or older. On rare occasions, you may encounter sample units occupied entirely by persons under 19 years old. When this occurs, use the following rules to designate the reference person: 1) If one of the household members owns or rents the sample unit, designate that person as the reference person; 2) If more than one household member owns or rents the sample unit, designate the oldest member as the reference person; 3) If none of the household members owns or rents the sample unit, designate the oldest household member as the reference person. " (NCHS 1996) From 1997, NHIS redefined the definition of reference person as "the person or one of the persons, 18 years old or older, who owns or rents the sample unit, that is, the first person mentioned by the respondent in the household roster. If more than one household member owns or rents the sample unit, or if none of the household members owns or rents the sample unit, designate

1992-1996, since adolescents only became eligible to be a reference person once they reach age 19, many adolescents of age 18 (who satisfied all conditions to be a reference person other than age) most likely became a reference person as soon as they turned age 19. This may have created a sudden increase in the proportion of adolescents who are reference persons at age 19. Since this coincides with the drop in coverage seen at age 19, it becomes difficult to attribute the change in outcomes to the change in insurance status. By limiting the years to 1997-2002, the problem is solved since adolescents are already eligible to be a reference person from age 18.

Most variables used in this study come from the person file or from the supplement files that provide information about all children included in the person file. The only exception is school days lost due to illness or injury in the past year for the years 1997-2002. This information comes from Sample Child file which includes information about only one child per household (i.e. not all children in the person file are included in the Sample Child file). Therefore the sample size for regressions using school days lost to illness or injury in the past year for the years 1997-2002 is smaller compared to other regressions. Also note that since all the other variables are for all persons in the person file, the samples in this study are much larger than those in the study by Wang et al (2007a) who use data from the Sample Child files (by approximately 10,000 observations).[19]

NHIS has detailed information on health insurance coverage from the health insurance supplement file for 1992-1996 and from the person file for 1997-2002. For the years 1992-1996, respondents are asked about their health insurance coverage in

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the oldest household member as the reference person. If no household member is 18 years old or older, designate the oldest person that owns or rents the sample unit as the reference person." (NCHS 1997)

[19] Wang et al (2007a) limited the sample to children in the Sample Child files for a wider selection of outcome measures for access to medical care and medical service use and to avoid within family correlation. Sample children file has detailed information on access (e.g. usual place for care, delayed care due to cost, unmet need for medical care, drug and mental care) and health care use by type of care (e.g. use of general and specialty doctor visit).

the previous month whereas for the years 1997-2002, they are asked about their coverage at the time of interview. Health insurance information from the first half of 1993 was not available in NHIS. The 1993 Health Insurance topic was administered only in the last half of 1993 (Quarters 3 and 4) for everyone in the NHIS. Therefore in this study, first half of 1993 is dropped from the sample.

Many outcome measures were not consistent over time due to the questionnaire redesign in 1997. The analyses for these measures had to be done for two separate time periods. However, since instruments were too weak when the analyses were restricted to only 1992-1996, these measures were conducted only for 1997-2002. Details are provided in the Appendix 2.17. Sample sizes vary by dependent variables used for analysis from approximately 44,000 to 241,000 due to missing observations and exclusion of irrelevant ages and years.[20]

Table 2.1 provides the descriptive statistics of all children and by age groups. Overall, individual characteristics are similar for all age groups. Older children are from slightly higher income families than their younger counterparts. This perhaps reflects that older children naturally have older parents who more likely have higher earning capacity than younger parents who have less work experience. They may also be more likely to be in the labor force due to less child care needs at home compared to younger mothers. As for insurance, perhaps reflecting more generous public insurance coverage for younger children, children between ages 0-6 have the highest public health insurance coverage rate whereas children between ages 13-18 have the highest employer/private health insurance coverage rate. The youngest children have the highest insurance coverage among all children. While the health status seems to be similar for all age groups, younger children use more health care than their older counterparts reflecting the higher health care needs at younger ages.

[20] Sample used to estimate number of school days lost to illness or injury in past 12 months includes children ages 5 to 17.

Table 2.1 Descriptive Table for All Children and by Age Groups

Variables	All			Ages 0-6			Ages 7-12			Ages 13-18		
	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	N
Individual Characteristics												
Female	0.488	0.500	243021	0.487	0.500	89227	0.491	0.500	82157	0.487	0.500	71637
NH White	0.198	0.399	243021	0.200	0.400	89227	0.198	0.398	82157	0.197	0.397	71637
Hispanic	0.229	0.420	243021	0.244	0.429	89227	0.227	0.419	82157	0.213	0.409	71637
%FPL income	260.226	178.674	243021	245.101	177.874	89227	253.069	170.372	82157	287.262	185.819	71637
Family size	4.438	1.409	243021	4.309	1.356	89227	4.584	1.412	82157	4.433	1.454	71637
Mother's education												
HS graduate	0.190	0.392	243021	0.190	0.392	89227	0.189	0.392	82157	0.191	0.393	71637
Some college	0.365	0.481	243021	0.352	0.478	89227	0.370	0.483	82157	0.376	0.484	71637
College graduate	0.258	0.438	243021	0.269	0.443	89227	0.258	0.438	82157	0.246	0.430	71637
Mother's age	35.927	7.311	243021	31.105	6.173	89227	36.410	5.976	82157	41.375	5.830	71637
Mother married	0.782	0.413	243021	0.797	0.402	89227	0.779	0.415	82157	0.768	0.422	71637
Mother working	0.667	0.471	243021	0.589	0.492	89227	0.685	0.464	82157	0.745	0.436	71637
Health Insurance												
Any Insurance	0.863	0.343	243021	0.879	0.326	89227	0.861	0.346	82157	0.846	0.361	71637
Public Insurance	0.198	0.398	243021	0.256	0.436	89227	0.189	0.392	82157	0.135	0.342	71637
Employer/Private Insurance	0.681	0.466	243021	0.644	0.479	89227	0.687	0.464	82157	0.722	0.448	71637
Health care Use												
1997-2002												
Any doctor visits (past 2 wks)	0.115	0.320	130806	0.158	0.364	46493	0.093	0.290	44630	0.092	0.289	39683
# doctor visits (past 2 wks)	0.145	0.471	130806	0.194	0.525	46493	0.113	0.401	44630	0.122	0.472	39683
Any hospital episodes (past yr)	0.060	0.237	125012	0.128	0.334	40689	0.017	0.131	44620	0.027	0.162	39703
# hospital episodes (past yr)	0.073	0.379	125012	0.152	0.477	40689	0.023	0.259	44620	0.036	0.345	39703
Any hospital days (past yr)	0.059	0.235	124997	0.126	0.331	40684	0.017	0.131	44616	0.027	0.162	39697
# hospital days (past yr)	0.273	3.232	124997	0.513	3.623	40684	0.097	2.032	44616	0.192	3.783	39697

Table 2.1 (Continued)

Variables	All			Ages 0-6			Ages 7-12			Ages 13-18		
	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	N
<u>Health Status</u>												
1992-2002												
Excellent health	0.526	0.499	241614	0.543	0.498	88623	0.525	0.499	81700	0.504	0.500	71291
Limitation in Activities	0.064	0.245	243021	0.039	0.194	89227	0.081	0.273	82157	0.076	0.266	71637
1997-2002												
Any lost school days (past yr)	0.735	0.441	43788	0.709	0.454	6460	0.747	0.435	19579	0.731	0.443	17749
# lost school days (past yr)	7.478	36.979	43788	21.753	79.619	6460	3.964	14.220	19579	6.151	27.398	17749

Note: Fractions of the children with health insurance for all ages and by age group using unweighted data from 1992-2002 NHIS. The sample consists of children ages 0-18 years, who live with their mother at the time of interview, and are the children of the household reference person. Health insurance information for the years 1992-1996 and for the years 1997-2002 are for the previous month and at the time of interview, respectively.

2.6.2. Current Population Survey

Simulated measure of child's own and family eligibilities for public health insurance comes from Current Population Survey March supplement. The Current Population Survey is a monthly survey of about 50,000 households conducted by the Census. It covers the civilian non-institutionalized population of the United States. Simulated eligibility measures were merged to individuals using month and year for which health insurance was given, i.e. previous month and year at the time of interview for the years 1992-1996; month and year at the time of interview for the years 1997-2002.

Figure 2.1 shows the change in the simulated fractions of children that are eligible for public health insurance (Medicaid or SCHIP) from 1992 to 2002. Panel A suggests that the simulated fraction of children eligible for Medicaid has risen steadily for all ages but more so for the older age groups. While the rate of increase is greatest for older children, fraction of children eligible for Medicaid is greatest among the youngest age group. Panel B shows the simulated fraction of children eligible for SCHIP. Here, although the greatest fraction of children eligible for SCHIP is among ages 7-12 and the lowest is among the youngest age group, the greatest increase in the eligibilities is found in the two older age groups. Panel C is the simulated eligibilities for Medicaid and SCHIP combined and this reinforces the trend from Panels A and B. The trends are consistent with the general trend of increase in eligibility thresholds seen in 1990s and early 2000s. There are also variations in eligibility expansions across states. Currie, Decker and Lin (2008) compares changes in eligibility thresholds by age groups over time for California, Illinois, New York and Texas and find a big variation across states. Appendix Figure 2.2 shows that these four states indeed show different patterns of eligibility threshold changes over time.

The identification from this study comes from the variation in the timing and intensity of Medicaid and SCHIP policies within states for each age group, i.e. the children are compared within the same age group within each state.

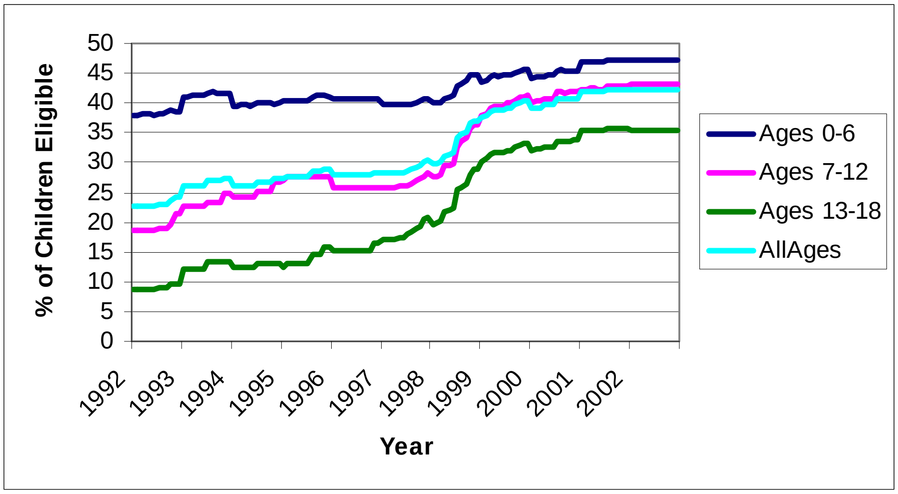

Figure 2.1 Simulated Fraction of Public Insurance (Medicaid or SCHIP) Eligible Children for Years 1992-2002 for All Ages and by Age Group

Note: Fraction of a nationally representative sample of children who are eligible for public insurance (Medicaid or SCHIP) in a given state in a given year for each age calculated from 1992-2002 CPS. Panel A shows the simulated fraction of children eligible for Medicaid , Panel B shows the simulated fraction of children eligible for SCHIP and Panel C shows the simulated eligibilities for both Medicaid and SCHIP.

For example, if New York State increases eligibility threshold for adolescents in one year, adolescents in that year and beyond are more likely to have insurance than adolescents that were in New York State in years prior to the increase. Moreover the magnitude of the difference will correlate with the intensity of the eligibility increase. Essentially, New York adolescents from one time period are being compared with other New York adolescents from other time periods who experienced different eligibility threshold legislation. The main variation comes from the poor younger children for the early 1990s and from the higher income younger children and of older children for the late 1990s.

2.7. Trends in Insurance Coverage and First Stage Results

Figure 2.2 shows the fractions of the children with health insurance for all ages and by age group using unweighted data from 1992-2002 NHIS. As mentioned earlier, health insurance information for the years 1992-1996 and for the years 1997-2002 are for the previous month and at the time of interview, respectively.

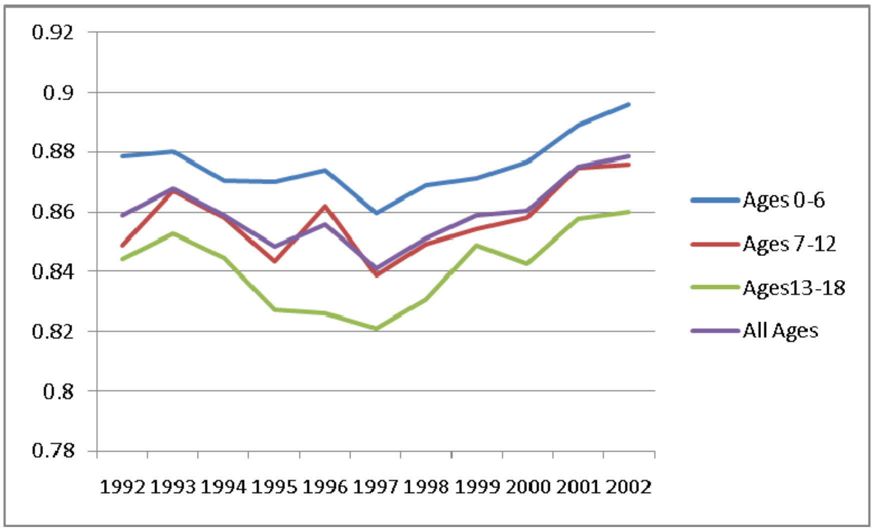

Figure 2.2 Fraction of Children with Insurance for Years 1992 to 2002 for All Children and by Age Groups

Note: Fractions of the children with health insurance for all ages and by age group using unweighted data from 1992-2002 NHIS. The sample consists of children ages 0-18 years, who live with their mother at the time of interview, and are the children of the household reference person. Health insurance information for the years 1992-1996 and for the years 1997-2002 are for the previous month and at the time of interview, respectively.

It shows that the fraction of children with any insurance decreases slightly until the mid 1990s but increases thereafter. Although the results are not shown, when public and private insurance are examined separately, in general, public health insurance coverage increased for all age groups during the time period but the increase was greatest among the youngest age group in the earlier years. This is consistent with public insurance eligibility expansions: increased Medicaid eligibility for younger children in the late 1980s to 1990s and increased SCHIP eligibility for older children in the late 1990s. Public insurance coverage drops slightly from 1996-1998 which is also consistent with previous research that found decrease in public insurance coverage due to confusions caused by welfare reform (Garrett and Holahan 2000, Kaestner and Kaushal 2003, Bitler, Gelbach and Hoynes 2004, Cawley, Schroeder and Simon 2006). During this time period, private insurance coverage experienced a gradual decrease.

Table 2.2 First Stage Results for Any Insurance

	1992-2002	1992-1996	1997-2002
Own Eligibility	-0.017	0.037	-0.142***
	(0.023)	(0.038)	(0.025)
Family Eligibility	0.090***	0.017	0.365***
	(0.022)	(0.025)	(0.051)
Observations	243021	112069	130952
R-squared	0.095	0.098	0.104

Notes: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses
Each regression includes controls for child's sex, race, mother's education, mother's age, family size, mother's marital status, family income as percent federal poverty level and its square, mother's work status and a vector of state characteristics including the seasonally-adjusted unemployment rate, real median wages, the maximum value of the federal and state EITC for a single mother with two children, the annual employment growth rate, the amount of federal housing money spent per 1,000 residents in the state, a dummy variable indicating whether the state has AFDC waiver, and a dummy indicating whether the state has TANF. The instruments used for Medicaid and SCHIP eligibilities are: 1) Simulated measure of children's own eligibility for public health insurance which is the fraction of a nationally representative sample of children who are eligible for public insurance (Medicaid or SCHIP) in a given state in a given month and year for each age; and 2) simulated measure of child's family eligibility for public health insurance is the family mean of simulated eligibilities after assigning children's simulated eligibilities that vary by state, year, month and age and adults' simulated eligibilities that vary by state, year, month, sex and whether they are a female head or not. All regressions are estimated using a linear probability model on weighted data and include state, year and child's age fixed effects. Robust standard errors are clustered by state.

In summary, during the time period when public health insurance program expanded, public insurance coverage increased but private insurance coverage decreased (suggesting a crowd out effect). Rate of insurance consequently increased over the years although there was a slight drop immediately after the welfare reform.

Table 2.2 presents the first stage results. It shows that children's own public insurance eligibility expansion has no association with children's insurance coverage except for 1997-2002 when coverage actually declines. The negative sign for the coefficient of children's own eligibility is puzzling since one would expect the expansion in children's own public insurance eligibility to increase their insurance coverage. To understand the nature of the unexpected sign on the instrument, each instrument was entered in the regression separately (Appendix Table A.1). The results indicate that both children's own and family public insurance eligibility expansions increased insurance coverage among children. The effect of family eligibility is considerably larger than that of children's own eligibility for 1997-2002. This may be a reflection of the fact that family eligibility captures children's own eligibility. Moreover, considering that 1997-2002 was the time when parental eligibility began to increase, it may also reflect parents' higher motivation to obtain children's coverage when more family members became eligible. Overall, the positive correlation between insurance coverage and each instrument provides a strong case that the unexpected signs may be due to multicollinearity between the two simulated eligibility variables. Since both instruments capture the same policies that expanded public insurance eligibility, there may have been high correlation between the two which might have led to the unexpected signs of the instrument.

To further examine the issue of unexpected signs in the first stage of the main specification, public and private insurance coverages were used as alternative outcome variables for the years 1992-2002 (Appendix Table A.2). Columns 1 and 4 show results using both instruments and columns 2, 3, 5, and 6 show results using only one instrument at a time. Column 1 shows that children's own eligibility expansions increased public insurance coverage but no effect was found for family eligibility expansions. This is perhaps again due to the multicollinearity between two instruments. In fact, when instruments are entered separately (thereby eliminating the problem of multicollinearity), the results show that both children's own and family eligibility expansions increased public insurance coverage (columns 2 and 3). The suggested take up of public insurance is around 7%, slightly lower than what previous studies have found.[21] As for employer or private insurance coverage, results from columns 4 show that expansions in children's own eligibility had a decreasing effect although an opposite effect was found for expansions in family eligibility. Negative coefficient for children's own eligibility suggests a crowd out effect that is expected. However, a statistically significant positive effect of family eligibility on employer or private insurance coverage is puzzling. Multicollinearity between children's own and family eligibility may again be the cause of the problem because when instruments are entered separately, effect of family eligibility disappears. Estimated crowd out rate is around 42% which is consistent with earlier studies.[22, 23]

Since simulated eligibility variables capture expansion in public insurance eligibility, a natural extension of the previous eligibility studies is to examine the effect of public insurance coverage instead of any insurance coverage. To examine the effect of public insurance coverage, two endogenous variables must be included in the outcome regressions (i.e. public insurance coverage and private insurance coverage) in order to use 'no coverage' as a reference category. However, this did not work because when two endogenous variables were used (i.e. public insurance coverage and private insurance coverage), the first stage F-statistics were very low. Specifically, the first stage F-statistic for public insurance was very low at 3.44 when

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[21] Previous studies have found that the take up rate for Medicaid is around 10-25% percent and 10% for SCHIP (e.g. Card & Shore-Sheppard, 2001; Cutler & Gruber, 1996; LoSasso and Buchmueller 2004, Bansak and Raphael 2007, Davidson, Blewett and Call 2004).
[22] Crowd out rate is estimated using estimates from columns 2 and 5 since they are free from multicollinearity problem.
[23] Previous studies that found crowd out rate of about 5-30% for Medicaid and 50-60% for SCHIP (Gruber and Simon 2007)

both instruments were used. It remained even lower when the time periods 1992-1996 and 1997-2002 were examined separately (2.45 and 2.36). For private insurance, the first stage F-statistic was slightly higher at 9.03. When the time periods 1992-1996 and 1997-2002 were examined separately, the F-statistic was considerably lower for 1992-1996 compared to 1997-2002 (0.06 versus 37.91). Large differences in magnitudes of the F-statistic for private insurance most likely reflect the fact that public insurance eligibility expansions did not affect privately insured children in earlier years but affected them in later years. Expansions in 1992-1996 targeted low income children who were less likely to be privately insured whereas those in 1997-2002 targeted relatively higher income young children who were more likely to be privately insured. Overall, these results suggest that the instruments are not strong enough for identification when there are two endogenous variables in the regression.

Another issue is the use of two instruments as opposed to only one of the instruments when the two instruments are correlated. Since the strength of instruments are reflected in the first stage F statistics, the best set of instruments in terms of identification is the one with the highest first stage F statistics. For regressions with any insurance coverage as a dependent variable, this would mean that the use of both instruments is preferred (first stage F-statistic=28.62) over only children's own simulated eligibility (first stage F-statistic=2.13) and only simulated family eligibility (first stage F-statistic=26.94).[24] [25] The use of both instruments to predict public insurance coverage yielded a first stage F-statistic of 2.36. This was lower than the one from the regression with only children's own eligibility (4.12) but higher than using only children's family eligibility (1.9). The stronger correlation between public insurance coverage and children's own eligibility, compared to

[24] Results are from regressions using 1997-2002 when both children's and parental eligibility expanded.

[25] First stage F-statistics for regressions with only children's own simulated eligibility and only simulated family eligibility are calculated by taking the square of the t-statistic of the coefficient on each instrument.

correlations between public insurance coverage and either family eligibility or both instruments, is expected given the more direct link between children's own public insurance eligibility and public insurance coverage. Although 1) the main identification comes from children who were initially uninsured but gained public insurance coverage after the eligibility expansions and 2) the children's own eligibility seems to be the key correlate for public insurance coverage, the main specification uses both instruments to improve first stage fit and to consequently increase the power for identification.

The last thing to note about the first stage regressions from the main specification is that simulated eligibility instruments do not have any statistically significant effect on children's insurance status when only the years 1992-1996 were used. These results reflect relatively less steep public insurance expansions during this time period compared to the major Medicaid expansions that took place before 1992 and the SCHIP implementation after 1998. Statistically insignificant results during the 1992-1996 time period indicate that simulated eligibility variables do not have a strong enough correlation with insurance status to be safely used as instruments. Consequently in this study, separate analyses with only earlier years will not be conducted.[26] Moreover, since correlation between insurance and simulated eligibilities is weak for the years 1992-1996, the instruments will be stronger when the analysis is limited to later years (1997-2002) than for the entire study period (1992-2002).

Overall, these results provide convincing evidence of instrument validity despite the unexpected signs found in the main first stage regressions in Table 2.2.

[26] First stage F statistics for regression with inconsistently measured outcomes for years 1992-1996 were below 5.

2.8. Results for All Children and by Age Groups

2.8.1. Descriptive Analyses: The Relationship between Insurance Status, Health Care Use and Health

Appendix Figures A.3 to A.8 show the change in children's health care use and health status for all ages by health insurance status. During the time period of public health insurance expansion, children's use of hospitals decreased although there was no change in doctor visits. Their general health improved and fewer children were missing school due to illness especially in the later years although there was no change in the probability of having conditions that limit activities. For most health care use and health status measures, children with and without insurance experienced similar changes.

Panels A and B in Appendix Figure A.3 show the fraction of children who had any doctor visit and the number of such visits in the past 2 weeks for the years 1997-2002. These figures do not show much change in the trends over time, but they all show that more children with insurance had contact with doctor visits than the children without insurance. There are no differences in the changes over time for children with different health insurance coverage.

Panels A and B in Appendix Figure A.4 show the fraction of children who had any short stay hospital episode and the number of such episodes in the past year for the years 1997-2002. The panels show that for those with insurance, the fraction of children who had at least one short stay hospital episode and the number of such episodes declined slowly. For those without insurance, there were also general declining pattern for both the fraction of children with at least one short stay hospital episode and the number of such days for the early 1990s. However, the decline was not very consistent throughout the years. Since hospital episodes are almost always undesirable as opposed to doctor visits which may include desirable visits such as well care visits, these are change in the positive direction. However, because these declines in hospital episodes are similar for both children with and without insurance, it is not clear whether insurance had any effect in the decline in hospital use.

Panels A and B in Appendix Figure A.5 show the fraction of children who had any short stay hospital day and the number of such days in the past year for the years 1992-1996 and for the years 1997-2002, respectively. The basic trends are similar to those for short stay hospital episodes.

Moving on to children's health outcomes, both Appendix Figures A.6 and A.7 show that the health of children remained fairly stable throughout the years. Appendix Figure A.6 shows the change in the fraction of children in excellent health. The health of children with health insurance declined slowly until 1996 and improved afterwards. While their counterparts with no insurance experience similar overall changes, the changes were not very consistent especially in the late 1990s unlike the insured children. Again with very little discernable difference in the trends, it is hard to tell whether public insurance expansion had any causal effect on children's health.

Appendix Figure A.7 shows that the proportion of children with activity limitation has not changed much over the years except for small fluctuations for both groups of children, indicating that public insurance expansions may not have had any effect on the fractions of children with activity limitations. The lack of change in children's functional limitations may be because functional limitation is a health condition that is not easily treatable with health care. Children with insurance consistently have the highest proportion with activity limitation. This may be the result of higher rates of diagnosed activity limitation due to better access to care or an evidence of adverse selection into the health insurance programs.

Panels A and B in Appendix Figure A.8 show the fraction of children who had any school days lost due to illness and the number of such days in the past year for the years 1997-2002. There is a general deceasing trend in the lost school days for children in both groups although again the decline is not very consistent for uninsured children. The overall picture seems to be that although insured children miss school more often (i.e. higher frequency of illness), when they do get sick, they miss a lesser number of school days than their uninsured counterparts (i.e. shorter duration of illness episode).

When these trends are examined by age groups, the overall patterns are similar to those observed for all children. The change in health care use seems to be greater for older age groups whereas the change in health status is greater for the youngest and the oldest age groups. From all these figures, it is difficult to tell whether there were any causal effects of insurance coverage on children's health care use and health status.

2.8.2. Regression Analyses: The Effect of Any Insurance Coverage on Health Care Use and Health

Table 2.3 presents OLS and IV results of the effect of children's health insurance coverage on their health care use and health status. First stage F statistics show that the instruments are strong with the exception of the outcomes for excellent health and activity limitation (F statistics=8.50, 8.60).[27] Although the results on these outcomes are also presented here, one must use caution when interpreting these results since they may be biased from weak instruments. If the excluded instruments are only weakly correlated with the endogenous regressors, then the IV estimates suffer from a

[27] First stage F statistics vary across outcomes in the second stage due to sample size differences due to missing data, different set of years used and different age groups in which the outcome variables were available.

weak instrument problem. When IV regression suffers from a weak instrument problem, 1) IV estimates will have larger standard errors, i.e. asymptotic variance is higher than that of an OLS estimator and it will be larger when there is lower correlation between the endogenous regressor and the instrument (asymptotic problem #1); 2) IV estimates may be inconsistent if the instrument is not entirely exogenous (asymptotic problem #2); and
3) in finite samples, IV estimates will be biased in the same direction as the OLS estimates and the magnitude of the bias increases as the correlation between the instrument and the endogenous regressor decreases (Bound, Jaeger and Baker 1995). More explanation about the weak instrument problem is available in the Appendix 2.18.

In general, when compared to IV estimate, bias in OLS estimate seems to point out that those who have health insurance are negatively selected, i.e. those who require more health care (e.g. due to worse health), are more likely to be insured. OLS estimates on all health care use outcomes except for any doctor visits in the past 2 weeks have an upward bias when compared with IV estimates; the effect of health insurance on health care use will be overstated without any correction for endogeneity. When OLS and IV estimates are compared for health outcomes, there is a downward bias on outcome measures where higher values indicate better health (e.g. excellent health) and an upward bias on those where higher values indicate worse health (e.g. activity limitation, lost school days) both of which imply that unhealthier individuals are more likely to have insurance. As expected, results show that there is adverse selection into health insurance among the sample population.

When IV estimates are examined, most effects are statistically insignificant. The point estimates suggest that insurance coverage led to an increase in doctor visits but a decrease in hospital use.

Table 2.3 OLS and IV Results for the Effect of Any Insurance Status on Children's Health care Use and Health Status

	Any Doctor Visits (past 2 wks)	# of Doctor Visits (past 2 wks)	Any Hospital Episodes (past year)	# of Hospital Episodes (past year)	Any Hospital Days (past year)	# of Hospital Days (past year)	Excellent Health	Activity Limitation	Any Lost School Days (past year)	# of Lost School Days (past year)
	1997-2002						1992-2002		1997-2002	
<u>OLS</u>										
Any Insurance	0.049***	0.063***	0.015***	0.023***	0.015***	0.147***	0.010	0.021***	0.014*	-1.261*
	(0.004)	(0.005)	(0.002)	(0.003)	(0.002)	(0.019)	(0.007)	(0.002)	(0.008)	(0.694)
<u>IV</u>										
Any Insurance	0.104	0.034	-0.095***	-0.070	-0.093***	-0.693	0.647	0.001	-0.354***	-17.927
	(0.075)	(0.108)	(0.033)	(0.059)	(0.033)	(0.656)	(0.432)	(0.109)	(0.130)	(12.612)
First Stage F stat	28.56	28.56	23.33	23.33	23.35	23.35	8.50	8.60	17.17	17.17
Observations	130806	130806	125012	125012	124997	124997	241614	243021	43788	43788

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$; Robust standard errors in parentheses

Each regression includes controls for child's sex, race, mother's education, mother's age, family size, mother's marital status, family income as percent federal poverty level and its square, mother's work status and a vector of state characteristics including the seasonally-adjusted unemployment rate, real median wages, the maximum value of the federal and state EITC for a single mother with two children, the annual employment growth rate, the amount of federal housing money spent per 1,000 residents in the state, a dummy variable indicating whether the state has AFDC waiver, and a dummy indicating whether the state has TANF. The instruments used for Medicaid and SCHIP eligibilities are: 1) Simulated measure of children's own eligibility for public health insurance which is the fraction of a nationally representative sample of children who are eligible for public insurance (Medicaid or SCHIP) in a given state in a given month and year for each age; and 2) simulated measure of child's family eligibility for public health insurance is the family mean of simulated eligibilities after assigning children's simulated eligibilities that vary by state, year, month and age and adults' simulated eligibilities that vary by state, year, month, sex and whether they are a female head or not. All regressions are estimated using a linear probability model for categorical dependent variables and an OLS for continuous dependent variables on weighted data and include state, year and child's age fixed effects. Robust standard errors are clustered by state.

They also suggest that insurance coverage improved health. Since this study uses a linear probability model, the magnitude of the effects cannot be literally interpreted (since linear probability models do not assume the values of the dependent variable to be restricted to 0-1 interval). The results do suggest however that insurance coverage led to a small statistically significant decrease in the probability of having had any hospital episodes or short overnight stays at a hospital in the past year (by approximately 10 percentage points from the linear probability model) and a fairly large decrease in the probability of having had lost school days due to illness (by approximately 35 percentage points from the linear probability model).[28]

Table 2.4 presents the results based on age groups. First stage F statistics show that the instruments are strong with the exception of outcomes for the youngest age group (F statistics ranges from 6.94 to 20.89). For comprehensiveness, results for this age group are presented here, but these results need to be interpreted carefully due to possible weak instrument bias. When IV estimates are compared with OLS estimates, results again suggest negative selection in health insurance with degree of selection comparable among all age groups. Again, many effects suggested by IV estimates are statistically insignificant. However, statistically significant results suggest that having health insurance coverage decreases the use of hospital care and improves health. The effects are greater in magnitude for younger children in general (although one should note that the instruments are quite weak for this age group). This is possibly due to the inherent greater need for health care for the younger children compared to the older children. Health insurance coverage decreases the probability of having at least one hospital day or episode by approximately 17-18 percentage points for the youngest children but only 10 percentage points for children between ages 7-12.

[28] Large difference between the OLS and IV estimates for outcome measures on lost school days is puzzling. The only possibility is that the endogeneity bias is greater in these measures than in other outcome measures, i.e. those who are insured are also those children who are a lot more likely to miss school due to some unobserved characteristics compared to the differences for other outcomes.

Table 2.4 IV Results for the Effect of Any Insurance Status on Children's Health care Use and Health Status by Age Groups

	1997-2002									
	Any Doctor Visits (past 2 wks)	# of Doctor Visits (past 2 wks)	Any Doctor Visits (past 2 wks)	# of Doctor Visits (past 2 wks)	Any Doctor Visits (past 2 wks)	# of Doctor Visits (past 2 wks)	Any Hospital Episodes (past year)	# of Hospital Episodes (past year)	Any Hospital Days (past year)	# of Hospital Days (past year)
	age 0-6		age7-12		age13-18		age 0-6			
OLS										
Any Insurance	0.059***	0.079***	0.044***	0.052***	0.045***	0.058***	0.024***	0.034***	0.024***	0.190***
	(0.006)	(0.009)	(0.005)	(0.008)	(0.004)	(0.007)	(0.003)	(0.006)	(0.003)	(0.044)
IV										
Any Insurance	0.202	0.101	-0.058	-0.198	0.095	0.102	-0.182**	-0.113	-0.173**	0.816
	(0.134)	(0.179)	(0.097)	(0.147)	(0.097)	(0.170)	(0.089)	(0.142)	(0.087)	(1.192)
First Stage F stat	17.45	17.45	18.23	18.23	20.89	20.89	8.69	8.69	8.72	8.72
Observations	46493	46493	44630	44630	39683	39683	40689	40689	40684	40684

	1997-2002							
	Any Hospital Episodes (past year)	# of Hospital Episodes (past year)	Any Hospital Days (past year)	# of Hospital Days (past year)	Any Hospital Episodes (past year)	# of Hospital Episodes (past year)	Any Hospital Days (past year)	# of Hospital Days (past year)
	age7-12				age13-18			
OLS								
Any Insurance	0.011***	0.018***	0.011***	0.090***	0.011***	0.017***	0.012***	0.161***
	(0.002)	(0.002)	(0.002)	(0.017)	(0.002)	(0.005)	(0.002)	(0.031)
IV								
Any Insurance	-0.104*	-0.116	-0.104*	-1.157	-0.025	0.012	-0.023	-1.292
	(0.055)	(0.097)	(0.055)	(0.882)	(0.054)	(0.098)	(0.054)	(1.284)
First Stage F stat	19.43	19.43	19.44	19.44	17.22	17.22	17.13	17.13
Observations	44620	44620	44616	44616	39703	39703	39697	39697

Notes: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. See notes from Table 2.3.

Table 2.4 (Continued)

	1992-2002					
	Excellent Health	Activity Limitation	Excellent Health	Activity Limitation	Excellent Health	Activity Limitation
	age 0-6		age7-12		age13-18	
<u>OLS</u>						
Any Insurance	0.004	0.018***	0.006	0.023***	0.020**	0.024***
	(0.008)	(0.002)	(0.009)	(0.003)	(0.009)	(0.005)
<u>IV</u>						
Any Insurance	1.050***	0.207*	1.305***	0.085	0.700**	-0.141
	(0.381)	(0.117)	(0.504)	(0.169)	(0.327)	(0.140)
First Stage F stat	6.94	7.40	10.20	9.96	11.24	10.98
Observations	88623	89227	81700	82157	71291	71637

	1997-2002					
	Any Lost School Days (past year)	# of Lost School Days (past year)	Any Lost School Days (past year)	# of Lost School Days (past year)	Any Lost School Days (past year)	# of Lost School Days (past year)
	age 0-6		age7-12		age13-18	
<u>OLS</u>						
Any Insurance	0.012	-0.094	0.014	0.107	0.013	-2.598**
	(0.020)	(4.044)	(0.013)	(0.532)	(0.011)	(1.185)
<u>IV</u>						
Any Insurance	0.152	-89.750	-0.510**	-11.177*	-0.477*	3.163
	(0.386)	(69.053)	(0.251)	(6.101)	(0.278)	(15.686)
First Stage F stat	7.61	7.61	14.46	14.46	9.15	9.15
Observations	6460	6460	19579	19579	17749	17749

Notes: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. See notes from Table 2.3.

The effects for the oldest children are statistically insignificant but the point estimates suggest a smaller decrease of less than 3 percentage points. Health insurance coverage also increases probability of excellent health and magnitude of the effect seems strongest for children between ages 7-12 and lowest for the oldest children. Although nothing can be concluded from statistically insignificant coefficients, directions of the signs also suggest that having health insurance increases doctor visits and improves health status with the magnitude greater for younger children.

In general, health insurance seemed to have decreased hospital use and improved health while its effect on doctor visits (both preventive and curative care) is unclear due to statistical insignificance of the estimates. Since there is no evidence of increased use of preventive care, health insurance coverage may have improved their health status through pathways other than preventive care such as improved health knowledge and behaviors (improved self management of health and illnesses). This may have led to decrease in the use of hospital care. Although this issue needs further investigation, the hypothesis of improved health behaviors due to health insurance coverage is consistent with the recent study by Courbage and de Coulon (2004).

2.9. Results for Immigrant Children

2.9.1. Descriptive Analyses for Immigrant Children: The Relationship between Insurance Status, Health Care Use and Health

Summary statistics of immigrant children are described in Table 2.5. Immigrant children have slightly less educated mothers who are more likely to be married than all children. For those who are insured, immigrant children are more likely to be covered by public insurance than private insurance at a much higher rate than other children.

Only about three quarter of immigrant children are insured; uninsurance rate for immigrant children is more than one and a half times more than those for all children. Compared to all children, immigrant children use less health care and are in poorer health. These data show that immigrant children are indeed suffering from lack of access to insurance coverage and consequently have less health care use and are in poorer health.

Table 2.6 shows health care use and health status for immigrant children by insurance status. Similar to all children, immigrant children with insurance are more likely to use health care than those without insurance. They are in better health although there are reasons to believe that the reported health for uninsured immigrant children may more likely be biased from reporting bias as mentioned earlier.

2.9.2. Regression Analyses for Immigrant Children: The Effect of Any Insurance Status on Health Care Use and Health

Table 2.7 presents OLS and IV results for the effect of children's health insurance coverage on their health care use and health status. Similar to all children, in general, comparison of both estimates suggest negative selection although some exceptions exist (e.g. immigrant children who are less likely to use preventive and outpatient curative care are more likely to be insured). IV results indicate that covering children increases doctor visits; health insurance coverage increases the probability of children having at least one doctor visit in the past 2 weeks by 17 percentage points and increases the frequency of such doctor visits by 0.155. Statistically insignificant point estimates suggest that probability of immigrant children using hospitals decreases although among the users, the frequency of usage increases once they are covered by insurance.

Table 2.5 Descriptive Tables for All Children and Immigrant Children

	All Children			Immigrants		
Variables	Mean	SD	N	Mean	SD	N
Individual Characteristics						
Female	0.488	0.500	243021	0.486	0.500	57310
NH White	0.198	0.399	243021	0.189	0.392	57310
Hispanic	0.229	0.420	243021	0.635	0.482	57310
%FPL income	260.226	178.674	243021	212.762	168.743	57310
Family size	4.438	1.409	243021	4.847	1.620	57310
Mother's education						
HS graduate	0.190	0.392	243021	0.125	0.331	57310
Some college	0.365	0.481	243021	0.265	0.441	57310
College graduate	0.258	0.438	243021	0.203	0.402	57310
Mother's age	35.927	7.311	243021	35.952	7.460	57310
Mother married	0.782	0.413	243021	0.840	0.367	57310
Mother working	0.667	0.471	243021	0.570	0.495	57310
Insurance						
Any Insurance	0.863	0.343	243021	0.749	0.433	57310
Public Insurance	0.198	0.398	243021	0.254	0.435	57310
Employer/Private Insurance	0.681	0.466	243021	0.507	0.500	57310
Health care Use						
1997-2002						
Any doctor visits (past 2 wks)	0.115	0.320	130806	0.091	0.287	34887
# doctor visits (past 2 wks)	0.145	0.471	130806	0.110	0.394	34887
Any hospital episodes (past yr)	0.060	0.237	125012	0.055	0.229	33252
# hospital episodes (past yr)	0.073	0.379	125012	0.066	0.330	33252
Any hospital days (past yr)	0.059	0.235	124997	0.054	0.227	33249
# hospital days (past yr)	0.273	3.232	124997	0.238	3.029	33249

Note: Fractions of the children with health insurance for all children and immigrant children using unweighted data from 1992-2002 NHIS. The sample for all children consists of children ages 0-18 years, who live with their mother at the time of interview, and are the children of the household reference person. Immigrant children are children who have at least one foreign born parent. Health insurance information for the years 1992-1996 and for the years 1997-2002 are for the previous month and at the time of interview, respectively.

Table 2.5 (Continued)

	All Children			Immigrants		
Variables	Mean	SD	N	Mean	SD	N
<u>**Health Status**</u>						
1992-2002						
Excellent health	0.526	0.499	241614	0.451	0.498	56969
Limitation in Activities	0.064	0.245	243021	0.041	0.199	57310
1997-2002						
Any lost school days (past yr)	0.735	0.441	43788	0.629	0.483	10649
# lost school days (past yr)	7.478	36.979	43788	6.405	35.649	10649

Note: Fractions of the children with health insurance for all children and immigrant children using unweighted data from 1992-2002 NHIS. The sample for all children consists of children ages 0-18 years, who live with their mother at the time of interview, and are the children of the household reference person. Immigrant children are children who have at least one foreign born parent. Health insurance information for the years 1992-1996 and for the years 1997-2002 are for the previous month and at the time of interview, respectively.

Table 2.6 Descriptive Table for Immigrant Children by Insurance Status

	Any Insurance			Uninsured		
	Mean	SD	N	Mean	SD	N
Health care Use						
1997-2002						
Any doctor visits (past 2 wks)	0.106	0.308	26071	0.045	0.208	8832
# doctor visits (past 2 wks)	0.130	0.428	26071	0.053	0.261	8832
Any hospital episodes (past yr)	0.050	0.219	42946	0.027	0.162	14354
# hospital episodes (past yr)	0.060	0.316	42946	0.031	0.202	14354
Any hospital days (past yr)	0.050	0.217	42915	0.026	0.160	14347
# hospital days (past yr)	0.268	3.374	42915	0.096	0.966	14347
Health Status						
1992-2002						
Excellent health	0.473	0.499	42737	0.394	0.489	14248
Limitation in Activities	0.045	0.208	42964	0.029	0.168	14362
1997-2002						
Any lost school days (past yr)	0.646	0.478	7915	0.578	0.494	2736
# lost school days (past yr)	6.496	35.635	7915	6.228	36.176	2736

The effect of any insurance on immigrant children's health status is all statistically insignificant and inconsistent. In general, the effect sizes seem to be greater for immigrant children compared to all children. This suggests that immigrant children benefited more from public insurance expansion than other children, consistent with the fact that immigrant children are one of the most uninsured groups of children in the nation.

2.10. Results for Older Adolescents

2.10.1. Descriptive Analyses: The Relationship between Insurance Status, Health Care Use and Health

As stated earlier, it is often claimed that adolescents grow out of the realm of both public and private insurance once they turn 19. Therefore they are an age group with the lowest insurance coverage in the nation.

Table 2.7 OLS and IV Results for the Effect of Any Insurance Status on Children's Health care Use and Health Status for Immigrant Children

	Any Doctor Visits (past 2 wks)	# of Doctor Visits (past 2 wks)	Any Hospital Episodes (past year)	# of Hospital Episodes (past year)	Any Hospital Days (past year)	# of Hospital Days (past year)	Excellent Health	Activity Limitation	Any Lost School Days (past year)	# of Lost School Days (past year)
	1997-2002						1992-2002		1997-2002	
<u>OLS</u>										
Any Insurance	0.045***	0.059***	0.011***	0.017***	0.012***	0.134***	0.005	0.019***	0.039***	1.265*
	(0.007)	(0.009)	(0.001)	(0.003)	(0.002)	(0.025)	(0.007)	(0.003)	(0.011)	(0.754)
<u>IV</u>										
Any Insurance	0.166***	0.155*	-0.014	0.038	-0.016	0.489	0.185	-0.029	0.239	30.448**
	(0.064)	(0.089)	(0.049)	(0.097)	(0.051)	(0.930)	(0.292)	(0.080)	(0.191)	(15.495)
1st Stage F	22.48	22.48	50.99	50.99	50.94	50.94	7.47	7.10	36.00	36.00
Observations	34887	34887	33252	33252	33249	33249	56969	57310	10649	10649

Notes: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. See notes from Table 2.3.

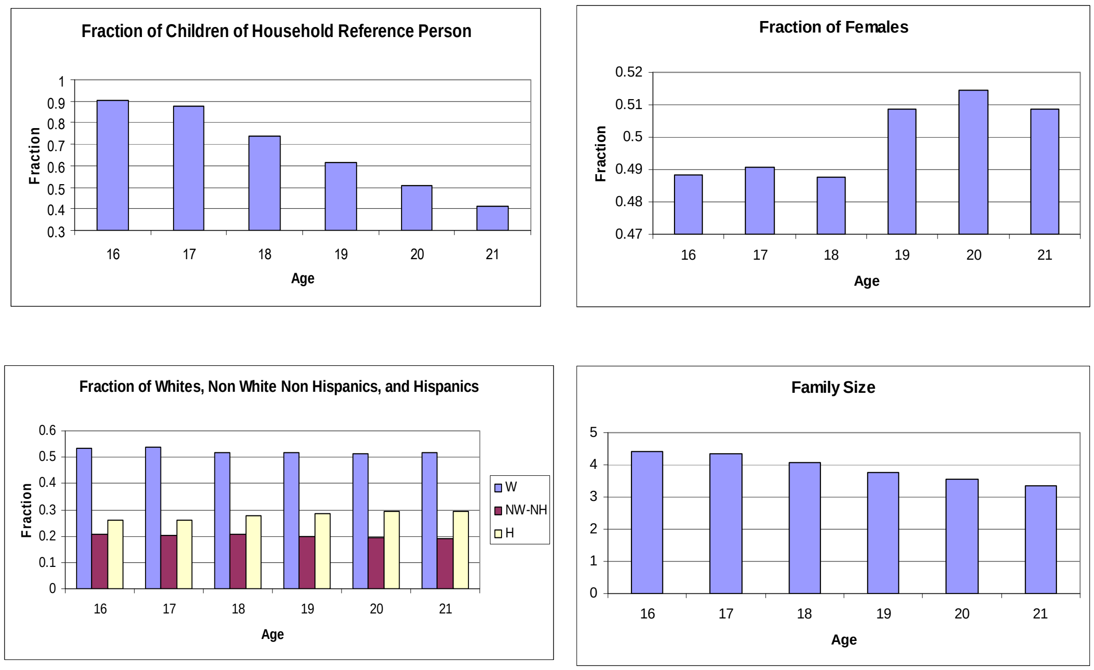
The analyses in this section explore: 1) whether a break in insurance coverage at age 19 is empirically supported by the data, 2) whether there are any other changes in characteristics that happen simultaneously at age 19, and 3) whether the same discontinuities exist even when the analysis is limited to those who are closer to the threshold, i.e. comparing adolescents who are about to turn 19 vs. those who just turned 19.

Figure 2.3 graphs characteristics of adolescents by age. Recall that the sample used in this section differs from those used in the earlier sections. Here, data comes from only years 1997-2002 and includes all adolescents ages 16-21 not limiting to those who are children of the household reference person. Graphs in Panel A, B, C, and D show selected individual characteristics, insurance health insurance coverage, health care use and health status, respectively. Panel A shows that the individual characteristics are overall similar except for a fraction of children of the household reference person.[29] As expected, the fraction of adolescents who are children of the household reference person is decreasing quite sharply. Although the gaps between the ages are quite consistent, i.e. the 18-19 gap does not seem to be any larger than other age gaps, loss of dependent status as inferred by this graph may play a big role in the drop in insurance coverage at the age of 19 which is discussed next.

Panel B shows a clear drop in health insurance coverage at 19. This drop is seen for both types of insurance. As discussed earlier, at the age of 19, most adolescents lose eligibility as dependents in their parents' insurance.

[29] Ideally one would like to know the dependent status of an adolescent and how it changes over time since one of the main reasons for the loss of coverage for older adolescents is loss of dependent status in their parents' insurance plan. However, since NHIS does not provide any information on dependent status or its closest proxy, whether an adolescent is living with his/her parents, this will be inferred here using a relationship to the reference person variable. If adolescent lives with parents, then either one of his/her parents will most likely be a household reference person and therefore the adolescent will be classified as a child of reference person. Since there may be adolescents who lives with uncles or aunts who are their household reference person (therefore adolescents are nieces or nephews) or with grandparents (in which case adolescents are grandchildren). Therefore these fractions provide a lower estimate of the dependency level of adolescents.

A. Selected Individual Characteristics



B. Health Insurance

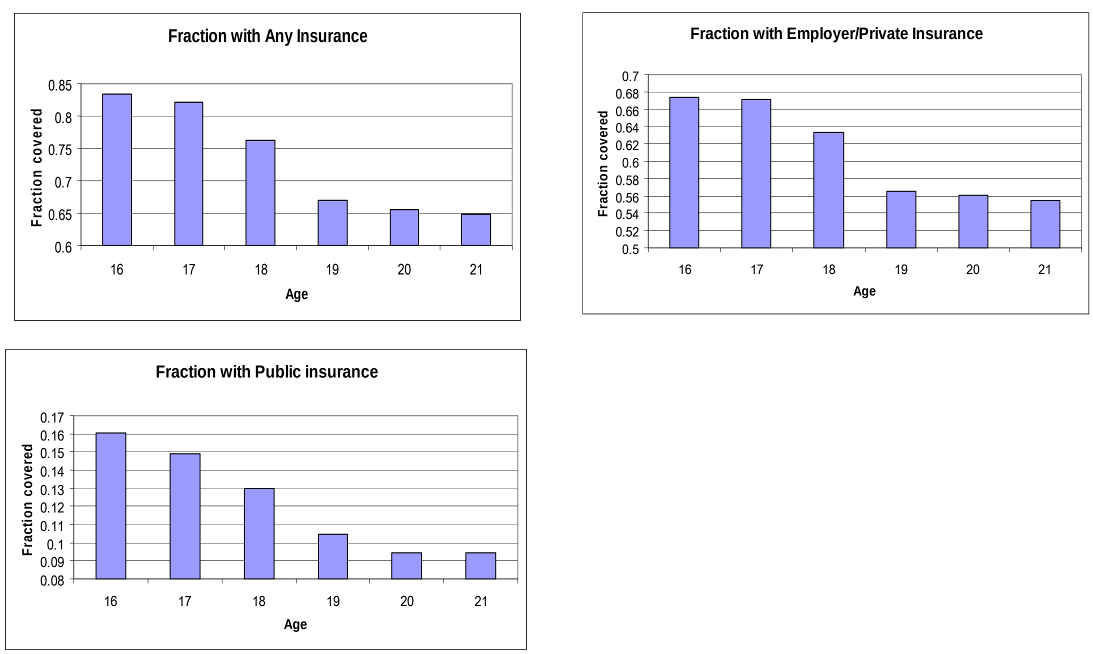


Figure 2.3 Characteristics of Adolescents Ages 16-21 by Age

Note: Author's calculations using unweighted data from 1997-2002 NHIS. The sample consists of all adolescents ages 16-21 regardless of their relationship to the reference person. Unlike the main sample, this sample does not limit to individuals who are the children of the household reference person

C. Health care Use

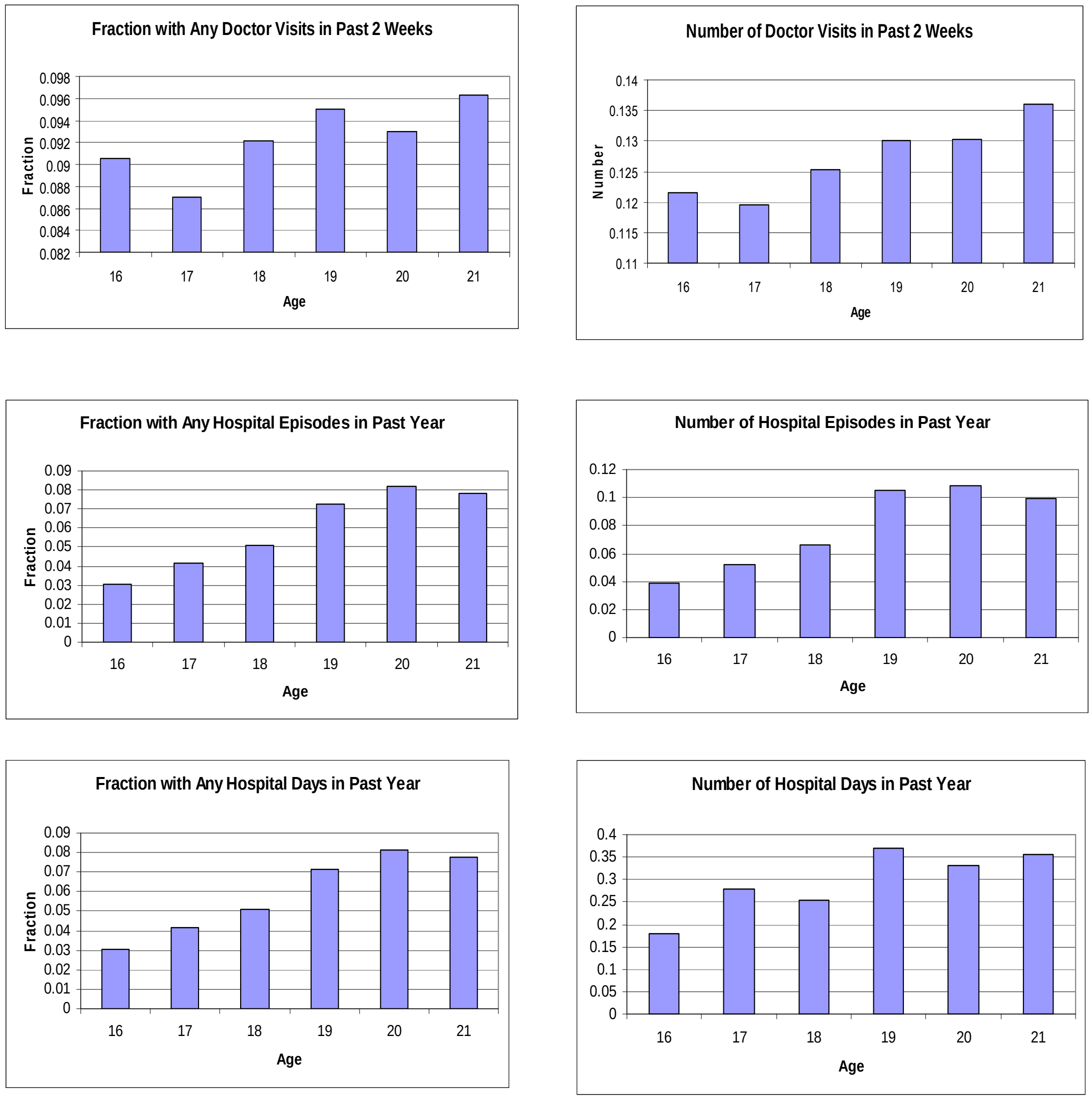
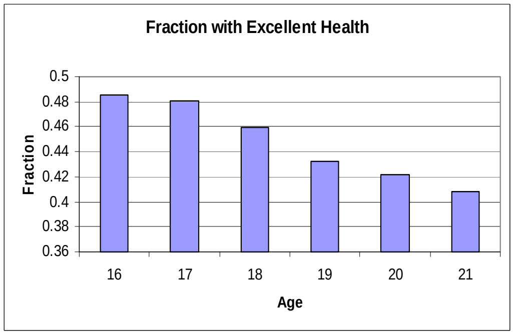


Figure 2.3 (Continued)

D. Health Status



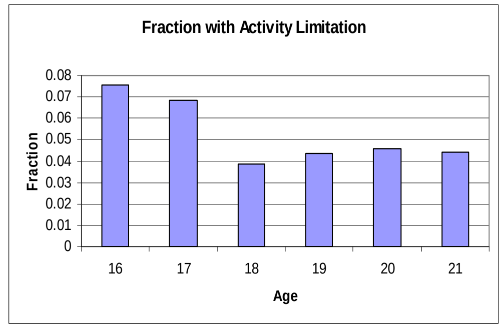


Figure 2.3 (Continued)

They also become ineligible for Medicaid and SCHIP which they once enjoyed as children. The graphs clearly reflect these changes in coverage.

Graphs in Panel C show that health care use increases after 19 for both outpatient and inpatient health care. This is contrary to what one would expect because lack of insurance may deter adolescents from using health care. Increase in health care may be a reflection of increase in unique health care needs that emerge as children age such as reproductive health care. It may also be an indication of poorer health. Trends from graphs in Panel D show a mixed picture for health. Consistent with the earlier hypothesis on increased health care use after the age of 19, adolescents' health seem to deteriorate as they age although the fraction of adolescents with activity limitation seems to drop in later ages (from age 18). Activity limitation is a health problem that requires consistent long term care and the effect only manifests in the long run. Therefore the change in health insurance coverage in this small time frame may not be sufficient to see the effects. The trends seen here may only be a spurious relationship.

It seems clear that adolescents lose insurance coverage once they turn 19 with one of the reasons being decline in dependency on their parents. To find out whether the same drop exists among those who are closer to age 19 threshold, the same exercise was done comparing adolescents who are almost 19 and those who just turned 19. This was done using the public use NHIS data. In order to classify adolescents into those who are about to turn 19 and those who have just turned 19, one must know the age dates of birth and interview. Unfortunately public use NHIS data does not have exact interview date; it only has interview quarter. If interview and birth quarters are the same, then it is not possible to find out whether the adolescent's birthday fell before or after the interview date without the exact dates of birth and interview.[30] Instead, quarter was the unit used to classify adolescents according to their proximity to the cutoff. Adolescents were classified as "cutoff+1" if their birth quarter is one quarter after the interviewer quarter. Similary, adolescents were classified as "cutoff+2" if their birth quarter is two quarters after the interview quarter. The same logic was used to define "cutoff-1" and "cutoff-2". Although the results are not shown due to space limitation, the findings from this exercise were consistent with the earlier results: 1) fractions of dependent adolescents and insurance coverage drops as soon as they turn 19 and this is likely to be due to the drop in fraction of children of the household reference person that happen at the age of 19 while all other individual characteristics remain consistent; and 2) there seems to be a slight increase in health care use (both outpatient and inpatient) while the results for health are inconsistent.[31]

If one can safely assume that observed and unobserved determinants of health insurance coverage, health care use, and health are consistent across age 19 threshold, then the sharp discontinuities in health insurance gap at 19 can be used to identify the causal effect of health insurance coverage on health care use and health. This is called regression discontinuity method (RD). The identification of RD comes from the fact

[30] Since this is the group of adolescents one is potentially most interested in due to their proximity to the cutoff, the amount of information one could infer from these exploratory exercise were compromised due to the use of public use data.
[31] The results are available upon request.

that current health insurance policies (definition of a dependent in private health insurance policies) and laws (Medicaid and SCHIP regulations) produce sharp difference in health insurance coverage among older adolescents on either side of age 19 threshold. While previous graphical analyses showed that most mean individual characteristics were smooth before and after the age 19 threshold, there was a clear discontinuity in the fraction of children of household reference person. This discontinuity is a threat to the methodology since it may be correlated with not only insurance status but also health care use and health. For example, adolescents who still live with parents (i.e. children of the household reference person) may more likely use health care and be in better health because they are in good parental custody. On the other hand, adolescents who live independently (i.e. not children of the household reference person) may more likely use health care and be in better health if they are responsible adolescents who take care of their health better than their counterparts who stay back in their parents' homes. In either case, both health insurance status and living arrangements change drastically at age 19 which independently affect health care use and health. Therefore it will be difficult to separate out the effect of one from the other making RD not a feasible method to examine the causal effect of health insurance.

2.11. Other Methods

Some studies use methodologies other than IV to examine the effect of public insurance eligibility or coverage including difference in differences (e.g. Banthin and Selden 2003, Blumberg et al 2000, Dubay and Kenney 2003) and regression discontinuity methods (e.g. Card and Shore-Sheppard 2001). Here difference in difference method is used to examine robustness of the main results. For the 1992-1996 time period, the effect of Medicaid expansion on health care use and health status is examined using younger children since they were the targeted population for the expansion. For the 1997-2002 time period, the effect of SCHIP on health care use and health status is examined using older adolescents since they experienced the greatest change in public insurance eligibility. Treatment and control groups are constructed using the discontinuities in coverage by age and family income. The regressions control for individual and state characteristics as well as state and year fixed effects.

Following sets of treatment and control groups are used: 1) for 1992-1996 time period, children between ages 0-6 who have family incomes of 100-200% FPL (>=100%FPL & <200%FPL) vs. children between ages 0-6 who have family incomes 200-300% FPL; and 2) for 1997-2002 time period, older adolescents between ages 15-18 who have family incomes of 0-200% FPL vs. young adults between ages 19-20 who have family incomes of 0-200% FPL. For former analyses, years 1995-1996 and 1992 are used for post and pre years, respectively. For latter analyses, years 2000-2002 and 1997-1998 are used for post and pre years, respectively.

During the 1992-1996 time period, there were only four states with Medicaid eligibility at or above 200% FPL (U.S. Department of Health and Human Services, Centers for Medicare and Medicaid, Centers for Medicaid and State Operations referenced in Owcharenko 2006). Therefore if the trends in health care use and health status of young children with family incomes of 100-200% FPL are similar to those with family incomes of 200-300% FPL in the absence of the Medicaid expansions, then the difference in the changes may be attributed to the change in expansions. The misclassification of children living in the four states will produce conservative estimates of the effect of Medicaid expansion since the bias should move the estimates towards zero.

Table 2.8 shows the results. As expected, insurance coverage, more specifically public insurance coverage, increased during this time period. Employer and private insurance coverage did not experience any statistically significant change.[32] Despite the expected effect on insurance coverage, there were no detectable effects on children's health care use or health. In general, statistically insignificant coefficients are in line with the earlier findings for children between ages 0-6 that indicated a decrease in hospital use and inconsistent effect on health.

As mentioned earlier in the study, due to the SCHIP implementation, older children experienced the greatest expansion in public insurance eligibility in the late 1990s. In 1997, eligibility thresholds were as low as 15% FPL in some states. By 2001, however, many states set their eligibility thresholds at 200% FPL and some even set it at more than 300% FPL (Morreale and English 2003). Since children grow out of public insurance once they reach age 19, adolescents between ages 19-20 did not benefit from this public insurance eligibility increase. Therefore, if the trends in health care use and health status of older adolescents between ages 15-18 are similar to young adults between ages 19-20 in the absence of the SCHIP implementation, then the difference in the changes may be attributed to SCHIP implementation.

Table 2.9 shows the results. Contrary to expectations, differences in difference results suggest no effect on insurance coverage. Signs on coefficients on any insurance and public insurance are also puzzling since they suggest a decrease in coverage albeit their statistical insignificance. Most effects on health care use and health are also statistically insignificant. The only effects that are detected are a decrease in the number of hospital days and an increase in excellent health.

[32] Although statistically insignificant, sign on the coefficient is negative consistent with the expectations.

Table 2.8 Difference in Difference Results for Children Ages 0-6 for Years 1992-1996

	Any Insurance	Public Insurance	Employer / Private Insurance	Any Doctor Visits (past 2 wks)	# of Doctor Visits (past 2 wks)	Any Hospital Episodes (past year)	# of Hospital Episodes (past year)	Any Hospital Days (past year)	# of Hospital Days (past year)	Excellent Health	Activity Limitation
Treatment	-0.101***	0.092***	-0.183***	-0.004	0.001	0.013**	0.018**	0.013**	0.112	-0.060**	0.034***
	(0.015)	(0.012)	(0.023)	(0.010)	(0.234)	(0.006)	(0.009)	(0.006)	(0.071)	(0.030)	(0.009)
Post	-0.075	0.000	0.000	0.000	0.000	0.041	0.102	0.041	0.560	-0.032	0.000
	(0.080)	(0.000)	(0.000)	(0.000)	(0.000)	(0.087)	(0.108)	(0.087)	(1.358)	(0.195)	(0.000)
Treatment * Post	0.041***	0.048***	-0.001	-0.011	0.313	-0.011	-0.015	-0.011	0.014	-0.054	0.002
	(0.014)	(0.014)	(0.023)	(0.015)	(0.262)	(0.010)	(0.013)	(0.010)	(0.085)	(0.047)	(0.012)
Observations	30628	30639	30620	12407	12407	12482	12482	12482	12482	3736	3775
R-squared	0.067	0.158	0.188	0.043	0.026	0.021	0.019	0.021	0.014	0.074	0.041

Notes: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. Each regression includes controls for child's sex, race, mother's education, mother's age, family size, mother's marital status, mother's work status and a vector of state characteristics including the seasonally-adjusted unemployment rate, real median wages, the maximum value of the federal and state EITC for a single mother with two children, the annual employment growth rate, the amount of federal housing money spent per 1,000 residents in the state, a dummy variable indicating whether the state has AFDC waiver, and a dummy indicating whether the state has TANF. Treatment group is defined as children ages 0-6 who have family incomes of 100-200% FPL (>=100%FPL & <200%FPL) and control group is defined as children ages 0-6 who have family incomes 200-300% FPL. Post years include years 1995-1996 and pre year include year 1992. All regressions are estimated using a linear probability model for categorical dependent variables and an OLS for continuous dependent variables on weighted data and include state and year fixed effects. Robust standard errors are clustered by state.

Although these effects are consistent with earlier findings for the oldest age group (ages 13-18), results should be interpreted with caution due to lack of statistical significance (and puzzling signs) on insurance coverage.

Overall findings from difference in difference method are consistent with the main results. Effects of eligibility expansions on children's health insurance coverage for younger children in the years 1992-1996 confirm appropriateness of the method providing confidence in the findings on children's health care use and health. However, the effects for older children in the years 1997-2002 are perplexing and therefore findings for their health care use and health are less likely to be reliable.

Unexpected effects seen on insurance coverage of older adolescents may indicate a violation of the main assumption of difference in difference method. In order for the method to be valid, difference in outcomes between treatment and control groups (older adolescents between ages 15-18 who have family incomes of 0-200% FPL and young adults between ages 19-20 who have family incomes of 0-200% FPL) must remain constant in absence of the treatment. However, there may have been many differences between two groups that may have produced diverging trajectories over time for two groups. For example, Section 2.10 highlighted that dependency on parents change drastically at the age of 19. This, as well as any other unobserved differences between two age groups, may be the reasons why the difference in difference method failed to work for older adolescents.

2.12. Discussion

The lack of strong evidence of the effect of health insurance coverage (public or private) on children’s health care use and health status is consistent with the previous studies (e.g. Kaestner, Joyce and Racine 1999, Currie, Decker and Lin 2007). Unlike some of the previous studies, however, main specification from this study did not provide any strong evidence of the effect of health insurance coverage on children's preventive care use for 1997-2002. For example, Currie, Decker and Lin (2007) found that public insurance eligibility decreases the probability of children going without any doctor visits in the past year by 6.8 percentage points for 1986-2005 time period. Currie and Gruber (1996) found that public insurance eligibility decreases the probability of children going without any doctor visits in the past year by 9.6 percentage points for the 1984-1992 time period. Wang et al (2007a) found that public insurance coverage increases the probability of children having at least one general doctor visit by 25.7 percentage points for the 1997-2002 time period.[33] The difference in the findings may be due to the difference in the specific research question (eligibility vs. coverage), sample size, sample definition and specific variables used for the analysis.

Currie, Decker and Lin (2007) examined the effect of public insurance eligibility (not coverage) on health care use. As mentioned in the previous studies, since only a small portion of children who are made eligible through Medicaid and SCHIP expansions took up public health insurance, the effect of eligibility should be weaker than the effect of insurance coverage on health care use. Although the finding from this study is statistically insignificant, the point estimate indeed shows that the effect of insurance coverage is greater (9.9 percentage point, insignificant) than the effect of eligibility on preventive care use found in Currie, Decker and Lin (2007)'s study. The larger estimate of the eligibility found in Currie and Gruber (1996)'s study may be driven by the time period of their study. They used years when the youngest children were most affected, an age group that seems to be most responsive to the eligibility expansions.

[33] They also found an increase in the probability of children having at least one specialty doctor visit in the past year by 8.8 percentage points

Table 2.9 Difference in Difference Results for Children Ages 15-20 for Years 1997-2002

	Any Insurance	Public Insurance	Employer / Private Insurance	Any Doctor Visits (past 2 wks)	# of Doctor Visits (past 2 wks)	Any Hospital Episodes (past year)	# of Hospital Episodes (past year)	Any Hospital Days (past year)	# of Hospital Days (past year)	Excellent Health	Activity Limitation
Treatment	0.194***	0.147***	0.047*	0.006	-0.005	-0.033**	-0.024	-0.026*	0.038	-0.009	0.040***
	(0.021)	(0.021)	(0.023)	(0.016)	(0.026)	(0.015)	(0.019)	(0.014)	(0.090)	(0.029)	(0.013)
Post	0.095	0.169***	-0.064	-0.041	-0.027	0.000	0.000	-0.011	0.040	0.000	-0.003
	(0.058)	(0.062)	(0.059)	(0.025)	(0.042)	(0.000)	(0.000)	(0.025)	(0.223)	(0.000)	(0.034)
Treatment * Post	-0.023	-0.020	-0.005	0.027	0.049	0.017	-0.024	0.010	-0.264*	0.073*	-0.017
	(0.036)	(0.035)	(0.029)	(0.017)	(0.032)	(0.020)	(0.032)	(0.019)	(0.147)	(0.041)	(0.018)
Observations	8160	8160	8160	8186	8186	7992	7992	7988	7988	8183	8200
R-squared	0.113	0.169	0.191	0.042	0.035	0.026	0.020	0.025	0.012	0.047	0.036

Notes: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. Each regression includes controls for child's sex, race, mother's education, mother's age, family size, mother's marital status, mother's work status and a vector of state characteristics including the seasonally-adjusted unemployment rate, real median wages, the maximum value of the federal and state EITC for a single mother with two children, the annual employment growth rate, the amount of federal housing money spent per 1,000 residents in the state, a dummy variable indicating whether the state has AFDC waiver, and a dummy indicating whether the state has TANF. Treatment group is defined as adolescents ages 15-18 who have family incomes of 0-200% FPL and control group is defined as young adults age 19-20 who have family incomes of 0-200% FPL. Post years include years 2000-2002 and pre years include years 1997-1998. All regressions are estimated using a linear probability model for categorical dependent variables and an OLS for continuous dependent variables on weighted data and include state and year fixed effects. Robust standard errors are clustered by state.

The statistical insignificance in this study may be due to its smaller sample size compared to these other studies. Studies by Currie and Gruber (1996) and Currie, Decker and Lin (2007) have sample sizes of almost two to three times as those for this study.

The finding for the effect of insurance coverage on preventive care use was slightly different in this study compared to the finding by Wang et al (2007a) when the point estimates were compared. This study found a statistically insignificant 9.9 percentage point increase in preventive care use whereas Wang et al (2007a) found a statistically significant 25.7 percentage point increase. The main reason may be the differences in the sample definitions. The sample used by Wang et al (2007a) only included low income children with family incomes that are below state SCHIP eligibility limits. This study does not limit itself to those from low income families. Low income children are more likely to have had poorer access to care than the average children. Therefore they must have increased health care, especially preventive care, after obtaining insurance coverage. Moreover, if there is an endogenous sample selection bias introduced by limiting the sample to low income children, then the bias would be in the upward direction consistent with the differences seen in two studies (e.g. families with children who have higher health care needs may make sure their income does not exceed the eligibility threshold for public insurance). The differences in statistical significance may be due to the differences in the question for which outcome measure is based. As mentioned in the earlier section, they based their outcome on doctor visits on the questions found in the Sample Child file that had considerably higher frequency of doctor visits compared to the ones from the person file used in this study. The frequency of doctor visits is higher in the Sample Child file because this is the measure for the past year whereas the information from the person file is for the past 2 weeks.[34] Magnitudes and statistical significance of the effects on doctor visits may perhaps be due to these differences in definitions of the outcome measures.

Currie and Gruber (1996) also found that public insurance eligibility increased the probability of children with hospitalizations in the past year by 4 percentage points. The estimates from this study suggested that the insurance coverage due to public insurance program expansions in the late 1990s actually decreased the probability of hospital use by 9 percentage points. These results are consistent with the low take up of public insurance making the effect of eligibility much lower than the coverage. However, this may also be a suggestive evidence that compared to the late 1980s to early 1990s, the care delivered to publicly insured children have become more efficient by reducing the use of more costly care at hospitals compared to private doctors and clinics.

As mentioned earlier, Currie (1999) found that Medicaid eligibility led to a large increase in the preventive care use among the immigrant children. She found that Medicaid eligibility decreased the probability of children without having any doctor visit in the past year by about 35 percentage points (more than 150% of the sample mean). In this study, there was a strong effect of insurance coverage on children's use of preventive care in the late 1990s when SCHIP was implemented (i.e. 17 percentage point increase in the probability of children having at least one doctor visit in the past 2 weeks which is about 150% of the sample mean). This magnitude is comparable to the magnitude found for the earlier time period by Currie (1999). Since this study estimates the effect of insurance coverage and not eligibility like Currie (1999)'s study, the findings from this study imply that the magnitude of the effect of

[34] This observation is consistent from the summary statistics from this study.

SCHIP on health care use were greater than those for Medicaid expansions in the early 1990s.

Graphical evidence of older adolescents' drop in health insurance coverage occurring at the same time as the increase in their health care use suggests a relationship that is contrary to what previous correlational studies have found (e.g. Callahan and Cooper 2005, McManus, Greaney and Newacheck 1989). For example, these correlational studies have found that uninsured older adolescents (ages 19-24) had 1.5-2.5 times the adjusted risk of reporting no doctor visits in the past year when compared to the privately insured counterparts (Callahan and Cooper 2005). Those who used less health care or reported to be in poor health were much more likely to be uninsured than those who used health care more often and reported to be in excellent health (McManus, Greaney and Newacheck 1989). Older adolescents who did not have any hospital episodes in the past year were also found to be 27% more likely to be uninsured than those who had at least one hospital episode in the past year (McManus, Greaney and Newacheck 1989). The difference between the relationships suggested by this study and previous studies may be another indication of discontinuities in major characteristics at the age of 19 that have not been examined in this study. Since estimates of earlier studies are also biased due to the failure to address endogeneity of health insurance, it is not completely clear which relationship is the true one. However, this discrepancy definitely calls for further investigation using methods that correct for endogeneity of health insurance.

2.13. Conclusions and Implications for Future Research

This study examined the effect of Medicaid expansion and SCHIP implementation on children's use of health care and health in the US by using the 1992-2002 National Health Interview Survey. It analyzed how insurance coverage affected children's health care use and health status. Using the variation in the generosity of public insurance eligibilities by state over time, an instrumental variables model was estimated where the simulated fractions of children and family eligibilities for public insurance were used as instruments for health insurance coverage. The effects were estimated separately for different age groups and for the immigrant children and older adolescents.

For all children, the insurance coverage decreased hospital use and improved health status. The effects seemed to be greater in magnitude for the younger children. For immigrant children, insurance coverage increased doctor visits (preventive and curative care) but there was no effect on health status. The magnitude of the effects seemed to be greater for immigrant children compared to all children. For older adolescents, exploratory graphical analyses suggested that at the age of 19, health insurance coverage dropped drastically (for all coverage as well as by types) but health care use increased for both outpatient and inpatient services. The findings for health were inconsistent. However, because of a clear discontinuity in living arrangements that simultaneously occurred at 19 and its independent correlation with older adolescents' health care use and health, the suggested relationship was most likely biased and the bias would not be corrected using a regression discontinuity method.

The findings from this study are consistent with the conventional hypothesis that the health insurance increases health care use especially preventive care. As Kaestner, Joyce and Racine (1999) mention in their paper, the health status measures that are used in this study may not be relevant measures that adequately capture the effect of Medicaid expansion and SCHIP implementation. Maternal report of children's general health status may not be an accurate measure of children's health. Maternal report of whether the child has any activity limitation may not be a health condition that may easily be cured through health care use that health insurance finances. A measure that is more objective and which reflects the health conditions that are altered by the changes in health care use may have more accurately captured the effect of public insurance. The results for the lost school days due to illness offer a weak indication that insurance coverage may have improved children's health.

Despite the limitations, in general, there is a weak evidence that health insurance increased children's use of preventive and curative care but decreased intensive and emergency care at hospitals and, improved health status. The findings also weakly suggest that the benefit was larger for immigrant children, one of the highest uninsured groups of children in the nation. From this study, although there is a clear drop in insurance coverage at the age of 19, its effect on health care use and health is not clear. These results are informative in light of the increasing health care costs and the recent trend of expanding coverage to the children who are left behind.

APPENDIX

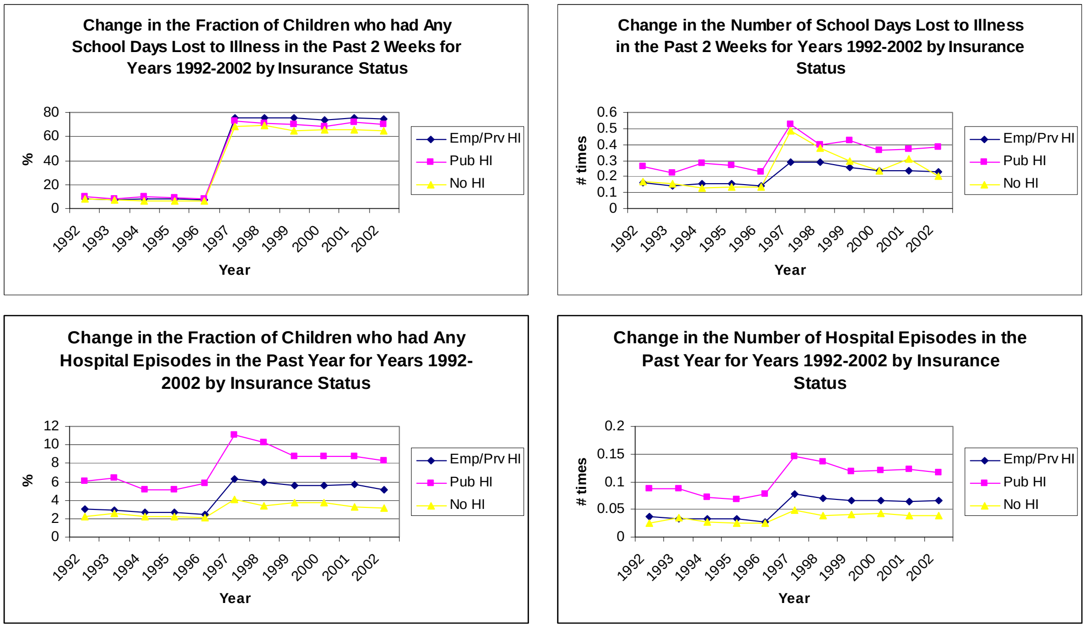
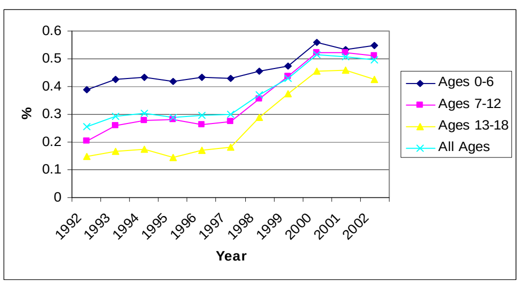
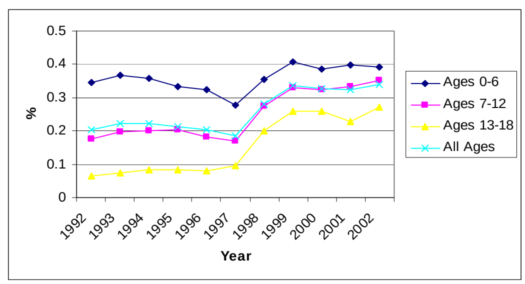


Figure A.1 Selected Outcome Variables for Years 1992-2002

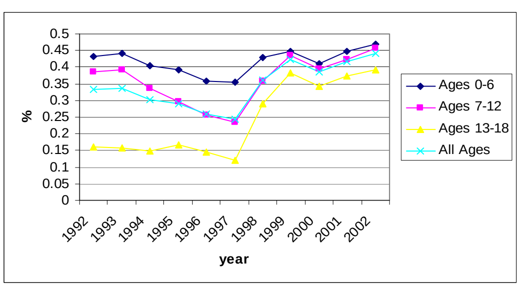
Note: Author's calculations using unweighted data from 1992-2002 NHIS.



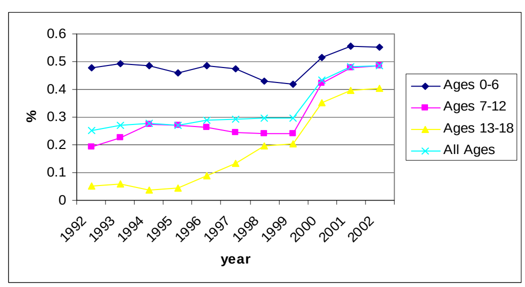
A.



B. Illinois



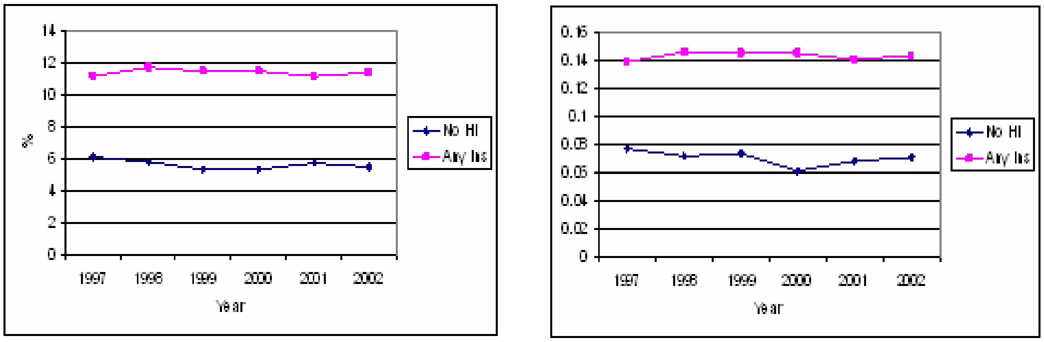
C. New York



D. Texas

Figure A.2 Change in Simulated Fraction of Medicaid or SCHIP Eligible Children for years 1992-2002 for All Ages and by Age Group

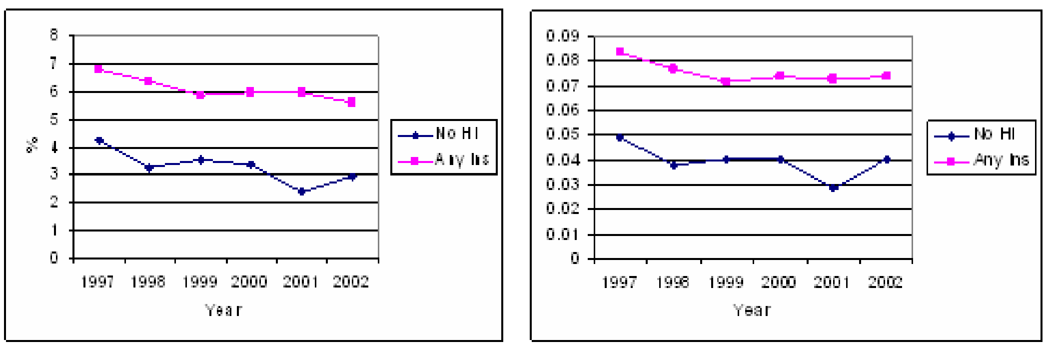
Note: Author's calculations using unweighted data from 1992-2002 NHIS.



A. Any DV Past 2 Weeks **B. # of DV Past 2 Weeks**

Figure A.3 Percentage of Children who had Any Doctor Visits (DV) and Number of Such Illness in the Past 2 Weeks for 1997-2002 for All Children and by Health Insurance Status

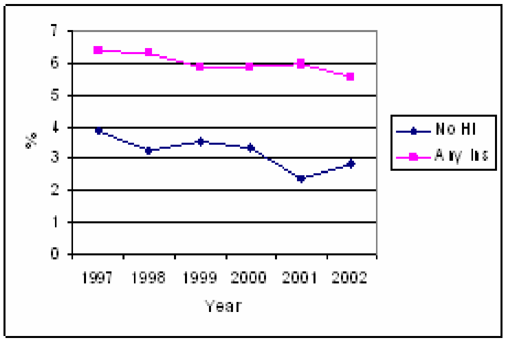
Note: Author's calculations using unweighted data from 1997-2002 NHIS.



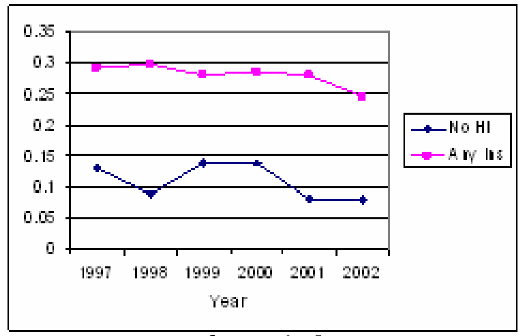
A. Any Hospital Episodes Past Year **B. # of Hospital Episodes Past Year**

Figure A.4 Percentage of Children who had Any Short Stay Hospital Episodes and Number of Such Episodes in the Past Year for Years 1997-2002 for All Children and by Health Insurance Status

Note: Author's calculations using unweighted data from 1997-2002 NHIS.



A. Any Hospital Days Past Year



B. # of Hospital Days Past Year

Figure A.5 Percentage of Children who had Any Short Stay Hospital Days and Number of Such Days in the Past Year for 1997-2002 for All Children and by Health Insurance Status

Note: Author's calculations using unweighted data from 1997-2002 NHIS.

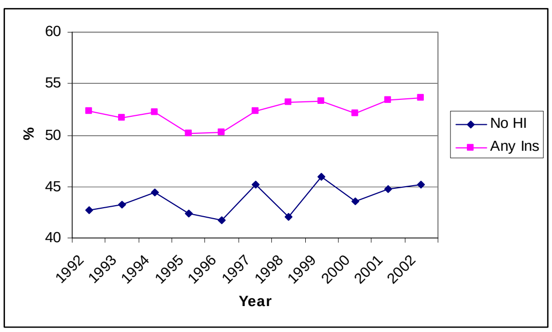


Figure A.6 Change in the Fraction of Children in Excellent Health for Years 1992-2002 for All Children and by Health Insurance Status

Note: Author's calculations using unweighted data from 1992-2002 NHIS.

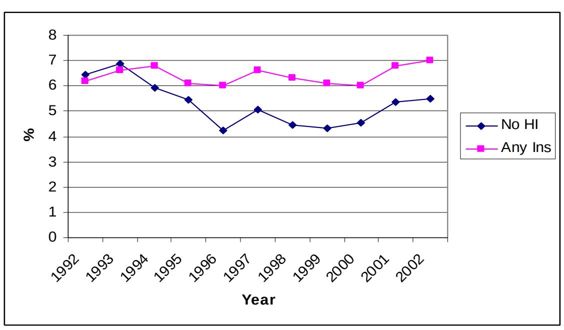
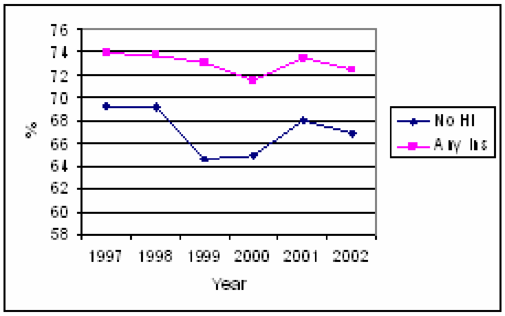
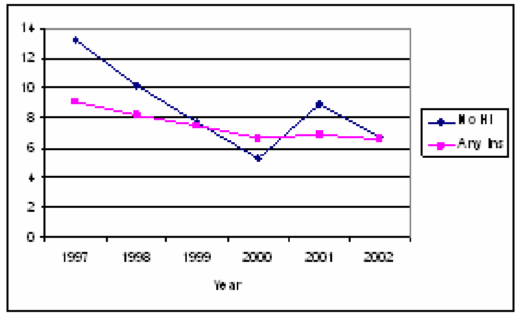


Figure A.7 Change in the Fraction of Children with Any Activity Limitation for Years 1992-2002 for All Children and by Health Insurance Status

Note: Author's calculations using unweighted data from 1992-2002 NHIS.



A. Any Days Past Year



B. # of Days Past Year

Figure A.8 Percentage of Children who had Any School Days Lost to Illness and Number of Such Days in the Past Year for Years 1997-2002 for All Children and by Health Insurance Status
Note: Author's calculations using unweighted data from 1997-2002 NHIS.

Table A.1 First Stage Results for Any Insurance When Instruments Are Entered Separately for 1992-2002 and 1997-2002

	Any (Own eligibility)		Any (Family eligibility)	
	1992-2002	1997-2002	1992-2002	1997-2002
Own Eligibility	0.040**	0.031		
	(0.019)	(0.021)		
Family Eligibility			0.078***	0.246***
			(0.020)	(0.047)
Observations	243021	130952	243021	130952
R-squared	0.098	0.103	0.098	0.105

Notes: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses
Column 1 and 2 use instruments individually with any insurance coverage as a dependent variable. Column 3 uses employer/private insurance as a dependent variable. Column 3 uses public insurance as a dependent variable. Regressions include all control variables from the main specification. See notes from Table 2.2.

Table A.2 First Stage Results for Public and Employer or Private Health Insurance for 1992-2002

	Public			Employer / Private		
	(1)	(2)	(3)	(4)	(5)	(6)
Own Eligibility	0.069**	0.071**		-0.078***	-0.030*	
	(0.027)	(0.027)		(0.020)	(0.018)	
Family Eligibility	0.002		0.049**	0.076***		0.024
	(0.017)		(0.024)	(0.022)		(0.021)
Observations	242768	242768	242768	242969	242969	242969
R-squared	0.366	0.366	0.365	0.417	0.416	0.416

Notes: *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses
Column 1 and 2 use instruments individually with any insurance coverage as a dependent variable. Column 3 uses employer/private insurance as a dependent variable. Column 3 uses public insurance as a dependent variable. Regressions include all control variables from the main specification. See notes from Table 2.2.

2.14. More on Identification

Insurance status captures many characteristics about a person including income (most likely in a nonlinear way), unobserved health care needs and health status that cannot be necessarily controlled for in a regression. Therefore ordinary least squares estimates of the effect of insurance on health care use and health will be biased. Without correction for endogeneity of insurance, coefficient on insurance will capture both true causal effect of insurance and effects of other variables that are correlated with insurance but not included in the regression. Instrumental variable approach is one way to address this issue. In an instrumental variable approach, one needs to choose an instrument that is correlated with the endogenous variable but not with outcome variable (i.e. error term in outcome regression). Intuitively, one predicts the change in insurance status caused by an exogenous variation in the instrument and examines how this change affected the outcome. Simulated measure of public health insurance eligibility is an instrument for insurance that has been used extensively in previous studies. For example, a child's insurance is instrumented by their simulated public health insurance eligibility measure defined as the fraction of a nationally representative sample of children who are eligible for public insurance (Medicaid or SCHIP) in a given state in a given month and year for each age often calculated from nationally representative datasets such as CPS.

As mentioned in the main text, this study uses these simulated eligibility measures as instruments to address endogeneity of insurance. However, unlike many earlier studies that used simulated public insurance eligibilities as instruments for *public insurance eligibility* (e.g. Currie and Gruber 1996a, Dafny and Gruber 2000, 2005, Currie, Decker and Lin 2008), this study uses the same instruments for *insurance coverage* (i.e. have insurance or not). This section provides detailed explanations on the methodologies used in the studies.

Both the previous studies and this study use the same simulated public insurance eligibility instruments. Therefore identification for both studies comes from variation in public insurance eligibility expansions that differed in magnitudes and by child's age across states over time. For both studies, children who took up public coverage after eligibility expansions provide main identification for the analyses. While the source of identification is the same, instrumented endogenous variables are different. Whereas previous studies used public insurance eligibility, this study uses any insurance coverage. If one wants to examine the effect of insurance coverage instead of eligibility using simulated public insurance eligibility expansions as instruments, a logical extension may be to examine the effect of public insurance coverage after controlling for public insurance coverage instead of any coverage as it is done in this study. This study does not examine the effect of insurance coverage by types of insurance because, as mentioned in the main text, first stage F statistics were not very high when the effect of public insurance coverage was examined. Low F statistics may have been due to the use of two instruments that are highly collinear when there are two endogenous variables. Since there are two endogenous variables (public and private insurance coverage), minimum of two instruments are necessary for the regression to be identified. However because both instruments (simulated eligibilities for child's own public insurance and child's family's public insurance) capture the same policies that expanded public insurance eligibility, there might have been a high correlation between the instruments which might have led to inadequate power of the instruments to identify the regression.

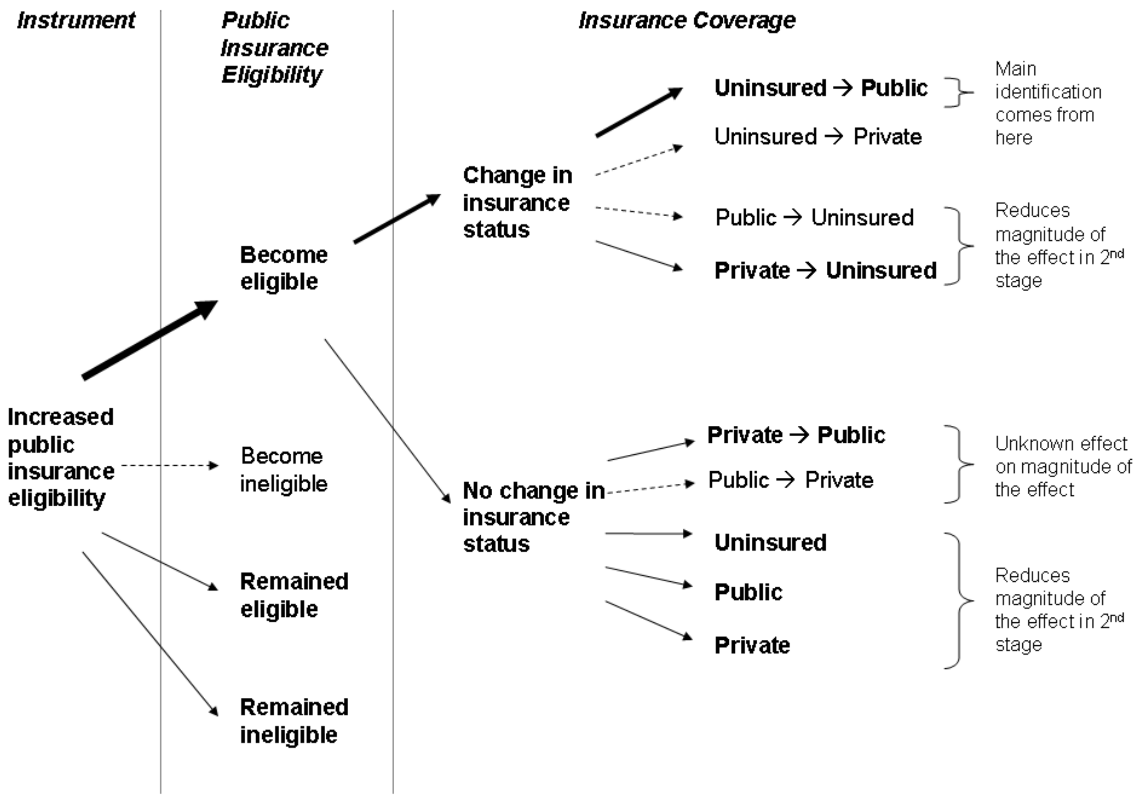


Figure A.9 The Effect of Public Insurance Eligibility Expansions on Children's Health Insurance Status in Eligibility Studies

Previous studies like Currie and Gruber (1996a) examined the effect of public insurance eligibility on children's health care use and health. Identification comes from children who became eligible for public insurance due to eligibility expansions. As shown in Figure A.9, children who became eligible consists of different groups in terms of coverage; they include children who changed their health insurance status (uninsured to publicly insured, uninsured to privately insured, privately insured to publicly insured, publicly insured to privately insured, publicly insured to uninsured, privately insured to uninsured), and those who did not change their health insurance status (remained uninsured, publicly insured, or privately insured). Essentially the comparison is between children who are eligible (driven by those who newly became eligible) and those who are ineligible. Both groups include different types of children in terms of their health insurance coverage status.

Close examination of insurance coverage is informative in determining the source of main identification and magnitudes of eligibility effects. Even though there are six combinations of changes in insurance that children may have experienced, there are no theoretical explanations for changes from uninsured to privately insured, from publicly insured to privately insured, and from publicly insured to uninsured. For example, there is no reason to believe that uninsured children would get private coverage when they become newly eligible for public insurance. Therefore any relationship that seems to exist is most likely to be spurious and not causal. This leaves three plausible changes in insurance that may have resulted from eligibility expansions: from uninsured to publicly insured, from privately insured to publicly insured, and from privately insured to uninsured. First set of children are those who took up public insurance coverage after they became eligible. Estimates of the effect of eligibility are mainly driven by these children. Second set of children are those who switched from private to public coverage, essentially capturing crowd out effect. The effect that crowd out children have on estimates are ambiguous because it depends on the extent of coverage of private and public insurance that children had originally and newly acquired, respectively. If private insurance was more extensive than newly acquired public insurance, then children would have most likely used less health care after obtaining public insurance.

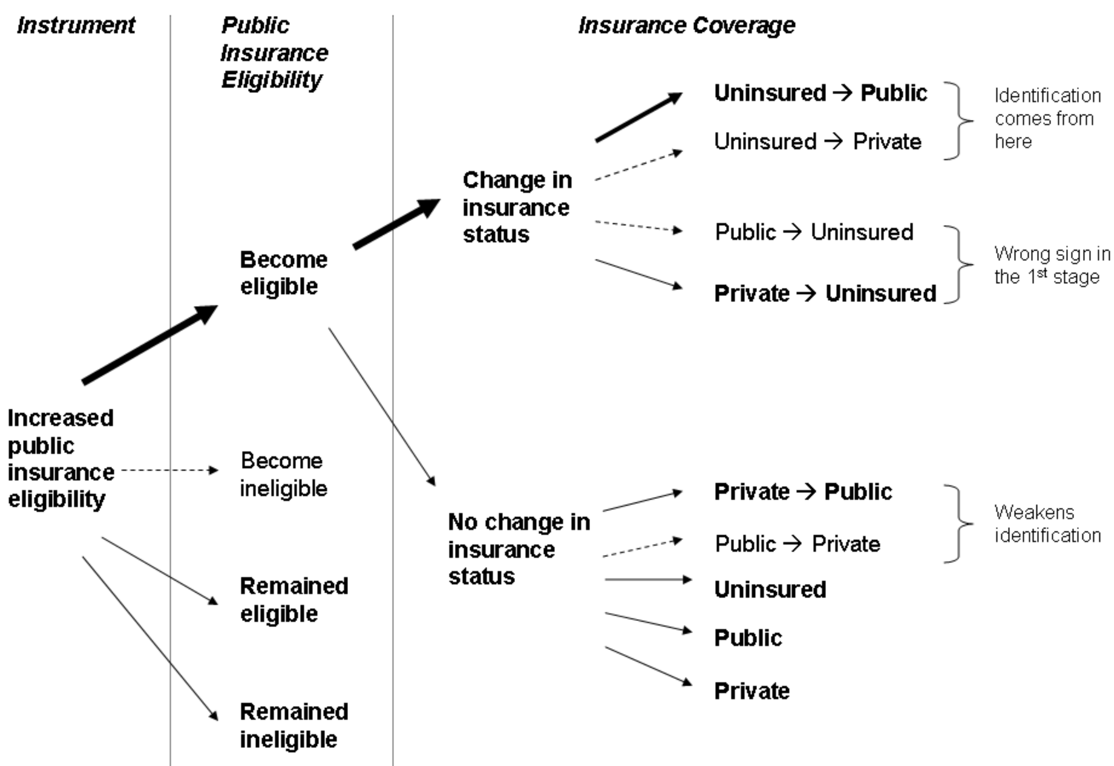


Figure A.10 The Effect of Public Insurance Eligibility Expansions on Children's Health Insurance Status in This Study

Therefore the magnitude of estimates would decrease. On the other hand, if the opposite is true, then children would have used more health care. Therefore the magnitude of estimates would increase. The latter set of children are those who dropped private coverage to become eligible for public insurance since some states impose waiting periods before allowing children to enroll in public health insurance programs (CBO 2007). These children will reduce magnitude of estimates since they will most likely reduce their use of health care since they are no longer covered by insurance.

Among newly eligible children, there are also children who do not change their health insurance status despite the fact that they are now eligible for public insurance. Since these children most likely do not change their health care seeking behaviors, the overall estimate of the effect of eligibility will decrease. As repeatedly mentioned in the study, this study examines the effect of any insurance coverage, not public insurance eligibility, on children's health care use and health. Identification comes from children who changed their insurance status due to eligibility expansions. As before, children who changed their insurance status consist of different groups of children. They include children who changed from uninsured to publicly insured, from uninsured to privately insured, from privately insured to publicly insured, from publicly insured to privately insured, from publicly insured to uninsured, and from privately insured to uninsured as shown in Figure A.10. In this study, the comparison is between children who are insured (driven by those who newly became insured) and those who are uninsured. Both groups include different types of children in terms of their health insurance coverage status.

As stated before, of the six combinations of changes in insurance that children may have experienced, there is no theoretical explanation for changes from uninsured to private insurance, public insurance to private insurance, and public insurance to uninsured. Of the remaining combinations, children who experienced changes from uninsured to public insured provide the main identification for the study. Those who change from private insurance to public insurance weaken identification because the change in the type of insurance coverage does not change the status of insurance status in terms of any coverage and therefore does not contribute to identification. Those who change from private insurance to uninsured will produce wrong signs of coefficients in the first stage since it is hypothesized that public insurance eligibility increases insurance coverage among children.

As evident from Figures A.9 and A.10, simulated public insurance eligibilities have more direct relationship with public insurance eligibility than insurance coverage. This means that the correlation between simulated public insurance eligibility and actual public insurance eligibility is stronger than that between simulated public insurance eligibility and any insurance coverage. Therefore first stage F-statistics of eligibility studies should be larger in eligibility studies than this study. On the other hand, public insurance eligibility is more remotely related to health care use and health than insurance coverage as explained above, i.e. there are several combinations of changes in insurance that have reducing effect on magnitudes on the estimates. Therefore, magnitude of the coefficients on public insurance eligibility in the second stage in eligibility studies should be smaller than those on insurance coverage in the second stage in this study.

In both types of studies, children who change from uninsured to public insurance are those who more likely need care and are unhealthier than the average child. This is because children may not get coverage until they actually seek care. Some may enroll in Medicaid or SCHIP at the time of health care use (e.g. at a hospital) and these children are more likely to have higher need for health care or are less healthy than the average child. Therefore these instruments will most likely produce larger estimates for effects on health care use and health than for the average child.

2.15. Explanation of Endogeneity Bias

In a classical case where orthogonality of one of more of the regressor variables, X and the disturbance term, u is assumed, least square estimate is the best linear unbiased estimator. However, if this condition does not hold, e.g. in case of endogeneity bias, OLS estimators will be biased and inconsistent.

In the general linear model, $y = X\beta + u$, OLS estimator, b can be expressed as:

$$b_{OLS} = \beta + (X'X)^{-1}X'u \tag{A1}$$

If one or more of the regressors are correlated with the disturbance term, then b_{OLS} will be biased:

$$E[b_{OLS} \mid X] = \beta + (X'X)^{-1}X'u \neq \beta \tag{A2}$$

where the $(X'X)^{-1}X'u$ is the bias term. The direction of the bias cannot be generalized. When probability limit are taken,

$$\text{plim } b_{OLS} = \beta + \text{plim}(X'X/n)^{-1} * \text{plim}(X'u/n) \tag{A3}$$

If one assumes that $\text{plim}(X'X/n) = Q_{XX}$ and $\text{plim}(X'u/n) = Q_{Xu}$, then Equation A3 can be re-expressed as:

$$\text{plim } b_{OLS} = \beta + Q_{XX}^{-1} * Q_{Xu} \tag{A4}$$

Note that Equation A4 implies that if there are no correlations between the regressor variables and the disturbance term, then $Q_{Xu} = 0$, the second term drops out of the equation and is left with plim $b_{OLS} = \beta$, i.e. OLS estimates are consistent. On the other hand, if there are correlations between the regressor variables and the disturbance term, then $Q_{Xu} = 0$, then $Q_{Xu} \neq 0$ and therefore OLS estimates will not be consistent.

One solution to obtaining a consistent estimator when $Q_{Xu} \neq 0$ is to use instrumental variables. Instrumental variables, Z need to satisfy two conditions: 1) Z must be correlated with X and $\text{plim}(Z'X/n) = Q_{ZX}$ must be a finite matrix of full rank;

and 2) Z must not be correlated with u, i.e. $plim(Z'u/n) = Q_{Zu} = 0$. First, the general equation, $y = X\beta + u$ is premultiplied by Z':

$$Z'y = Z'X\beta + Z'u \qquad \text{(A5)}$$

This suggests IV estimator to be:

$$\begin{aligned} b_{IV} &= (X'Z(Z'Z)^{-1} Z'X)^{-1} X'Z(Z'Z)^{-1} Z'y \\ &= (X'P_Z X)^{-1} X'P_Z y \\ &= \beta + (1/n\ X'P_Z X)^{-1}(1/n\ X'P_Z u) \qquad \text{(A6)} \end{aligned}$$

Since instruments are assumed to be uncorrelated with the disturbance term in the limit, $plim(1/n\ X'P_Z u) = 0$, and therefore in the limit, the second term drops out and IV estimator is consistent:

$$plim\ b_{IV} = \beta \qquad \text{(A7)}$$

One special care of endogeneity bias may arise from measurement error. For example, in this paper, one can think of a case where more health conscious individuals are correctly reporting their health insurance status and health outcomes than those who are less health conscious. In this case, without controlling for health awareness levels of the individuals, reported health insurance status will be endogenous. In the case of measurement error, the bias on OLS estimator is towards zero. Consider a simple relationship:

$$y = \beta x + u \qquad \text{(A8)}$$

However, x is measured with error and thus a combination of true value x-tilda and an error, v:

$$x = x\text{-tilda} + v \qquad \text{(A9)}$$

Therefore the correct relationship will be:

$$y = \beta x\text{-tilda} + u \qquad \text{(A10)}$$

If instead of x-tilda, one uses x to estimate β, the OLS estimate will be:

$$\begin{aligned} b_{OLS} &= \sum yx/\sum x^2 \\ &= \sum x(\beta x\text{-tilda} + u)/\sum x^2 \\ &= \beta \sum x x\text{-tilda}/\sum x^2 + \sum xu/\sum x^2 \qquad \text{(A11)} \end{aligned}$$

Since 1) mutual independence of u, x-tilda and v; and 2) existence of appropriate second order moments and their probability limits are assumed, the following equations are true:

$$\text{plim}(1/n \sum x^2) = \sigma^2_{x\text{-tilda}} + \sigma^2_v \quad \text{(A12)}$$
$$\text{plim}(1/n \sum x\tilde{x}\text{-tilda}) = \sigma^2_{x\text{-tilda}} \quad \text{(A13)}$$
$$\text{plim}(1/n \sum xu) = 0 \quad \text{(A14)}$$

Substituting Equations A12-A14 into Equation A11 gives:

$$\text{plim } b_{OLS} = \beta(\sigma^2_{x\text{-tilda}}/(\sigma^2_{x\text{-tilda}} + \sigma^2_v)) \quad \text{(A15)}$$

Equation A15 implies that b_{OLS} is both biased and inconsistent with the bias going towards zero since $(\sigma^2_{x\text{-tilda}}/(\sigma^2_{x\text{-tilda}} + \sigma^2_v))$ is less than 1, i.e. attenuation bias. For more details, refer to econometrics text books such as Greene (2003) and Johnston and DiNardo (2007).

2.16. The Use of Linear Probability Models for Dichotomous Outcome Variables and Sensitivity of Results to Instruments

An interesting study by Leininger (2007b) examined the robustness of instrumental variables results in the identification of the effect of private and public health insurance on children's health care use. Using data from 1999 and 2002 waves of National Survey of America's Families, Leininger (2007b) found both private and public insurance increases health care use (any provider visit in the past year any well visit in the past year) among children with the effect of public insurance slightly greater than that of private insurance. She controlled for the endogeneity of insurance coverage using different combinations of family and state level instruments. Family level instruments included number of parents working in firms with 50 or more employees and number of parents who have worked with their current employer for more than one year. State level instruments included simulated eligibility for Medicaid, an index of the various enrollment requirements states had in place during the survey year,[35] the percent of all firms in the state that offer insurance to their employees, the percent of all firms that offer insurance, the percent of all firms that offer family plans with no employee contribution, and the percent of all firms that require a waiting period for insurance eligibility.

The effect of insurance was very large; on an average, even the smaller estimates produced by two stage non-linear residual included model suggested that public and private insurance increased provider visit by 45 and 42 percentage points, respectively. Many of her estimates from the linear probability model was out of range or unrealistically high; for example, the findings suggested that public and

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[35] The index compiles information on presence of an asset test, joint Medicaid/SCHIP application form at initial enrollment, joint Medicaid/SCHIP application form at redetermination, requirement of a face-to-face interview at initial enrollment, requirement of a face-to-face interview at redetermination, presumptive eligibility, being continuously eligible for at least 12 months after approval, and acceptance of self-reported income as an income measure. This is a state-year level index that takes on values from 2 to 8 in the sample with higher values denoting a lower burden of enrollment.

private insurance increased provider visit by 107 and 92 percentage points, respectively.

The interesting contribution of her paper was that the effects of insurance status on children's health care use were very sensitive to both instruments used and the estimation methods employed. She found that using state level instruments produced larger estimates than family level instruments and among state level instruments, simulated eligibility instruments seemed have produced smaller estimates than state level firm characteristics. She also found that for dichotomous outcome measures, non linear methods may be more appropriate for estimation than the linear method when the values of dependent variables are close to one.

Her findings are relevant for this study for several reasons: 1) The use of a linear probability model: Since the mean values of dichotomous outcome variables used in this study are generally low with values ranging around 0.1, the use of linear probability may not be a big issue. However, especially for the outcome measure for lost school days has a mean of 0.7 which may be problematic, one must keep in mind that linear probability model may produce out of range point estimate which may not be appropriate and therefore nonlinear estimation techniques may be necessary to produce better estimates. 2) IVs: This study uses simulated public insurance eligibility measures as instruments for health insurance. One must keep in mind that the estimates are sensitive to the instruments used and the results are only applicable to the population that the IVs are impacting on.

2.17. Inconsistencies in Outcome Measures Due to 1997 NHIS Questionnaire Redesign

Due to the questionnaire redesign that was implemented in 1997, some of the outcome measures used in the study were not consistently measured over time. The affected measures were: doctor visits, school days lost to illness visits, short stay hospital episodes, short stay hospital days. Before the 1997 redesign, the information on the number of school days lost to illness and the number of doctor visits were collected for past 2 weeks and for past year, respectively. However, after the redesign, NHIS collected information on the number of school days lost to illness for past year and the number of doctor visits in past 2 weeks. While past 12 months values were calculated by multiplying past 2 weeks values by 26 and similarly past 2 weeks values were calculated by dividing past 12 months values by 26 to create consistent measures over time, there were clear gaps in the outcomes before and after the redesign when the trends were observed. While mathematically, 2 weeks multiplied by 26 is equal to one year, altering the recalled data in this way may not work to maintain consistency for several reasons. First, past 2 weeks recall data most likely suffers from less measurement error than past year recall data. If there are differences in recall errors, the data will not be consistent over time. Second, since there is natural variation in event occurrences, incidents that happened in past 2 weeks cannot necessarily be generalized to what would have happened in the entire past year.

1997 NHIS data codebook also warns data users of the qualitative differences in the questions used for doctor visits (National Center for Health Statistics 2000). Prior to the questionnaire redesign, doctor visits included " (office) visits to medical doctors or health care professionals working with or for a medical doctor" (National Center for Health Statistics 2000, p.26) as well as home care visits by these professionals (National Center for Health Statistics 2000). The questionnaire gave examples of the types of doctors or health care professionals including "dermatologists, psychiatrists, and ophthalmologists, as well as general practitioners and osteopaths" (United States Department of Health and Human Services. 1996, p.18). However, after the redesign, the questionnaire separated out home visits from office visits and mentioned an expanded list of doctors; the respondents were asked to include nurses, physical therapists, and chiropractors (National Center for Health Statistics 2000). This change in the context of the questions before and after the redesign may have produced both quantitatively and qualitatively inconsistent estimates.

For some measures, NHIS changed the wording of the questionnaire and this created a gap in the measurement consistency. The measures that were affected in this way are: short stay hospital episodes and short stay hospital days. Before the questionnaire redesign, NHIS asked for the number of short stay hospital episodes and the number of short stay hospital days in the past 12 months. After the redesign, NHIS asked for the number of times when the person stayed in any hospital overnight or longer and the number of nights the person had been in the hospital in the past 12 months. There is a clear break in the trend between the two periods of years for all of these measures, and therefore two separate analyses had to be done, one for the years 1992-1996 and one for the years 1997-2002. Figure A.1 shows the gaps in trends for selected outcome variables.

2.18. Weak Instruments Problem

As mentioned in the text, when IV regression suffers from a weak instrument problem, 1) IV estimates will have larger standard errors, i.e. asymptotic variance is higher than that of OLS estimator and it will be larger if the correlation between the endogenous regressor and the instrument is lower (asymptotic problem #1); 2) IV estimates may be inconsistent if the instrument is not entirely exogenous (asymptotic problem #2); and 3) in finite samples, IV estimates will be biased in the same direction as the OLS estimates and the magnitude of the bias is inversely related to sample size and the correlation between the instrument and the endogenous regressor (Bound, Jaeger and Baker 1995).

The intuition behind low precision of IV estimator (problem #1 from above) is as follows. Recall that variance of OLS estimator is greater if the variation in X is smaller (recall that $var(b_{OLS}) = (X'X)^{-1}\sigma^2$). Since IV only uses the part of variation in X that is correlated with the instrument (so naturally the variation that is being used for IV is smaller than the total variation), the variance of IV estimator will be larger than that of the OLS estimator. As an instrument becomes weaker, the variation in X correlated with the instrument becomes smaller and therefore the variance of IV estimator increase.

If the instrument has even a small correlation with X, then weak instrument leads to a large inconsistency in the IV estimate. Suppose there is a small direct correlation between the instrument Z and y:

$$y = X\beta + Z\gamma + \varepsilon \tag{A16}$$

and therefore $\gamma \neq 0$. Consider just identified case. The first line in Equation A6 simplifies to:

$$b_{IV} = (Z'X)^{-1}Z'y \tag{A17}$$

Substituting Equation A16 into Equation A17 gets:

$$b_{IV} = \beta + (Z'X)^{-1}Z'Z\gamma + (Z'X)^{-1}Z'\varepsilon \tag{A18}$$

When probability limits are taken,

$$\begin{aligned} \text{plim } b_{IV} &= \beta + \text{plim}(Z'X/n)^{-1} * \text{plim}(Z'Z/n)\gamma \\ &= \beta + Q_{ZX}^{-1} * Q_{ZZ}\gamma \end{aligned} \tag{A19}$$

Equation A19 implies that when $\gamma \neq 0$ and the instruments are weak (Q_{ZX} will be small so Q_{ZX}^{-1} will be large), there will be a large inconsistency in $b_{IV.}$

Finite samples properties of IV estimators are complicated. But in general, IV estimates will be biased in the same direction as the OLS estimates and the magnitude of the bias increases as the correlation between the instrument and the endogenous regressor decreases. For intuition, consider an extreme case where there is no correlation between Z and X. However, in the first stage regression, since the regression will desperately look for correlation between Z and X, coefficient on Z will not be completely zero. In this case, the variation in X that the regression picks up using Z is arbitrary and therefore the expectation of b_{IV} will most likely equal expectation of b_{OLS} although the variance of b_{IV} will be much larger than b_{OLS} due to all the noise in the second stage. For more details, refer to Bound, Jaeger and Baker (1995).

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CHAPTER THREE:
THE EFFECT OF WELFARE REFORM ON CHILDREN'S HEALTH CARE USE AND HEALTH

3.1. Introduction

The U.S. has seen a slow improvement in child health over the past several decades. A report by the Federal Interagency Forum on Child and Family Statistics (FIFCFS 2004) shows that general child health status had remained fairly stable since the mid 1980s with a slight upward trend. Although the health gap between low- and high-income children decreased slightly over the years, low-income children remain consistently and considerably less healthy compared to high-income children (FIFCFS 2004).

In the light of slow improvements in child health and evidence that underscores the importance of non-medical policies for child health (Klerman 1996), it is critical to understand the effects of recent changes in social policy that have affected resources available for low-income families with children. One of these policy changes is welfare reform that culminated in the passing of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996. It not only affected polices that directly relate to children's health care but also made significant changes to their home environments.

This study examines the effect of welfare reform on children's use of health care and health status by using the 1990-2002 National Health Interview Survey, a survey collected by the National Center for Health Statistics, Centers for Disease Control and Prevention of the U.S. Department of Health and Human Services. Difference-in-difference model is estimated to compare the difference in the change in outcomes between treatment and control groups before and after AFDC waiver and TANF implementation. Following the earlier studies, the treatment group is defined as children living in a low educated single mother family and the control groups are defined as those living in a high-educated single mother family and those in a two-parent family. Alternative definitions are also used to check the robustness of the results. Effects for all children and for immigrant children are estimated.

This study extends the existing literature in several ways. This is the first econometric study on welfare reform and child health using a nationally representative dataset. To this date, most studies had used small-scale experimental data and hence the findings have low external validity and a few large scale non-experimental studies only examine limited outcomes (infant health and child health, i.e. birthweight) or for a specific population (e.g. immigrant children in Kaushal and Kaestner (2007)).

Second, this study examines the effect of welfare reform on health care use and health status for immigrant children. There is evidence that welfare reform reduced overall insurance coverage among the immigrant population (Kandula et al 2004) although Borjas (2003) found that welfare reform increased employer provided insurance coverage by inducing more work among immigrants. How these changes in welfare policies affected health is not yet examined by previous research with the exception of Kaushal and Kaestner (2007). Unlike Kaushal and Kaestner (2007), this study examines the effect for all children (not limiting to those ages 0-14). Children who are above age 14 are less likely to be affected by changes in their environment due to welfare reform because health decisions for younger children are more likely taken by the mothers who are directly affected by welfare reform. It is nevertheless important to include all children who may have been affected by the new welfare scheme.[1] A treatment group is defined as children with low educated single mothers

[1] Kaushal and Kaestner (2007) do not explain the reason for excluding children ages 15-18 in their paper.

where low educated is defined as those who have 12 or less years of education as opposed to the definition used by Kaushal and Kaestner (2007) which consists of those who have 15 years or less years of education. The definition used in this study may be more relevant for the purpose of examining the effect of welfare reform using difference in difference method because previous studies had found that the majority of single women with less than 12 years of education are welfare recipients (Kaestner and Lee 2003). Unlike Kaushal and Kaestner (2007), only pre-redesign data (years 1990-1996) is used for some outcomes in this study (and therefore examining the effect of AFDC waivers and not TANF) since these measures were not consistently measured over time, i.e. pre and post 1997 NHIS questionnaire redesign. By pooling data from all the years together, the effect captured by the TANF dummy in Kaushal and Kaestner (2007) may or may not reflect the true effect of TANF implementation.

The remainder of the paper proceeds as follows. In section 3.2, some background information on welfare reform is introduced. Section 3.3 reviews existing literature on welfare reform and child health. Section 3.4 explains the basic model and conceptual framework underlying the study. Section 3.5 outlines the identification procedures. Section 3.6 introduces the data. Section 3.7 tests for the difference in difference method. Section 3.8 presents the results from the descriptive analyses and Section 3.9 provides the results from the regression analyses. Section 3.10 explores some of the pathways of the effect of welfare reform and Section 3.11 presents the results from the sensitivity analyses and Section 3.12 offers conclusions and implications for future research.

3.2. Background Information on Welfare Reform

Aid to Dependent Children (ADC), a precursor to Aid to Families with Dependent Children (AFDC) was enacted in 1935. Initially, ADC only covered children who lived with a parent or a close relative. However, after 1950, benefits were also extended to caretaking relatives (Office of Human Services Policy 2008). In the same year, congress also required states to provide benefits to all eligible applicants, making AFDC benefits a need-based entitlement (Office of Human Services Policy 2008).

Beginning in the early 1990s, many states started seeking waivers from the federal government to make changes to their AFDC programs under Title IV Section 1115(a) of the Social Security Act. This act allowed states to have more flexibility in designing their programs. Most state waivers fell in one of the three categories: encouraging work, increasing personal responsibility, and restricting recipients' eligibility (Future of Children 1997). In essence, implementing waivers was a way for the states to tighten their welfare services. States' efforts to reform welfare culminated on August 22, 1996 when the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) was enacted, ending the entitlement to cash assistance, mandating work requirements and time limits for those receiving assistance, and allowing states to sanction to those who did not follow the new rules. This was a drastic change from AFDC that served the nation for more than 60 years.[2] Instantaneously a universal safety net had turned itself into an "employment contingent social contract" (Haskins 2001). The key changes of PRWORA are summarized in Appendix 3.13 (Urban Institute 1996, Weil and Finegold 2002). The biggest change was replacing the old AFDC cash assistance program with a new program known as Temporary Assistance for Needy Families (TANF). Table 3.1 shows the implementation dates of AFDC waivers and TANF. AFDC waivers were implemented as early as 1992 in some states and by early 1998, all states had implemented TANF and the new welfare system was in place.

[2] By August 1996, however, 44 states had AFDC waivers in place (Future of Children 1997).

Table 3.1 Implementation Dates of AFDC waivers and TANF

State	AFDC Waiver Implemented	TANF Implemented	
		Official	Actual
Alabama		11/15/1996	
Alaska		7/1/1997	
Arizona	11/1/1995	10/1/1996	
Arkansas	7/1/1994	7/1/1997	
California	12/1/1992	11/26/1996	1/1/1998
Colorado		7/1/1997	
Connecticut	1/1/1996	10/1/1996	
Delaware	10/1/1995	3/10/1997	
Dist. of Columbia		3/1/1997	
Florida	(1)	10/1/1996	
Georgia	1/1/1994	1/1/1997	
Hawaii	2/1/1997	7/1/1997	
Idaho		7/1/1997	
Illinois	11/23/1993	7/1/1997	
Indiana	5/1/1995	10/1/1996	
Iowa	10/1/1993	1/1/1997	
Kansas		10/1/1996	
Kentucky		10/18/1996	
Louisiana		1/1/1997	
Maine		11/1/1996	
Maryland	3/1/1996	12/9/1996	
Massachusetts	11/1/1995	9/30/1996	
Michigan	10/1/1992	9/30/1996	
Minnesota	(2)	7/1/1997	
Mississippi	10/1/1995	10/1/1996	7/1/1997
Missouri	6/1/1995	12/1/1996	
Montana	2/1/1996	2/1/1997	
Nebraska	10/1/1995	12/1/1996	
Nevada		12/3/1996	
New Hampshire		10/1/1996	
New Jersey	10/1/1992	2/1/1997	7/1/1997
New Mexico		7/1/1997	
New York		12/2/1996	11/1/1997
North Carolina	7/1/1996	1/1/1997	
North Dakota	(3)	7/1/1997	
Ohio	7/1/1996	10/1/1996	
Oklahoma		10/1/1996	
Oregon	2/1/1993	10/1/1996	
Pennsylvania		3/3/1997	
Rhode Island		5/1/1997	
South Carolina		10/12/1996	
South Dakota	6/1/1994	12/1/1996	

Source: Health and Human Services, Assistant Secretary for Planning and Evaluation, Setting the Baseline: A Report on State Welfare Waivers and other unpublished documents. (Crouse 1999)
Notes:
(1) Partial implementation in Florida (two counties for time limits and eight counties for increased earning disregard beginning in February 1994).
(2) Partial implementation in Minnesota (seven counties for consolidated AFDC and Food Stamp payments and increased earning disregard beginning in April 1994).
(3) Partial implementation in North Dakota (11 counties for work sanctions beginning in July 1996 and ten counties for increased earning disregard beginning in October 1996).

Table 3.1 (Continued)

State	AFDC Waiver Implemented	TANF Implemented	
		Official	Actual
Tennessee	9/1/1996	10/1/1996	
Texas	6/1/1996	11/5/1996	
Utah	1/1/1993	10/1/1996	
Vermont	7/1/1994	9/20/1996	
Virginia	7/1/1995	2/1/1997	
Washington	1/1/1996	1/10/1997	
West Virginia	2/1/1996	1/11/1997	
Wisconsin	1/1/1996	9/30/1996	9/1/1997
Wyoming		1/1/1997	

Before the enactment of PRWORA, there was no difference in the treatment of legal immigrants and citizens under the welfare laws (Wasem 2004). They received the same benefits as citizens under AFDC. After PRWORA, however, policy changes were made that affected the recently arrived legal immigrants in receiving welfare. PRWORA restricted access of the recently arrived legal immigrants within their first 5 years of residence in the U. S. to cash payments, Medicaid, Food Stamps and Supplemental Security Income (Kaushal and Kaestner 2007). Most states offered coverage to immigrants who had arrived before 1996 using federal funds, but since they were not allowed to use federal funds to cover those who arrived after 1996, fewer number of states used state level funds to cover the recently arrived legal immigrants (Kaushal and Kaestner 2007, Fix and Passel 1999). Research found that legal immigrants' use of welfare declined more sharply than the use by citizens; these studies attribute the decline in welfare use to the economic growth in the 1990s, changes in welfare policies for legal immigrants and the confusion and fear that enveloped the immigrant community after the welfare reform (Kaushal and Kaestner 2005, 2007, Fix and Passel 1999). It should be emphasized that illegal immigrants were barred from receiving benefits throughout the years.

3.3. Literature

Considering that the welfare policies in the U. S. developed around indigent children and their families, it is important to see how such policies have affected children. However, despite many child-related goals addressed in PRWORA, research in this area still remains very limited with national data (Duncan and Chase-Landsdale 2001, Blank 2002). This scarcity of research may be due to the lack of data on child outcomes of interest, especially in large datasets (Grogger et al. 2002). It may also be because effects on children crystallize over a longer span and therefore it may be difficult to detect any changes in the short run.

Studies on welfare reform and child health are limited in particular (Chavkin and Wise 2002, Smith et al 2000, Wise et al 2002).[3] The few studies that exist are mostly experimental studies, which have produced inconclusive results. In their synthesis of research, both Grogger et al. (2002) and Morris et al. (2001) found mixed effects of welfare reform on child health. For example, out of 53 child health measures covered in 24 small-scale random assignment studies studied by Grogger et al. (2002), favorable outcomes were found for 29 measures and unfavorable outcomes for 24 measures, of which only 11 measures (7 and 4 measures, respectively) were statistically significant.[4] More recent studies also find mixed results (Tout et al 2004).

Non-experimental studies on infant health find mixed results as well. Currie and Grogger (2002) find that increase in welfare caseloads is associated with insignificant decrease in *very low* birth weight but insignificant increase in *low* birth weight for AFDC waiver years. A similar trend seems to continue on to TANF years; Kaestner and Lee (2005) find that welfare caseload is associated with insignificant increase in low birth weight. Overall, existing literature suggests that welfare reform

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[3] Much still remains unknown about the effect of welfare reform on health status in general (Bitler, Gelbach and Hoynes 2004).

[4] For studies that conducted separate analysis for each age group, results from all children were reviewed whenever available and if not , middle age group (ages 6-11) were reviewed.

did not significantly affect health care use and health outcomes among children (Bitler and Hoynes 2006).

To the author's knowledge, there is only one study that examined the effect of welfare reform on health care use and health status of immigrant children. Kaushal and Kaestner (2007) examined the effect of TANF on the health care use and health status of immigrant children. They found no statistically significant effect except for some evidence of an increase in the number of school days lost to illness. This study extends their study in several ways which may or may not affect the results.

First, they defined low educated mothers as those whose education is 15 years or less. This definition combines mothers with some college education with those who are high school dropouts. Although this may not be a big issue if employers consider education obtained from foreign countries as inferior to those from the U.S., it may be an issue for two reasons. First, these are different groups of mothers (those with some college, high school graduates and high school drop outs) combined into one low educated group. Second, the meaningful education cutoff for immigrant welfare recipient is most likely whether or not the mother is a high school graduate. In the difference in difference method, treatment and control groups must be defined as those groups that are similar in everyway except for the fact that one group has a high probability of receiving welfare whereas another group has a low probability. Previous studies found that the average education of welfare receiving immigrant mother is 11 years (Hao and Kawano 2001). Therefore the relevant cutoff is most likely high school graduation and not college education. Whether the mother has 12 or more years of education or not is the basis of this study.

Second, Kaushal and Kaestner (2007) used health care use and health status measures that were not consistently measured over time. In their analysis of the health care use and health status of the children, they used outcome measures that included:

any doctor visits in the past 2 weeks, any doctor visits in the past 12 month, doctor visits in the past 12 months exceeding 10 times, any overnight stay in a hospital in the past 12 months, and loss of school days in past 12 months. As discussed in another study of the author (Ueyama 2008), these measures were not consistently measured over time. There is a clear break in the trends before and after the questionnaire redesign that was implemented in 1997. Since the method of analysis is difference in difference, this would not be a problem if the differences in the trends in health care use and health status between treatment and control groups are consistent over time in the absence of welfare reform. However, since welfare reform and NHIS questionnaire redesign occurred at the same time, it is very difficult to find out whether the difference in trends between treatment and control groups before and after 1997 was due to welfare reform or questionnaire redesign. Therefore for these measures, difference in difference method may not be the most appropriate method to examine the effect of TANF. In this paper, to avoid these uncertainties, these measures are used to examine the effect of AFDC waivers that happened before 1997 using pre-questionnaire redesign data. Detailed discussion of this inconsistency in NHIS health care use and health status measures can be found in Ueyama (2008).

Third, Kaushal and Kaestner (2007) do not include data from 1997 because they claim that there is no nativity data in the NHIS public use person file. To the author's knowledge, this is not true. This study uses all years from 1990-2002 including 1997 for the analysis of outcome measures that are consistently measured over time.

3.4. Basic Model and Conceptual Framework

The basic model used in this study is based on Kaestner, Joyce and Racine's (1999) adaptation of Grossman's (1972) model to child health. Child health Y at age a in year

t is a function of current and past health care, *HC*, other market goods, *OG*, and parental time, *L*:

$$Y_t = f(HC_t, HC_{t-1},...,HC_{t-a}, OG_t, OG_{t-1},...,OG_{t-a}, L_t, L_{t-1},...,L_{t-a}; \varepsilon, \nu) \tag{1}$$

While *HC*, *OG* and *L* are choice variables, child's health endowment, *ε*, and child health production efficiency parameter, *v*, are not. This model is used to derive the reduced form of child health function:

$$Y_t = f'(p^{hc}{}_t, p^{hc}{}_{t-1},...,p^{hc}{}_{t-a}, p^{og}{}_t, p^{og}{}_{t-1},...,p^{og}{}_{t-a}, w_t, w_{t-1},...,w_{t-a}, I_t, I_{t-1},...,I_{t-a},; \varepsilon, \nu, \theta) \tag{2}$$

where p^{hc} is price of health care, p^{og} is price of other market goods, *w* is opportunity cost of parental time, *I* is family income and *θ* is taste parameter. This study estimates model 2.

Welfare Reform in the 1990s may have affected child health through several complex pathways.

First, welfare reform may affect the level of family resources and income (Smith et al. 2000). Changes in welfare payments, earnings, and other welfare transfers may influence parents' decisions on child investment such as food, health care, etc. that would ultimately affect child health. If family income (*I* in model 2) increases, child health will increase through increased household spending on child investments. Previous studies suggest that although family incomes increased after welfare reform (e.g. Schoeni and Blank 2000), neither the total household expenditure nor the proportion of the household expenditure allocated to health care and food has seen any changes (Kaushal, Gao and Waldfogel 2007).[5]

[5] Kaushal, Gao and Walfogel (2007) did find that when food expenditure was disaggregated for analyses, there was a decrease in spending for food prepared at home and an increase in spending for

Also, welfare reform may affect mothers' interaction with children. Due to work requirements under TANF, there may be changes in the quantity and quality of mothers' time with children and in childcare arrangements (Smith et al. 2000). Changes in earnings and penalties for noncompliance with work requirements may change the opportunity cost of their time, i.e. w, and reduce the quantity of time spent with children. The quantity of time spent with children may also simply decrease due to the fixed time available to the mothers (fixed time constraint). Quantity of time with children may affect child health because mothers can detect more irregularities with their children if they spend time with them. Their ability to detect irregularities may also increase as interactions with children increases. Previous studies have found that work weeks and hours increased after welfare reform (e.g. Moffitt 1999, Schoeni and Blank 2000).

The quality of mothers' time with children may also decrease if employment negatively affects mothers' psychological and mental conditions. With decreased quality of mothers' time with children, they may not be as efficient as they had been before in producing child health, i.e. decrease in v. On the other hand, mothers' mental health, social supports and self-esteem may increase when they enter the labor market. Previous studies suggest that moving welfare dependent mothers to work increased positive parenting and decreased harsh parenting, improving overall parenting styles (Dunifon, Kalil and Danziger 2003). These effects of mothers' employment may be counteracted or exacerbated by the use of child care services depending on the quality of these services. If child care services are perfect substitutes of mothers' time both in terms of quantity and quality, there may not be any net effect on child health. However, if they are inferior substitutes, then child health will most likely be negatively affected.

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food away from home after the welfare reform. This may or may not affect children's health depending on the type of food consumed at home and outside.

Finally, welfare reform delinked application for Medicaid from those for welfare payments. This caused confusion and increased the perceived marginal cost of health care among the Medicaid eligibles. Earlier, those who applied for welfare payments were automatically covered by Medicaid. After the passage of PRWORA, there is a separate application process for each program. Medicaid and SCHIP eligibility expansions that began as a part of welfare reform may have counteracted the negative effect of this delinkage on children's use of health care and health. This study includes controls for the a simulated fraction of children who would be eligible for SCHIP or Medicaid at the state/year/age level (from CPS) that measures each state's generosity of public health insurance eligibility. Therefore if there are any effects of welfare reform on children's health care use and health through public health insurance programs, then it should be a negative effect created by the confusion at the time of welfare reform.

There are several reasons why the effects of welfare reform on children's health care use and health status may differ between natives and immigrants. First, time limits imposed on cash receipts after the welfare reform may have a larger negative impact on the immigrants. Since immigrants tend to stay on welfare longer than the natives (Borjas and Hilton 1996), they should have been receiving more welfare benefits than the natives. Thus income of immigrant families should have decreased by a greater amount than for the native families. Second, because of the restrictive provisions for immigrants' welfare benefits after welfare reform, the immigrant families' welfare receipts may have reduced more than in the native families. In both cases, decrease in welfare benefits may impact children's health if this decrease is not matched by increase in income through some other ways such as employment or naturalization. Previous studies have shown that welfare use among low income immigrants was much steeper than those of their native counterparts (Fix and Passel 2002). However, this decrease does not seem to have been compensated by the increase in naturalization or increase in income (Fix and Passel 2002). Third, it is likely that immigrant mothers may have greater barriers to employment (e.g. discrimination and language barriers) than the native mothers (Rosenbaum and Gilbertson 1995). In this case, fulfilling work requirements may be more difficult for immigrant mothers. Therefore, their welfare benefits may be cut due to incompliance with the work requirements or they may have to settle for inferior employment conditions than the native counterparts. This may negatively affect immigrant children. Although previous evidence does not provide any conclusive evidence of the differences in the increase in employment between the natives and the immigrants due to welfare reform (Kaestner and Kaushal 2005), this is very much a possibility.

It must be emphasized that some of the effects may take a long time to manifest itself. Since the variation exploited in this study is the timing of AFDC waiver and TANF adoption by states that happened in the mid 1990s, the time period used in this study may not be long enough for the effects to show up. Therefore this study will not be able to examine the long term effect of welfare reform on children's health and health care use.

3.5. Identification Procedures

The impact of welfare reform on health care use and child health is estimated using a difference-in-difference model of the following form:

$$Y_{ist} = \alpha_0 + \alpha_1 X_{ist} + \alpha_2 Z_{st} + \alpha_3 P_{st} + \alpha_4 T_{ist} + \alpha_5 P_{st} * T_{ist} + \alpha_6 \sigma_s + \alpha_7 \varpi_t + \varepsilon_{ist} \qquad (3)$$

where Y_{ist} is one of the several access to care measures or the health outcomes of child i in state s in year t: a dummy indicating whether the child is in excellent health (for all years), a dummy indicating whether the child has any limitation of activity (for all years), a dummy indicating whether the child had doctor visits in the past year and the number of such visits (for years 1990-1996), a dummy indicating whether the child had school days lost to illness visits in the past two weeks and number of such days (only for ages 5 to 17 and for years 1990-1996), a dummy indicating whether the child had short stay hospital episodes in the past year and the number of such episodes (for years 1990-1996), and a dummy indicating whether the child had short stay hospital days in the past year and the number of such days (for years 1990-1996).

These child health outcomes are used following previous experimental studies on welfare reform and child health (Grogger et al 2002). While these are the best child health measures available in NHIS, they are not perfect. Because all information on children in NHIS is provided by an (knowledgeable) adult member of the household, there are many reasons to believe that these child health measures are biased. For example, subjective child health may decline with the education level and income of the reporting adult member (Dow et al. 1997, Currie 2000). A better educated adult may be able to detect the symptoms of illness more accurately. A greater income allows children to have more frequent doctor visits finding out illnesses that had gone unnoticed before. Both lead to worse reported health Therefore, it is important to keep in mind that even relatively more objective measures used in this study such as number of school days lost due to illness, are reported, not measured (Currie 2000) and that they may be correlated with both observed and unobserved characteristics of the child. Lack of objective measures that are well suited for child health assessment in existing datasets is one of the major factors limiting the research (Currie 2000, Stein et al. 2005).

X_{ist} is a vector of individual characteristics including child's age, sex, race, and mother's age. Z_{st} is a vector of state characteristics including the seasonally-adjusted unemployment rate (data from the Bureau of Labor Statistics); real median wages (data from the CPS); the maximum value of the federal and state EITC for a single mother with two children (data from Green Book; Leigh, 2003); the income eligibility limit for Medicaid eligibility for pregnant women (data from the National Governor's Association); the annual employment growth rate (data from the National Bureau of Economic Analysis); the amount of federal housing money spent per 1,000 residents in the state (data from the U.S. Census Bureau); and the simulated fraction of children who would be eligible for SCHIP or Medicaid at the state/year/month/age level (from CPS).

P_{st} is a dummy variable of the policy of interest, i.e. AFDC waiver and TANF for analyses using all years only AFDC waiver for analyses using years 1990-1996, and it equals 1 if state *s* has a policy in year *t* and 0 otherwise.[6] AFDC waiver and TANF are included separately to capture differential effect of the two policies on child's health and health care use. AFDC waiver implementation turns P_{st} to 1 and it will remain 1 even after TANF implementation, i.e. it will not switch off when TANF is implemented. Therefore the coefficient of the TANF variable will capture the additional effect that TANF implementation had on the outcome variable over and above the effect from AFDC waiver implementation.

T_{ist} is a dummy variable that equals 1 if child *i* is in the treatment group and 0 if child *i* is in the control group. The treatment group is defined as the children living in a family headed by a single mother who has less than a high school degree. The control group is defined as the children living in a family headed by a single mother who has high school education or more. Alternative definitions of treatment and control groups are used as robustness checks. In the first alternative, the treatment

[6] For models with outcome variables referring to the measures at the time of interview or in the past 2 weeks, all the policy variables are merged using the month/year/state of interview. For models with outcome variables referring to the measures for the past 12 months at the time of interview, all the policy variables are merged using the month /previous year/state of interview.

group is defined as the children living in a family headed by a single mother who has less than a high school degree and the control group is defined as the children living in a married family where the mother has less than a high school degree.[7] In the second alternative, the treatment group is defined as the children living in a family headed by a never married mother who has less than a high school degree and the control group is defined as the children living in a family headed by never married mother who has high school education or more. In the third alternative, the treatment group is defined as the children living in a family headed by a never married mother who has less than a high school degree and the control group is defined as the children living in a married family where the mother has less than a high school degree.[8]

σ_s and ϖ_t are state and year fixed effects that are used to control for unobserved heterogeneity that may be correlated with the policy variable. Finally, ε_{ist} is the error term that captures the remaining unobservables that are not captured in the equation. The coefficient for the first order interaction term, $\alpha_{5,}$ is the DD estimator of interest.

Equation 3 is estimated using linear probability model for categorical dependent variables and OLS for continuous dependent variables on weighted data.[9,10] Because the error term is not normally distributed for categorical dependent variables, the use of the linear probability model will produce inefficient coefficient estimates. However, this is not a major problem because estimates are generally similar to those produced by nonlinear models when evaluated at the sample means (Greene 1993). In

[7] From their analysis of 1994 Current Population Survey, Kaestner and Lee (2003) reports that majority of single women with less than 12 years of education are welfare recipients whereas only a portion of married women regardless of their education are welfare recipients.
[8] Studies found that children of never married mothers are three times more likely to be on welfare and stay on welfare longer than children of divorced mothers (Besharov 1995). Note that my main specification of treatment and control groups use children of single mothers which include those with mothers who are both never married and divorced.
[9] Many DD studies with binary dependent variables used a linear probability model (Cawley et al. 2004, Acs and Nelson 2004, Currie and Gruber 1996, Asch and Warner 2001)
[10] The "Sample Child" weight (wtfa_sc) was used for the analysis.

fact, the linear probability model is more relevant for DD estimation because the coefficient estimate for the interaction term produced by nonlinear models cannot be interpreted as the DD estimator (Angrist 2001, Ai and Norton 2001). Nevertheless, nonlinear models (limited dependent variables techniques) are also used to complete the main findings of the study. Details are discussed in Appendix 3.14.

There were several changes at the state level that happened during the same time period as welfare reform that may have directly or indirectly affected children's health care use and health. Such changes include economic expansion of the 1990s, increases in Earned Income Tax Credit subsidies right before the welfare reform, the minimum wage increase in the mid 1990s, and the expansions of Medicaid and SCHIP in the 1990s. In the model used in this study, the effects of AFDC waiver and TANF implementation are isolated from all the other concurrent policy and economic changes that occurred around the same time by including state level characteristics and year fixed effects. For example, economic expansions are control by including variables such as unemployment rate, real median wages, and annual employment growth rate. Medicaid and SCHIP expansions are controlled by including the fraction of a nationwide sample of children that would be eligible for Medicaid or SCHIP as a measure for the state's generosity for public health insurance program and income eligibility limit for Medicaid eligibility for pregnant women. The effects from EITC expansion and minimum wage increase are controlled by the inclusion of year fixed effects because they both were implemented at the same time in all states. Despite the quite extensive list of state characteristics as well as state and year fixed effects included in the study, these measures may not fully control all the influential factors because there may have been possible interactions that occurred between these economic and other policy changes and the welfare reform. In addition, because of nonrandom selection of states that implemented AFDC waivers,[11] a small time frame in which TANF was implemented in all states, a diversity in the content of welfare policies by state and the difference between enacted policies and the actual practice on the field (e.g. Gais et. al. 2001), like the previous studies, this study is not immune from the bias caused by these issues.[12]

Some of these problems may be minimized by using within state control group. Even if other state level changes happened at the same time as welfare reform, if these changes affected both treatment and control groups in the same way, then comparing treatment and control group within each state minimizes some of the concerns raised above. However, if these other state level changes affected two groups in a different way at the same time as welfare reform, then the estimates obtained from this study will be biased. Section 3.7 explains more in detail the assumptions that need to be met for difference and difference method to be valid. More details on the identification strategy can be found in Appendix 3.15.

3.6. Data

The data for this study comes from National Health Interview Survey, which is a cross-sectional household interview survey conducted continuously throughout each year since 1957. It is a representative sample of the U.S. population from all 50 States and the District of Columbia. NHIS monitors the health of the U.S. population through collection of various health characteristics by many demographic and socioeconomic characteristics. It covers the civilian non-institutionalized population of the United States living at the time of the interview. Patients in long-term care facilities, persons on active duty with the Armed Forces, and U.S. nationals living in

[11] Previous studies suggest that states with higher unemployment rates were more likely to implement AFDC waivers compared to states with lower unemployment rates (eg. Schoeni and Blank 2000).
[12] More detailed discussion can be found in Blank (2002).

foreign countries are excluded from the survey. The annual response rate of NHIS is greater than 90 percent (Vital and Health Statistics Summary Health Statistics for U.S. Adults and Children Reports: National Health Interview Survey for various years). Blacks and Hispanics have been oversampled since 1985 and 1995, respectively. NHIS uses stratified multistage probability sampling. NHIS underwent two changes in 1997. The first is a change in the interview procedure. Until 1997, interviews were conducted using paper and pencil. However, starting from 1997, they have been conducted using a computer assisted personal interviewer (CAPI). The second change, which is the crucial one, is the questionnaire redesign that has been in effect starting with the 1997 survey.

1990-2002 NHIS data is used for the analyses. The sample for this study consists of children between ages 0-17 years, who lived with their mother at the time of interview, and are the children of the household reference person.[13] The sample is limited to those who were living with their mother at the time of interview because the effect of welfare reform is most likely to affect children through the mother by altering her work schedules or income. It is also limited to those children who are the children of the household reference person. Therefore it excludes all children who are the grandchildren of the household reference person. Children in mutigenerational families may have different ways in which their health and health care use may be affected than the children in standard nuclear families. For the sample of immigrant children, children who have at least one foreign born parent is used. Immigrant status is defined in this way since NHIS does not provide information about the citizenship status of the respondents. This is the same definition used by Kaushal and Kaestner (2007). Moreover, even if the citizenship status is available in NHIS, using nativity to

[13] In the sample child core, one child is randomly selected from each family and the basic information on health status, health care services, and behavior is collected from a responsible adult family member residing in the household.

define immigrant status may be better because it may eliminate potential selection bias due to naturalization. Some immigrants may have naturalized during the welfare reform due to stricter rules applied to legal immigrants under the new welfare rules (Kaushal and Kaestner 2005). Considering the fact that foreign born citizens are miscategorized as immigrants here, the results from this study will most likely result in an underestimation of the true effect of welfare reform. Sample sizes vary by dependent variables used for analysis due to missing observations and exclusion of irrelevant ages and years from 27423 to 69586 observations for all children and 11285 to 11395 observations for immigrant children.[14]

As briefly mentioned in the earlier section and thoroughly discussed in Ueyama (2008), due to the NHIS questionnaire redesign, many health care use and health status measures were not consistently measured over time. Most outcomes in this study were also not consistently measured before and after the redesign, creating a large break in the trends pre and post the redesign. Therefore, two separate analyses are conducted. For those outcome measures that were consistently measured over time, the effect of both AFDC waiver and TANF is examined. These measures are: a dummy indicating whether the child is in excellent health and a dummy indicating whether the child has any limitation of activity. For those measures that were not consistently measured over time, only the effect of AFDC waiver is examined using the pre redesign data, i.e. between the years 1990-1996. These measures are: a dummy indicating whether the child had doctor visits in the past year and the number of such visits, a dummy indicating whether the child had school days lost due to illness visits in the past two weeks and number of such days (only for ages 5 to 17), a dummy indicating whether the child had short stay hospital episodes in the past year and the number of such episodes, and a dummy indicating whether the child had short

[14] Sample used to estimate number of school days lost to illness or injury in past 12 months includes children ages 5 to 17.

stay hospital days in the past year and the number of such days. Unfortunately, since the questionnaire redesign occurred at the same time as TANF, it is not possible to examine the effect of TANF on these measures. Since immigrant children were affected by tighter restriction on immigrants' welfare receipts only after PRWORA, they would not have been affected by AFDC waivers. Therefore analyses for immigrant children were restricted to very limited set of outcomes.

The identification from this study comes from within state variation in the change of child health and health care use over time for the treatment and control groups. Therefore the changes in health and health care use for the treatment group children are compared with those of the control group children within each state. The mean numbers of all children in the treatment and control groups in state/year cells are 34 and 77, respectively. The mean numbers of immigrant children in the treatment and control groups in state/year cells are 20 and 14, respectively.

Upper panel of Table 3.2 compares descriptive statistics of the child's individual level characteristics for all children and by treatment and control groups. These statistics show that the two groups are fairly comparable in terms of observed characteristics although those in the treatment group are slightly younger than those in the control group. The difference in age between the two groups is about half a year. About 60% of the children in both groups are minorities although among the minority children, those in the treatment group are more likely to be Hispanic while those in the control group are more likely to be black. Since the children are younger in the treatment group, their mothers are also younger by around 2 years.

Lower panel of Table 3.2 compares the average children's health care use and health status measures for all years by treatment and control groups. Overall, the children in the treatment group have fewer doctor visits but higher hospital care use than their counterparts in the control group although the differences in hospital use between the two groups are not statistically significant. Children in the treatment group also fare badly in most child health measures.

Table 3.2 Descriptive Statistics of Individual Level Control and Outcome Variables for All Children and by Treatment and Control Groups

	All Children			Treatment Group			Control Group		
Variables	Mean	SD	N	Mean	SD	N	Mean	SD	N
<u>Individual Characteristics</u>									
Age	8.436	5.072	69586	8.371***	5.088	19797	9.005***	4.934	49794
Female	0.488	0.500	69586	0.500	0.500	19797	0.495	0.500	49794
NH White	0.200	0.400	69586	0.397***	0.489	19797	0.410***	0.492	49794
Hispanic	0.217	0.412	69586	0.417***	0.493	19797	0.174***	0.379	49794
Mother's age	35.445	7.206	69586	33.169***	8.538	19797	35.002***	7.563	49794
<u>Health Care Use</u>									
1990-1996 (past year)									
Any doctor visits	0.800	0.400	29483	0.778***	0.415	8869	0.820***	0.384	20614
# doctor visits	3.053	6.811	29483	2.991***	6.142	8869	3.322***	9.071	20614
Any hospital episodes	0.027	0.163	29597	0.038	0.191	8904	0.036	0.185	20693
# hospital episodes	0.034	0.260	29597	0.051	0.429	8904	0.047	0.302	20693
Any hospital days	0.027	0.163	29597	0.038	0.191	8904	0.036	0.185	20693
# hospital days	0.162	2.320	29597	0.242	2.634	8904	0.257	2.891	20693
<u>Health Status</u>									
1990-2002									
Excellent health	0.525	0.499	69149	0.350***	0.477	19631	0.464***	0.499	49523
Limitation in Activities	0.063	0.243	69586	0.102***	0.303	19797	0.094***	0.292	49794
1990-1996 (past 2 wks)									
Any lost school days	0.060	0.237	27423	0.067	0.250	7964	0.080	0.271	19459
# lost school days	0.129	0.687	27423	0.182**	0.917	7964	0.181**	0.843	19459

Notes: *** Significant at 1% level, *** Significant at 5% level, * Significant at 10% level Significance levels are for a test of difference between treatment and control groups. The treatment group is defined as the children living in a family headed by a single mother who has less than a high school degree. The control group is defined as the children living in a family headed by a single mother who has high school education or more. The summary statistics for individual characteristics are from the sample used for analyzing any activity limitation in the past year as a dependant variable.

Table 3.3 Descriptive Statistics of Individual Level Control and Outcome Variables for All Immigrant Children and by Treatment and Control Groups

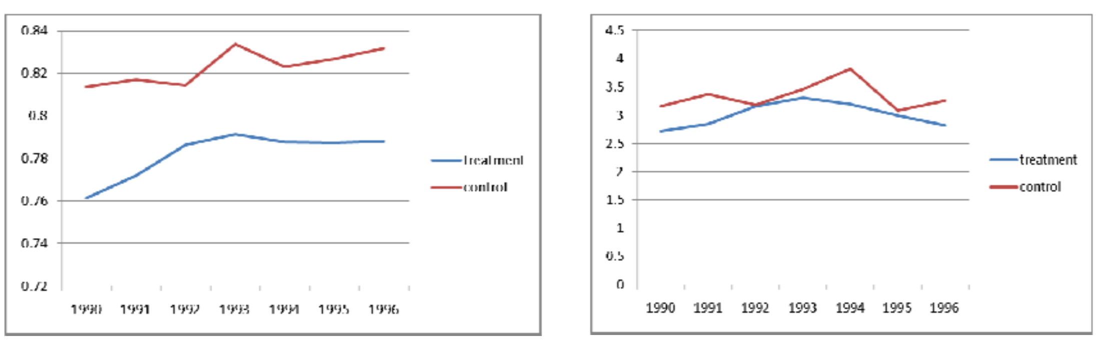
	All Children			Treatment Group			Control Group		
Variables	Mean	SD	N	Mean	SD	N	Mean	SD	N
Individual Characteristics									
Age	8.188	5.075	11395	8.835**	5.044	6018	9.041**	4.972	5381
Female	0.487	0.500	11395	0.495	0.500	6018	0.488	0.500	5381
NH White	0.193	0.394	11395	0.109***	0.311	6018	0.255***	0.436	5381
Hispanic	0.624	0.484	11395	0.871***	0.335	6018	0.599***	0.490	5381
Mother's age	35.517	7.352	11395	35.412***	8.225	6018	36.259***	7.543	5381
Health Care Use									
1990-1996 (past yr)									
Any doctor visits	0.762	0.426	4196	0.781***	0.414	2310	0.847***	0.360	1886
# doctor visits	2.551	4.938	4196	2.785***	4.964	2310	3.201***	5.268	1886
Any hospital episodes	0.023	0.149	4217	0.032	0.177	2324	0.029	0.168	1893
# hospital episodes	0.027	0.201	4217	0.042	0.261	2324	0.038	0.274	1893
Any hospital days	0.023	0.149	4217	0.032	0.177	2324	0.029	0.168	1893
# hospital days	0.147	2.374	4217	0.211	1.717	2324	0.291	4.373	1893
Health Status									
1990-2002									
Excellent health	0.452	0.498	11285	0.313*	0.464	5968	0.436*	0.496	5321
Limitation in Activities	0.041	0.197	11395	0.059**	0.236	6018	0.067**	0.251	5381
1990-1996 (past 2 wks)									
Any lost school days	0.044	0.205	3788	0.055	0.227	2080	0.053	0.224	1708
# lost school days	0.100	0.632	3788	0.138	0.776	2080	0.135	0.794	1708

Notes: *** Significant at 1% level, *** Significant at 5% level, * Significant at 10% level Significance levels are for a test of difference between treatment and control groups. The treatment group is defined as the children living in a family headed by a single mother who has less than a high school degree. The control group is defined as the children living in a family headed by a single mother who has high school education or more. The summary statistics for individual characteristics are from the sample used for analyzing any activity limitation in the past year as a dependant variable.

Table 3.3 shows the summary statistics for immigrant children. Overall, immigrant children are slightly younger and more likely to be a minority. They have less health care use and have poorer health. Despite their poorer health, they are less likely to miss school suggesting that immigrant children do not easily miss school due to health reasons. Among immigrant children, those in the treatment groups are more likely to be a minority, use less health care and have poorer health than those in the control group.

3.7. Testing for Difference in Difference Method

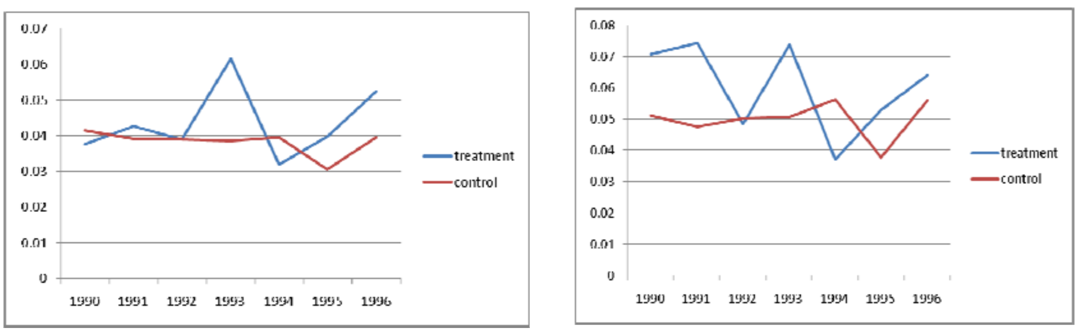
Difference in difference models assume that children in treatment and control groups have the same trajectories in health care use and health status over time in the absence of the welfare reform. The only difference between the two groups should be that the children in treatment group will be affected by the welfare reform whereas the children in the control group will not. Although it is impossible to find out what the trajectories would have been for the children in treatment group in the absence of the welfare reform, it is possible to compare the trajectories of the children in the two groups before the welfare reform. If the trajectories are similar before the welfare reform, then it provides convincing evidence that the trajectories would most likely be similar in the absence of welfare reform. In the sample period of this study, 1990 and 1991 are the years with no AFDC waivers or TANF.



A. Any DV Past Year **B. # of DV Past Year**

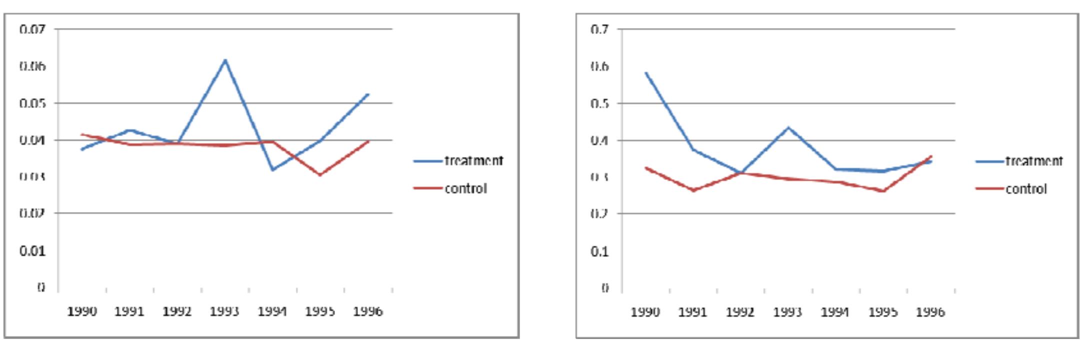
Figure 3.1 Change in the Fraction of Children who had Any Doctor Visits and Number of Such Illness in the Past Year for Years 1990-1996 for All Children by Treatment and Control Groups

Therefore if the trends are similar in these time periods, then it is likely that the trends would continue to be similar in the absence of welfare reform. Moreover, if this is true, then the change in trends from 1992 onwards is mostly due to the welfare reform. Figures 3.1 to 3.6 show the change in children's health care use and health status for all children and immigrant children over the sample period of this study. Since immigrant children were affected only by TANF, only outcomes that can be analyzed, i.e. excellent health and activity limitation are shown.



A. Any Hospital Episodes Past Year B. # of Hospital Episodes Past Year

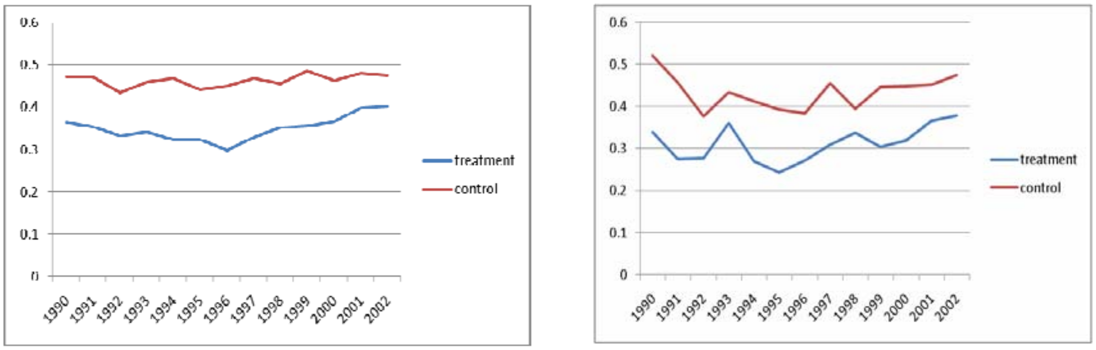
Figure 3.2 Change in the Fraction of Children who had Any Short Stay Hospital Episodes and Number of Such Episodes in the Past Year for Years 1990-1996 for All Children by Treatment and Control Groups



A. Any Hospital Days Past Year B. # of Hospital Days Past Year

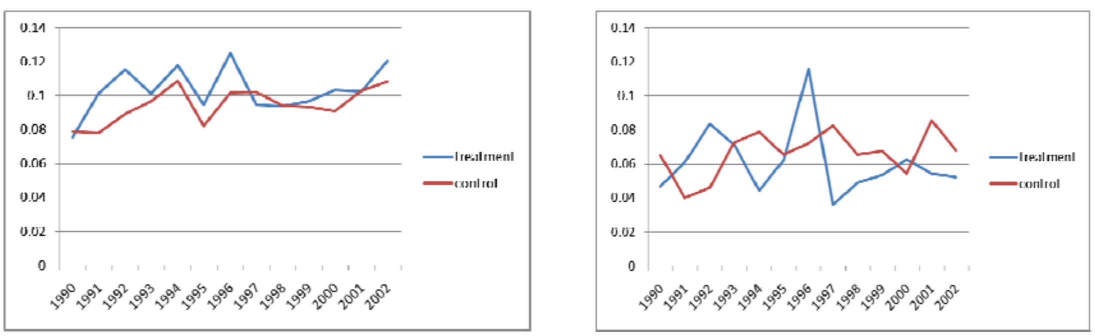
Figure 3.3 Change in the Fraction of Children who had Any Short Stay Hospital Days and Number of Such Days in the Past Year for Years 1990-1996 for All Children by Treatment and Control Groups

Figure 3.1 shows that the trends in the fraction of children with any doctor visits in the past year and the number of such doctor visits are fairly similar before 1992 between the two groups for all children. Figures 3.2 and 3.3 show quite different trends in hospital episodes and hospital days before 1992 between the two groups.



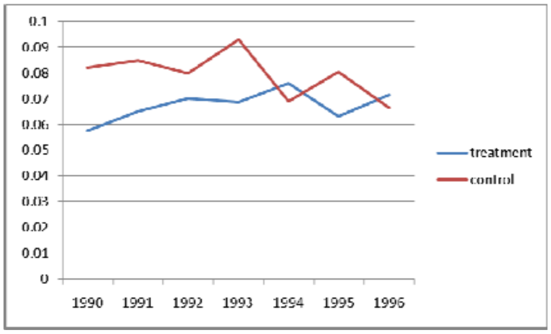
A. All Children **B. Immigrant Children**

Figure 3.4 Change in the Fraction Children in Excellent Health for Years 1990-2002 for All Children and Immigrant Children by Treatment and Control Groups

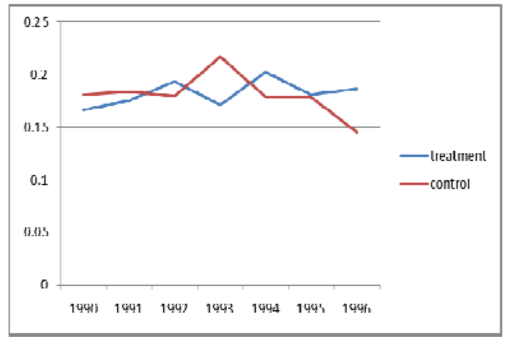


A. All Children **B. Immigrant Children**

Figure 3.5 Change in the Fraction Children in with Any Limitation of Activity for Years 1990-2002 for All Children and Immigrant Children by Treatment and Control Groups



A. Any Days Past 2 Weeks



B. # of Days Past 2 Weeks

Figure 3.6 Change in the Fraction of Children who had Any School Days Lost to Illness and Number of Such Days in Past 2 Weeks for Years 1990-1996 for All Children by Treatment and Control Groups

As for the health status measures, Figure 3.4 shows that trends in the fraction of children in excellent health are similar between the two groups before 1992 for both sets of children. Figure 3.5 shows that the trends in the fractions of children with activity limitation are very different before 1992 between the two groups for both sets of children but Figure 3.6 shows fairly similar trends in the fraction of children who had any school days lost to illness and the number of such days in the past year before 1992 between the two groups.

In summary, many health care use and health status measures have trends that varied in treatment and control groups even before the welfare reform. Although it is possible that after controlling for individual and state level characteristics, outcomes with completely different trajectories between two groups may actually be similar (i.e. hospital episodes, hospital days, activity limitation for all children; activity limitation for immigrant children), results should be interpreted with caution. Other definitions of treatment and controls groups are used as specification tests which will be explained later in this study, but as shown in Appendix Figure 1, all produce trends that differ for the two groups for selected outcomes as seen here. However, the important issue to keep in mind is that there should be no systematic difference in the divergence or convergence of the gap in trends between the states that is correlated with the timing of welfare reform and these simple time trend graphs at the national level masks these points.

3.8. Results from Descriptive Analyses

The key to finding out the causal effect of AFDC waiver and TANF implementation is to see whether there is a difference in the change of children's health care use and health status outcomes before and after AFDC waiver and TANF implementations for treatment and control groups. If welfare reform had an effect on children's health care use and health status only among those in the treatment group as it is assumed in the difference in difference method used in this study, the difference in child health measures between the groups should change after the welfare reform. This difference in the difference of outcomes between the two groups before and after the welfare reform is the causal effect of the welfare reform. In Tables 3.4 and 3.5, the change in children's health care use and health status is compared (before AFDC waiver implementation, after AFDC waiver implementation but before TANF implementation, and after TANF implementation) for the treatment and control groups for all children and immigrant children, respectively.

Descriptive statistics for all children suggest that the two groups experienced quite different trends in their use of doctor but similar trends for hospital use. For both the treatment and the control group children, more children had at least one doctor visit in the past year after AFDC waivers but the number of doctor visits decreased. The magnitudes are similar for both groups. After AFDC waivers, fewer children are using hospitals for both groups. But among those who use hospitals, the numbers of episodes and days in hospitals increase for the control group children whereas they decrease for the treatment group children.

Table 3.4 Change in Children's Health Care Use and Health Status Before AFDC Waiver, After AFDC Waiver but Before TANF, and After TANF for All Children

	Before AFDC Waiver		After AFDC Waiver		After TANF	
	Mean	SD	Mean	SD	Mean	SD
Control Group						
Any doctor visits (past yr)	0.819	0.385	0.831	0.375	n/a	n/a
# doctor visits (past yr)	3.348	9.484	3.149	5.462	n/a	n/a
Any hospital episodes (past yr)	0.036	0.186	0.034	0.181	n/a	n/a
# hospital episodes (past yr)	0.047	0.300	0.048	0.316	n/a	n/a
Any hospital days (past yr)	0.036	0.186	0.034	0.181	n/a	n/a
# hospital days (past yr)	0.250	2.684	0.301	4.024	n/a	n/a
Excellent Health	0.458	0.498	0.456	0.498	0.471	0.499
Activity Limitation	0.088	0.284	0.097	0.296	0.099	0.299
Any lost school days (past 2 wks)	0.104	0.305	0.101	0.302	n/a	n/a
# lost school days (past 2 wks)	0.236	0.943	0.240	1.017	n/a	n/a
Treatment Group						
Any doctor visits (past yr)	0.777	0.416	0.783	0.412	n/a	n/a
# doctor visits (past yr)	3.050	6.470	2.675	3.914	n/a	n/a
Any hospital episodes (past yr)	0.039	0.193	0.031	0.175	n/a	n/a
# hospital episodes (past yr)	0.054	0.457	0.036	0.222	n/a	n/a
Any hospital days (past yr)	0.039	0.193	0.031	0.175	n/a	n/a
# hospital days (past yr)	0.255	2.784	0.169	1.606	n/a	n/a
Excellent Health	0.345	0.475	0.305	0.460	0.371	0.483
Activity Limitation	0.108	0.311	0.078	0.268	0.104	0.305
Any lost school days (past 2 wks)	0.094	0.292	0.088	0.283	n/a	n/a
# lost school days (past 2 wks)	0.257	1.090	0.236	1.012	n/a	n/a

Table 3.4 shows the change in general health status for all children. For both groups of children, general health status decreases after AFDC waivers but increases after TANF for both groups with the magnitude of the change being slightly higher for the treatment group than the control group. Activity limitation among control children increases after AFDC waivers and TANF, but those among treatment group decreases and increases after AFDC waivers and TANF, respectively. After AFDC waivers, both groups of children have fewer proportions of children with at least one school

day lost due to illness. But among the children who have been absent from school at least once, the number decreases for the control group children whereas it is increasing for the treatment group children.

Table 3.5 Change in Children's Health Care Use and Health Status Before AFDC Waiver, After AFDC Waiver but Before TANF, and After TANF for Immigrant Children

	Before AFDC Waiver		After AFDC Waiver		After TANF	
	Mean	SD	Mean	SD	Mean	SD
Control Group						
Excellent Health	0.423	0.494	0.400	0.490	0.451	0.498
Activity Limitation	0.059	0.235	0.080	0.272	0.070	0.256
Treatment Group						
Excellent Health	0.297	0.457	0.254	0.436	0.347	0.476
Activity Limitation	0.072	0.259	0.054	0.226	0.053	0.224

For immigrant children, as repeatedly mentioned before, there should not have been any change in outcome induced by welfare reform after AFDC waivers. There should only be a change after TANF when more restrictive laws on immigrants were implemented. As shown in Table 3.5, there was a slight decrease in general health status after AFDC waiver and an increase after TANF for both treatment and control groups. Since a slight decrease in the general health status after AFDC waivers was experienced by both groups, it must be a natural trend in health that is not related to welfare reform. The magnitude of the increase in general health status is larger for the treatment group than the control group. Trends in activity limitation for the control group children are very different from the trends found for the treatment group children. There is a consistent decrease in the activity limitation for treatment group whereas it fluctuates for control group children. It is not entirely clear from this table whether there was any effect of the welfare reform on immigrant children.

In summary, for all children, welfare reform does not seem to have had much effect on preventive care use, except a decrease in the frequency of curative and hospital care use. This may be an indication that children's health status improved after the welfare reform. The effect of welfare reform on immigrant children's health status is ambiguous although there is a slight indication of improved health. Next section presents results from regression analyses that control for factors that may bias these descriptive results.

3.9. Results from Regression Analyses

Table 3.6 and 3.7 show the results from DD estimations using linear probability and OLS models for all children and immigrant children, respectively. Each column represents a different dependent variable. As mentioned earlier, regressions for doctor visits, hospital episodes, hospital days and lost school days use data only from 1990-1996. Since doctor visits, hospital episodes and hospital days are measures for the past one year, essentially capturing the outcomes up to 1995, only the effect of AFDC waivers can be estimated using these measures. Since lost school days are measures for the past 2 weeks, the outcomes include years up to 1996 and therefore for this measure, the effect of AFDC waivers and the earlier years of TANF are estimated.

The results for children's health care use for all children from Table 3.6 show statistically insignificant effects. While possibility of welfare reform having no effect on children's health care use cannot be ruled out, overall trends seem to imply that children are getting basic doctor's care (positive but statistically insignificant point estimate of the effect on the probability of children having at least one doctor visit in the past year) thereby reducing the incidence of hospital usage (negative but statistically insignificant point estimates of the effects on children's hospital episodes and hospital days).

Table 3.6. The Effect of AFDC Waiver and TANF on Children's Health Care Use and Health Status for All Children

	Any Doctor Visits	# of Doctor Visits	Any Hospital Episodes	# of Hospital Episodes	Any Hospital Days	# of Hospital Days	Excellent Health	Activity Limitation	Any Lost School Days	# of Lost School Days
AFDC waiver	0.012	-0.007	0.008	0.015	0.008	0.012	-0.004	0.006	-0.019**	-0.050*
	(0.012)	(0.181)	(0.006)	(0.010)	(0.006)	(0.098)	(0.011)	(0.006)	(0.009)	(0.028)
TANF							-0.009	-0.01	-0.009	-0.110**
							(0.017)	(0.010)	(0.026)	(0.043)
Treatment	-0.038***	-0.171	0.004	0.006	0.004	-0.015	-0.106***	0.031***	-0.001	0.033*
	(0.006)	(0.123)	(0.003)	(0.006)	(0.003)	(0.040)	(0.007)	(0.004)	(0.005)	(0.018)
AFDC * Treatment	0.004	-0.292	-0.003	-0.014	-0.003	-0.118	-0.013	-0.027***	0.002	-0.007
	(0.015)	(0.202)	(0.008)	(0.011)	(0.008)	(0.092)	(0.014)	(0.009)	(0.011)	(0.041)
TANF * Treatment							0.030**	0.028***	0.274***	0.571***
							(0.015)	(0.009)	(0.101)	(0.213)
Observations	29483	29483	29597	29597	29597	29597	69149	69586	27423	27423
R-squared	0.052	0.014	0.004	0.003	0.004	0.002	0.026	0.023	0.011	0.005

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Each regression includes controls for child's age, sex, race, mother's age and a vector of state characteristics including the seasonally-adjusted unemployment rate, real median wages, the maximum value of the federal and state EITC for a single mother with two children, the income eligibility limit for Medicaid eligibility for pregnant women, the annual employment growth rate, the amount of federal housing money spent per 1,000 residents in the state, and the simulated fraction of children who would be eligible for SCHIP or Medicaid at the state/year/age level. The treatment group is defined as the children living in a family headed by a single mother who has less than a high school degree. The control group is defined as the children living in a family headed by a single mother who has high school education or more. All regressions are estimated using linear probability model for categorical dependent variables and OLS for continuous dependent variables on weighted data and include state and year fixed effects. The first six regressions use data from 1990-1996 and the latter four regressions use data from 1990-2002.

While the number of doctor visits are decreasing, more children seem to have gotten at least one doctor visit in the past year. This is a favorable outcome considering the fact that children are recommended to have an annual routine checkup or preventive care by a physician. Perhaps due to this increased use of preventive care, children's use of curative care and hospitals is decreasing.[15] However, one cannot be completely sure whether this reflects a causal relationship between health care use and welfare reform. On one hand, increased labor force participation due to welfare reform may have increased awareness among mothers which in turn led to an increase in the use of preventive care for their children. Increase in income may also have allowed mothers to allocate more money to their children's preventive health care. As it will be discussed further in the later section, alternative specifications generally produce consistent results for preventive and curative care; there is reasonably convincing evidence that the relationship is not a statistical artifice. However, results for hospital care is quite sensitive to different specifications and therefore it is not possible to completely rule out the possibility of spurious effects, especially for hospital care.

Results from Table 3.6 shows that welfare reform had an inconsistent effect on children's health status for all children. AFDC waiver had a statistically insignificant effect on the proportion of children with excellent health but TANF implementation increased the proportion by 3 percentage points. The result for activity limitation is puzzling since AFDC waiver and TANF implementations have an effect in the opposite directions with the similar magnitude, ultimately canceling each other out to end up with zero net effect. Several possible explanations for AFDC waiver and

[15] Miller and Zhang (2007) suggested in their study that after welfare reform, the quality of parental inputs improved and the children's use of time changed in a beneficial way, most likely the result from improved parental disciplining, role modeling or newly introduced structure and stability in children's lives, which ended up more than offsetting the negative effects of decreased maternal time available to children after the welfare reform.

TANF having opposite effects are discussed in the later section. TANF had a fairly big impact on the proportion of children with lost school days due to illness with an increase by 27 percentage points but the number of such days did not increase as much (about half a day). Therefore the effect on children's health status is inconsistent with some positive effects (e.g. more children in excellent health) and some negative effects (e.g. more school days lost due to illnesses).

Moving on to the immigrant children, Table 3.7 shows that due to a much smaller sample size than for all children, most effects of welfare reform are imprecisely estimated. The only statistically significant effects are for AFDC waivers which indicate that fewer immigrant children have activity limitation. This effect of AFDC waivers on activity limitation is in fact puzzling considering the fact that immigrant children continued to enjoy the same benefits as native children after AFDC waivers (treatment changed for immigrant children only after TANF). The relationship indicated here between welfare reform and activity limitation among immigrant children is most likely spurious.

Table 3.7 The Effect of TANF on Children's Health Status for Immigrant Children

	Excellent Health	Activity Limitation
AFDC waiver	0.017	0.025
	(0.032)	(0.016)
TANF	0.046	-0.023
	(0.042)	(0.018)
Treatment	-0.110***	0.020**
	(0.018)	(0.009)
AFDC * Treatment	0.005	-0.045***
	(0.032)	(0.016)
TANF * Treatment	0.026	0.015
	(0.031)	(0.015)
Observations	11285	11395
R-squared	0.045	0.028

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; See Table 5 for notes.

For all children sample, there is some evidence that AFDC waiver and TANF implementation had a differential effect on certain outcomes such as activity limitation and excellent health (although the effect of AFDC waiver is not statistically significant). For those states that had both AFDC waiver and TANF implementations, this may be reflecting the fact that the welfare reform policies had different short term and long term effects.[16] Recall that AFDC waiver dummy turns on when the AFDC waiver is implemented and stays turned on for the succeeding years even after the TANF implementation. It is possible that the AFDC waiver dummy is reflecting the short term effect and the TANF dummy is reflecting the long term effect of the welfare reform. For example, children's health may have deteriorated after AFDC waiver implementation due to decreased quantity and quality of mother's time with the children, but by the time TANF was implemented, the increased income may have compensated for the decreased care time above and beyond what was lost, ultimately improving children's health. Another possibility for those states that had both AFDC waiver and TANF implementations is that the perceptions of the reporting adult member on the child's health may have changed with AFDC waiver and TANF implementations. For example, because many mothers had entered the work force after AFDC waiver implementation, they may not have been able to notice the child's limitation in activities as much as they had been able to before. However, again by the time TANF was implemented, they had become more used to their work schedules and were therefore more able to accurately detect children's activity limitations.

In summary, while all effects on children's health care use were imprecise, there is a weak indication that welfare reform increased preventive care use and reduced curative and emergency health care among all children. The effect of welfare

[16] 60% of the states implemented the AFDC waiver before TANF.

reform on children's health status is inconsistent for all children and immigrant children.

3.10. Possible Mechanisms

In this section, the possible mechanisms in which welfare reform may have affected children's health including mother's employment, family income and health insurance status are explored.[17] Although the effect of welfare reform on children's health care use and health status was inconsistent and negligible, it is nevertheless interesting to find out whether there were any effects of welfare reform on the potential pathways in which it may have affected children's health. A dummy indicating whether the mother is working, the number of children in the family, family income as percentage of federal poverty level and a dummy indicating whether the child has any health insurance are used as outcome variables. The existing literature suggests that welfare reform led to an increase in mother's labor force participation (e.g. Schoeni and Blank 2000), little to no (decreasing) effect on fertility rates (e.g. Kearney 2004, Joyce, Kaestner and Korenman 2002), an increase in family income (Schoeni and Blank 2000), although there is some evidence of a decrease in family income among blacks (Bitler, Gelbach and Hoynes 2003) and a decrease in health insurance coverage (e.g. Bitler, Gelbach and Hoynes 2004).

Table 3.8 shows results. The results for mother's labor force participation, fertility rates and health insurance are consistent with the literature. TANF implementation increased the proportion of working mothers, decreased the number of children in the family and decreased children's health insurance coverage. However, contrary to most previous studies, this study finds that welfare reform decreased family income. This may be due to the oversampling of Blacks and Hispanics in

[17] Since NHIS has information on health insurance status only from 1992, the analysis on children's health insurance status uses data from 1992-2002.

NHIS; some earlier studies found reductions in income among Blacks (Bitler, Gelbach and Hoynes 2003). Overall, these results suggest that the effect of welfare reform had expected effects on the potential mechanisms in which children's health care use and health status may have been affected. But these effects did not translate into changes in children's health care use and health status outcomes.

Table 3.8 The Possible Mechanisms of the Effect of AFDC Waiver and TANF Implementations on Children

	Mother Working	# of Children	Family Income (%FPL)	Any Health Insurance
AFDC waiver	0.029***	-0.049*	6.352**	0.019**
	(0.010)	(0.025)	(2.93)	(0.008)
TANF	-0.031**	-0.006	5.226	-0.026**
	(0.015)	(0.038)	(4.849)	(0.011)
Treatment	-0.317***	0.599***	-58.742***	-0.013*
	(0.006)	(0.019)	(1.175)	(0.007)
AFDC * Treatment	0.002	0.123***	-14.974***	-0.027**
	(0.014)	(0.043)	(3.336)	(0.012)
TANF * Treatment	0.077***	-0.211***	-8.492**	-0.028**
	(0.014)	(0.044)	(3.608)	(0.011)
Observations	66990	69586	68567	54158
R-squared	0.184	0.095	0.22	0.040

Notes: Robust standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$; See Table 5 for notes.

3.11. Results from Robustness Checks

To check the robustness of the results, several sensitivity analyses were conducted.

First, as mentioned earlier, this study tests whether using alternative definitions of treatment and control groups affect the results. Three alternative definitions are considered; 1) the treatment group is defined as the children living in a family headed by a single mother who has less than a high school degree and the control group is defined as the children living in a married family where the mother has less than a high school degree; 2) the treatment group is defined as the children living in a family headed by a never married mother who has less than a high school degree and the control group is defined as the children living in a family headed by never married mother has high school education or more; and 3) the treatment group is defined as the children living in a family headed by a never married mother who has less than a high school degree and the control group is defined as the children living in a married family where the mother has less than a high school degree. Appendix Tables A.1 to A.3 present these results. The estimates are still very imprecise for all the regressions especially those using alternative definition 2 where the sample size is very small. The only consistent effect seen for all the regressions is the increase in school days lost to illness, statistically insignificant decrease in the number of doctor visits and statistically insignificant decrease in the number of short stay hospital days. Recall that the results from the main specification weakly suggest that the welfare reform decreased the use of curative and hospital care while it increased the use of preventive care for all children. Although the increased use of preventive care and the decreased use of curative care are weakly supported by the results using alternative treatment and control group definitions, the results for hospital care are quite sensitive to the definition of treatment and control groups. The effect of welfare reform on children's health remains inconsistent in all specifications.

Second, difference in difference in difference (DDD) method is used following Kaushal, Gao and Waldfogel (2007) to control for the state level time varying factors that may have affected children's health care use and health status differently by mother's education status. Specifically, in the DDD model, in the first difference, time varying factors that affected both low educated and high educated single mothers similarly are controlled (i.e. comparing pre and post outcomes for the children with low educated single mothers and pre and post outcomes for the children with high educated single mothers). In the second difference, time varying factors that affected low and high educated mothers differently during the time period other than welfare reform are controlled (i.e. controlling for pre and post outcomes for the children with low educated married mothers vs pre and post outcomes for the children with high educated married mothers since married mothers are less likely to be on welfare). In the third difference, assuming that gaps in outcomes between low and high educated mothers are similar regardless of marital status, the gaps in outcomes for children with low versus high educated single mothers with the gaps in outcomes for children with low versus high educated married mothers are compared. The results are shown in Appendix Table A.4. The results are consistent with the main results from the study. Children of single mothers who are high school dropouts increased doctor visits in the past year by 6 percentage points and decreased the number of hospital days in the past year by 0.2 days after the AFDC waivers.

Third, this study tests whether the results stay the same when the combined AFDC waiver and TANF implementation variable is used for the analyses of children's health status measures.[18] The results are shown in Appendix Table A.5. Although the results are still imprecisely estimated, the general pattern of the results remains the same; welfare reform had inconsistent effects on children's health status.

Fourth, this study tests whether there is any effect of AFDC waivers on outcome for immigrant children during the years between 1990-1996. Since there was no change in the treatment of immigrant children after AFDC waivers, if identification strategy is correct, then one should not see any effect for pre TANF years. The results in Appendix Table A.6 show no effect of AFDC waivers on immigrant children's health care use. These results add credibility to the identification strategy used in this study.

[18] All the measures of health care use only examine the effect of AFDC waiver in this study.

Lastly, this study follows Kaushal and Kaestner (2007)'s specification of the sample immigrant children and treatment and control groups and examines how the results change when conditions are changed one by one. Specifically, specification used by Kaushal and Kaestner (2007) is: 1) The sample of children include children between ages 0-14 with foreign born mothers between ages 18 and 54; 2) Treatment and control groups are defined as children with single mothers with 15 or less years of education and children with married mothers with the same amount of education, respectively; 3) Instead of AFDC waiver dummy variable remaining one when TANF is implemented as in the main specification (i.e. coefficient on TANF indicates the incremental effect of TANF above and over those of AFDC waiver), here it is set to zero when TANF is implemented (i.e. the effects of AFDC waiver and TANF are separately estimated); 4) Data from 1997 is also removed from the sample; and 5) Time effects are controlled by state specific time trends. Moreover, outcomes for hospital episodes and days are treated as consistently measured variables. Appendix Table A.7 shows the results. The first three columns are results from regressions using Kaushal and Kaestner (2007)'s specifications. The succeeding columns present results when conditions are lifted one by one. Results from the first three columns are similar to those of Kaushal and Kaestner (2007). A small difference in magnitude for poor/fair health in this study and their study may be due to differences in control variables for state level characteristics. Results from succeeding columns show that results do not change much when conditions are lifted one by one although statistical significance disappears. Overall similarity of these results suggest that unconventional conditions used in Kaushal and Kaestner (2007)'s main specification do not have much impact on results that they would have gotten using specifications used by earlier studies. However, their treatment of inconsistently measured outcomes as consistently measured may still be problematic. Extent of the problem caused by using inconsistently measured outcomes to examine the effect of TANF cannot be verified here due to data limitations.

In summary, results from robustness checks show that results are quite sensitive to different treatment and control groups' definitions, especially for reported use of hospital care and health status, although DDD estimation of the effects of welfare reform on children's health care use and health status for all children were consistent with the study's main findings. For immigrant children, robustness checks provided suggestive evidence of validity of the study's identification strategy. Moreover, unconventional specifications used in Kaushal and Kaestner (2007)'s main model did not seem to significantly impact findings estimated here using more conventional specifications. However, not much can be inferred about the extent of the problem that arises from using inconsistently measured outcomes over time. In general, however, with the absence of consistent and statistically significant set of findings, it can be concluded that welfare reform did not affect children's health care use and health status for all children and immigrant children.

3.12. Conclusions and Implications for Future Research

This study examines the causal effect of welfare reform on children's use of health care and health status in the U. S. by using the 1990-2002 National Health Interview Survey. For all children, there were no statistically significant effects on their health care use. If anything, welfare reform led to an increase in the reported use of preventive care but a decrease in the reported use of curative care and an ambiguous effect on the use of hospital care but these effects were too imprecisely estimated to make any concrete conclusions. Welfare reform did not have a consistent effect on children's health status for all children and immigrant children. In general, with the absence of consistent and statistically significant set of results, none of the findings were strong enough evidence to conclude any effects of welfare reform on children's health care use and health status for all children and immigrant children. This was true despite the fact that there was an indication of changes in children's family environment and health insurance status due to the welfare reform that may have affected their use of health care and health status.

This study's results for all children confirm the previous studies that found no significant effects on children's health care use and health status. Lack of effects is consistent with the previous studies that found only a small adverse effect of welfare reform on the insurance coverage of low income children (e.g. Kaushal and Kaestner 2003, Cawley, Schroeder and Simon 2006) and a small positive to no effect of health insurance coverage on health care use and no effect of health insurance on health (See Levy and Meltzer 2001 for a review).[19] Moreover, similar to Kaushal and Kaestner (2007)'s finding, this study found no effect on immigrant children's health even after addressing some issues that may have affected their results. Lack of effects on immigrant children's health in this study is consistent with Kaushal and Kaestner (2007)'s study that found no effects of welfare reform on immigrant children's health insurance status.[20]

The welfare reform led to various changes in low income children's lives, including increased mother's labor force participation and reduced welfare benefit. Immigrant mothers faced more restrictive policies under the new welfare laws compared to the native mothers, creating a much more vulnerable situation for the immigrant children compared to the native children. However, fortunately, children's

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[19] For example, Cawley, Schroeder and Simon (2006) found that there were no effect of AFDC waivers on children's health insurance coverage, but TANF implementation decreased welfare-eligible child's insurance coverage by 3 percentage points and decreased their Medicaid coverage by 3.6 percentage points.

[20] Kaushal and Kaestner's earlier study (2005) using CPS found an increase in the uninsurance rate among immigrant children after welfare reform. Kaushal and Kaestner (2007) suggest that the differences in the results for immigrant children's health insurance found in two studies may be due to differences in sample compositions and sample sizes.

health care use and health status have not had major negative consequences due to the welfare reform. If anything, the findings from this study back up the study by Miller and Zhang (2007) which suggest that welfare reform improved children's lives by introducing structure and stability in daily lives through increased mother's employment, improved parental disciplining, role modeling and the quality of parental inputs.

APPENDIX

Table A.1 The Effect of AFDC Waiver and TANF Implementations on Children's Health Care Use and Health Status (Treatment/Control Group Definitions: Alternative 1)

	Any Doctor Visits	# of Doctor Visits	Any Hospital Episodes	# of Hospital Episodes	Any Hospital Days	# of Hospital Days	Excellent Health	Activity Limitation	Any Lost School Days	# of Lost School Days
AFDC waiver	-0.002	-0.097	0.002	0	0.003	0.032	0.003	-0.011*	-0.008	-0.011
	(0.013)	(0.180)	(0.005)	(0.009)	(0.005)	(0.080)	(0.011)	(0.006)	(0.008)	(0.028)
TANF							-0.008	-0.002	0.022	-0.007
							(0.017)	(0.009)	(0.035)	(0.083)
Treatment	0.064***	0.533***	0.007**	0.013**	0.007**	0.043	-0.056***	0.038***	0.032***	0.090***
	(0.007)	(0.107)	(0.003)	(0.005)	(0.003)	(0.039)	(0.007)	(0.004)	(0.005)	(0.019)
AFDC * Treatment	0.018	-0.27	0.005	0	0.005	-0.061	0.012	-0.005	0.012	0.017
	(0.016)	(0.184)	(0.007)	(0.010)	(0.007)	(0.082)	(0.014)	(0.008)	(0.011)	(0.039)
TANF * Treatment							-0.008	0.014	0.236**	0.432*
							(0.014)	(0.009)	(0.102)	(0.224)
Observations	28542	28542	28658	28658	28658	28658	62938	63456	25774	25774
R-squared	0.076	0.027	0.008	0.005	0.008	0.002	0.023	0.033	0.017	0.011

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; See Table 5 for notes with the exception of treatment and control group definitions. Here, the treatment group is defined as the children living in a family headed by a single mother who has less than a high school degree and the control group is defined as the children living in a married family where the mother has less than a high school degree.

Table A.2 The Effect of AFDC Waiver and TANF Implementations on Children's Health Care Use and Health Status (Treatment/Control Group Definitions: Alternative 2)

	Any Doctor Visits	# of Doctor Visits	Any Hospital Episodes	# of Hospital Episodes	Any Hospital Days	# of Hospital Days	Excellent Health	Activity Limitation	Any Lost School Days	# of Lost School Days
AFDC waiver	0.047**	-0.002	0.004	0.036	0.004	0.336	0.035*	0.003	-0.029*	-0.058
	(0.019)	(0.293)	(0.011)	(0.023)	(0.011)	(0.249)	(0.019)	(0.011)	(0.017)	(0.061)
TANF							-0.028	-0.002	0.039	0.032
							(0.028)	(0.015)	(0.062)	(0.098)
Treatment	-0.019*	0.209	0.011**	0.023**	0.011**	0.086	-0.056***	0.037***	0.001	0.072**
	(0.010)	(0.194)	(0.005)	(0.012)	(0.005)	(0.073)	(0.011)	(0.007)	(0.008)	(0.033)
AFDC * Treatment	-0.01	-0.451	0.008	-0.016	0.008	-0.392	-0.086***	-0.017	0.01	-0.05
	(0.021)	(0.312)	(0.014)	(0.024)	(0.014)	(0.255)	(0.023)	(0.014)	(0.019)	(0.071)
TANF * Treatment							0.078***	0.012	0.381**	0.928**
							(0.023)	(0.015)	(0.176)	(0.465)
Observations	9892	9892	9937	9937	9937	9937	23009	23160	7521	7521
R-squared	0.086	0.042	0.01	0.007	0.01	0.005	0.028	0.032	0.027	0.016

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; See Table 5 for notes with the exception of treatment and control group definitions. Here, the treatment group is defined as the children living in a family headed by a never married mother who has less than a high school degree and the control group is defined as the children living in a family headed by never married mother has high school education or more.

Table A.3 The Effect of AFDC Waiver and TANF Implementations on Children's Health Care Use and Health Status (Treatment/Control Group Definitions: Alternative 3)

	Any Doctor Visits	# of Doctor Visits	Any Hospital Episodes	# of Hospital Episodes	Any Hospital Days	# of Hospital Days	Excellent Health	Activity Limitation	Any Lost School Days	# of Lost School Days
AFDC waiver	-0.003	-0.099	0.002	0	0.002	0.024	0.008	-0.011*	-0.001	0.008
	(0.014)	(0.195)	(0.006)	(0.010)	(0.006)	(0.088)	(0.012)	(0.006)	(0.008)	(0.028)
TANF							-0.022	0.003	0.021	-0.01
							(0.019)	(0.009)	(0.035)	(0.084)
Treatment	0.055***	0.607***	0.013***	0.025***	0.014***	0.126*	-0.035***	0.050***	0.030***	0.128***
	(0.010)	(0.187)	(0.005)	(0.010)	(0.005)	(0.071)	(0.010)	(0.006)	(0.008)	(0.035)
AFDC * Treatment	0.032*	-0.378	0.012	0.004	0.012	-0.066	-0.029	-0.002	0.015	-0.022
	(0.019)	(0.272)	(0.012)	(0.017)	(0.012)	(0.121)	(0.019)	(0.012)	(0.016)	(0.060)
TANF * Treatment							0.017	0.002	0.384**	0.854*
							(0.019)	(0.012)	(0.179)	(0.501)
Observations	23754	23754	23847	23847	23847	23847	51916	52334	20930	20930
R-squared	0.081	0.029	0.009	0.006	0.009	0.002	0.024	0.032	0.017	0.012

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; See Table 5 for notes with the exception of treatment and control group definitions. Here, the treatment group is defined as the children living in a family headed by a never married mother who has less than a high school degree and the control group is defined as the children living in a married family where the mother has less than a high school degree.

Table A.4 Difference in Difference in Difference Estimation of the Effect of AFDC Waiver and TANF Implementations on Children's Health Care Use and Health Status

	Any Doctor Visits	# of Doctor Visits	Any Hospital Episodes	# of Hospital Episodes	Any Hospital Days	# of Hospital Days	Excellent Health	Activity Limitation	Any Lost School Days	# of Lost School Days
AFDC waiver	0.004	0.107	0.004*	0.007**	0.004*	-0.003	-0.004	0	-0.008**	-0.022**
	(0.005)	(0.092)	(0.002)	(0.003)	(0.002)	(0.024)	(0.005)	(0.002)	(0.004)	(0.009)
TANF							-0.003	-0.001	0.034**	0.060*
							(0.008)	(0.004)	(0.014)	(0.033)
Single	0.033***	0.552***	0.010***	0.015***	0.010***	0.093***	-0.073***	0.039***	0.032***	0.084***
	(0.003)	(0.063)	(0.001)	(0.002)	(0.001)	(0.023)	(0.004)	(0.002)	(0.003)	(0.008)
HS dropout	-0.080***	-0.232***	0.006***	0.007***	0.006***	0.046*	-0.130***	0.029***	-0.001	0.028***
	(0.004)	(0.058)	(0.002)	(0.002)	(0.002)	(0.025)	(0.004)	(0.002)	(0.003)	(0.008)
Waiver * Single	-0.004	-0.182	0.007	0.01	0.007	0.096	-0.015	0.008	-0.004	-0.002
	(0.009)	(0.164)	(0.005)	(0.008)	(0.005)	(0.074)	(0.009)	(0.005)	(0.006)	(0.020)
TANF * Single							-0.012	-0.005	-0.04	-0.165***
							(0.009)	(0.005)	(0.028)	(0.046)
Waiver * HS dropout	-0.011	-0.181	-0.005	-0.008*	-0.004	0.022	-0.041***	-0.018***	-0.012**	-0.021
	(0.010)	(0.128)	(0.004)	(0.005)	(0.004)	(0.067)	(0.008)	(0.004)	(0.005)	(0.019)
TANF * HS dropout							0.023***	0.007	0.003	-0.028
							(0.009)	(0.004)	(0.036)	(0.083)
Waiver * Single * HS dropout	0.060***	-0.031	-0.004	-0.012	-0.004	-0.214**	0.063***	-0.014	0.016	0.032
	(0.017)	(0.210)	(0.008)	(0.011)	(0.008)	(0.104)	(0.015)	(0.009)	(0.011)	(0.040)
TANF * Single * HS dropout							0.005	0.022**	0.271**	0.594***
							(0.017)	(0.010)	(0.108)	(0.228)
Observations	148692	148692	149266	149266	149266	149266	327613	329844	134989	134989
R-squared	0.058	0.015	0.003	0.003	0.003	0.001	0.041	0.019	0.006	0.004

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; See Table 5 for notes with the exception of treatment and control group definitions.

Table A.5 The Effect of Combined AFDC Waiver and TANF Implementations on Children's Health Status

	Excellent Health	Activity Limitation	Any Lost School Days	# of Lost School Days
AFDC waiver / TANF	-0.01	0	-0.020**	-0.059**
	(0.010)	(0.006)	(0.009)	(0.027)
Treatment	-0.106***	0.031***	0	0.033*
	(0.007)	(0.004)	(0.005)	(0.018)
AFDC/TANF * Treatment	0.01	-0.006	0.013	0.019
	(0.009)	(0.006)	(0.012)	(0.040)
Observations	69149	69586	27423	27423
R-squared	0.026	0.023	0.009	0.005

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; See Table 5 for notes.

Table A.6 The Effect of AFDC Waiver and TANF on Children's Health Care Use and Health Status for Immigrant Children

	Any Doctor Visits	# of Doctor Visits	Any Hospital Episodes	# of Hospital Episodes	Any Hospital Days	# of Hospital Days
AFDC waiver	-0.011	0.421	-0.006	-0.02	-0.006	-0.113
	(0.039)	(0.395)	(0.014)	(0.025)	(0.014)	(0.375)
TANF						
Treatment	-0.038**	-0.046	0.008	0.013	0.008	0.056
	(0.016)	(0.229)	(0.007)	(0.010)	(0.007)	(0.086)
AFDC * Treatment	-0.023	-0.094	-0.004	-0.009	-0.004	-0.506
	(0.034)	(0.363)	(0.012)	(0.019)	(0.012)	(0.489)
Observations	4196	4196	4217	4217	4217	4217
R-squared	0.111	0.062	0.021	0.019	0.021	0.009

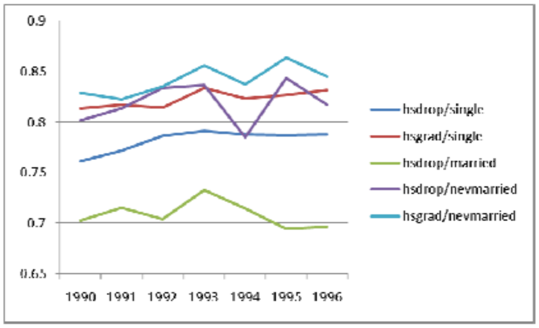
Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; See Table 5 for notes.

Table A.7 The Effect of AFDC Waiver and TANF Implementations on Immigrant Children's Health Care Use and Health Status (Following Kaushal and Kaestner 2007)

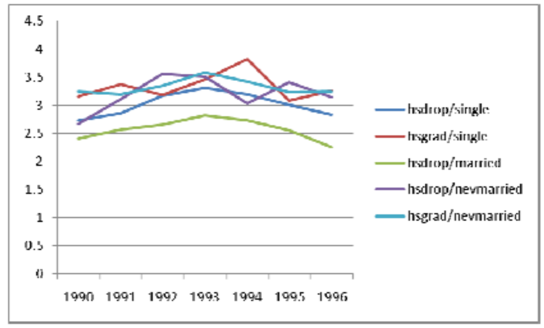
	Poor/ Fair Health	Any Hospital Episodes	Any Hospital Days	Poor/ Fair Health	Any Hospital Episodes	Any Hospital Days	Poor/ Fair Health	Any Hospital Episodes	Any Hospital Days
TANF * Treatment	-0.017***	-0.009*	-0.009*	-0.016***	-0.010**	-0.010**	-0.004	-0.007	-0.007
	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.014)	(0.011)	(0.011)
Treatment/control groups: single/married mothers with years of ed<=12							X	X	X
Child's age <=14	X	X	X	X	X	X	X	X	X
Mother's age bet 18 & 54	X	X	X	X	X	X	X	X	X
Treatment/control groups: single/married mothers with years of ed<=15	X	X	X	X	X	X			
Drop 1997	X	X	X						
Observations	39832	35142	35142	43866	38958	38958	21004	18751	18751
R-squared	0.011	0.011	0.011	0.010	0.012	0.012	0.013	0.014	0.014

	Poor/ Fair Health	Any Hospital Episodes	Any Hospital Days	Poor/ Fair Health	Any Hospital Episodes	Any Hospital Days
TANF * Treatment	-0.004	-0.007	-0.007	-0.015	-0.012	-0.012
	(0.014)	(0.011)	(0.011)	(0.014)	(0.011)	(0.011)
Treatment/control groups: single/married mothers with years of ed<=12	X	X	X	X	X	X
Child's age <=14	X	X	X			
Mother's age bet 18 & 54						
Treatment/control groups: single/married mothers with years of ed<=15						
Drop 1997						
Observations	21135	18859	18859	24756	22276	22276
R-squared	0.013	0.014	0.015	0.014	0.012	0.012

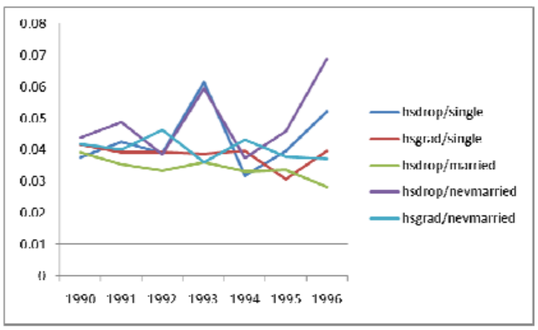
Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; See Table 5 for notes.



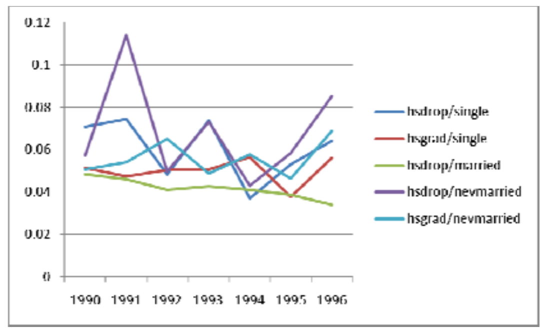
A. Any Doctor Visits Past Year



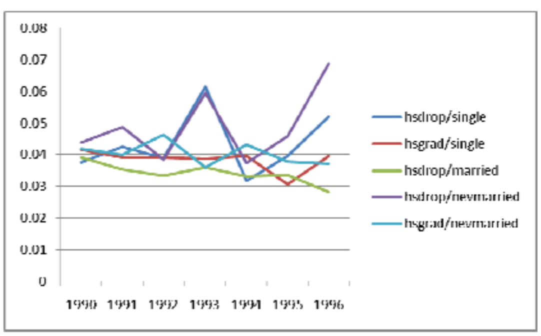
B. # of Doctor Visits Past Year



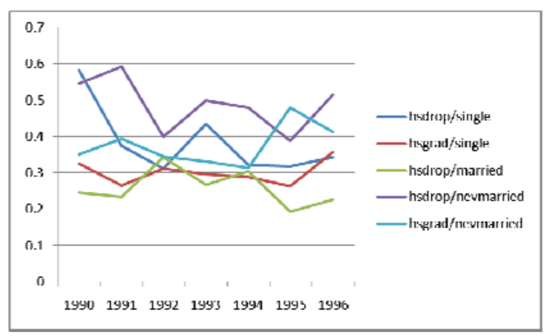
C. Any Hospital Episodes Past Year



D. # of Hospital Episodes Past Year

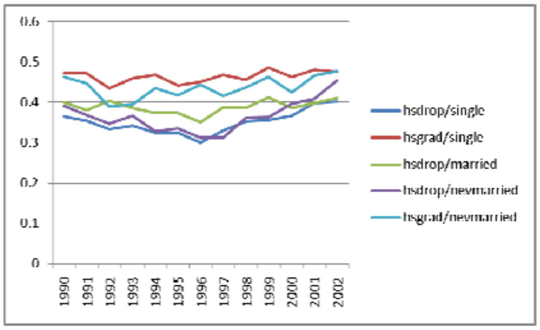


E. Any Hospital Days Past Year

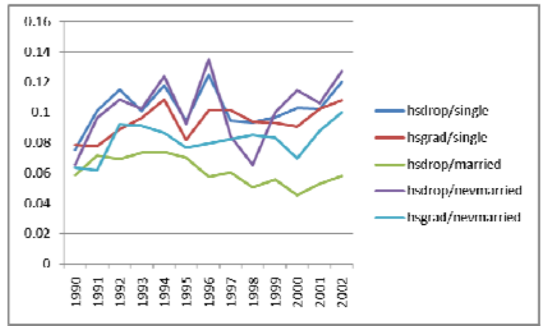


F. # of Hospital Days Past Year

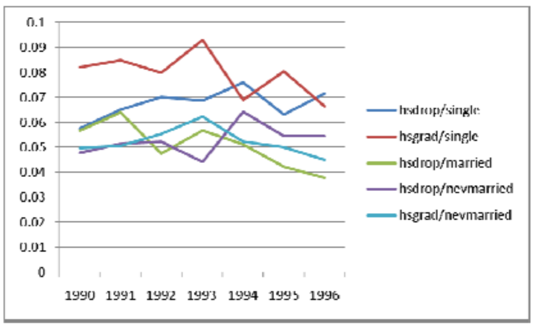
Figure A.1 Change in Children's Health Care Use and Health Status by Different Definitions of Treatment and Control Groups for All Children



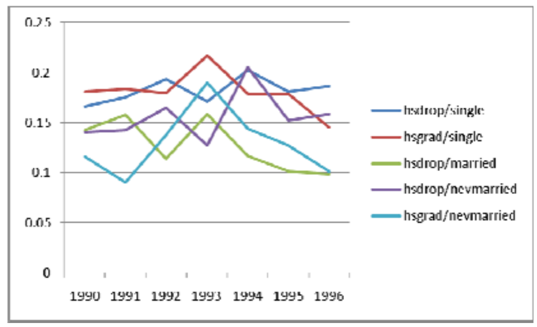
G. In Excellent Health



H. Any Activity Limitation



I. Any Lost School Days Past 2 Weeks



J. # of Lost School Days Past 2 Weeks

Figure A.1 (Continued)

3.13. Key Changes of PRWORA

<u>Temporary Assistance for Needy Families (TANF)</u>

1. Purpose: "[To] provide assistance to needy families so that children may be cared for in their own homes or in the homes of relatives; [to] end the dependence of needy parents on government benefits by promoting job preparation, work, and marriage; [to] prevent and [to] reduce the incidence of out-of-wedlock pregnancies and establish annual numerical goals for preventing and reducing the incidence of these pregnancies; and [to] encourage the formation and maintenance of two-parent families." (H.R.3734 SEC. 401.)
2. Time limits: Federal time limit of 5 years; states may set their own guidelines.
3. Work requirements: Recipients must start working or be involved in work related activities within 2 years of cash assistance. Penalty may apply for non-compliance.
4. Family cap: States have the option to make cash assistance on per family bases and not on number of children.
5. Unmarried minor parents: Must live at home with a responsible adult and participate in educational or training activities to receive assistance.

<u>Child Care</u>

1. Eliminated entitlements for recipients

2. Increased Child Care and Development Fund levels

Child Support

1. Strengthened child support enforcement
2. Noncooperation penalty is imposed for single mothers who do not cooperate in paternal establishment

Food Stamps

1. Reduced maximum benefit and limited deductions
2. Benefits limited to 3 months for able-bodied adults without children unless working at least 20 hours per week
3. Reduction of benefit levels and allowances for reductions in food stamp benefits for families penalized under TANF rules (Smith et al 2000).

Medicaid

1. Delinked Medicaid eligibility from welfare
2. States may set higher income eligibility standards

Supplemental Security Income

1. Restricted standards of disability for children
2. More frequent review of disability status for children and adults

Immigrants

1. Legal immigrants only eligible for Supplemental Security Income, food stamps
2. Legal immigrants are not eligible for TANF, Medicaid, and Title XX services for 5 years after arriving in the U. S. Federal benefits are denied to all illegal immigrants.

3.14. Findings from Nonlinear Models

In the main text, all categorical dependent variables are estimated using linear probability models. The study uses linear probability models for ease of interpretation of difference in difference (DD) estimate which is the interaction term between treatment and post dummies. Difficulty in using nonlinear models is that coefficient estimate for the interaction term cannot simply be interpreted as the DD estimator. The coefficient on interaction term will not represent the full interaction effect, the sign will vary for different values of the covariates and the statistical significance will be erroneous (Angrist 2001, Ai and Norton 2001, Norton, Wang and Ai 2004). In essence, full interaction effect is the entire cross-partial derivative of the expected value of dependent variable and this interaction effect is conditional on all independent variables. The coefficient of the interaction term is only a portion of full interaction effect that may not even remotely represent the true interaction term due to signs and magnitudes of the additive terms not taken into account.

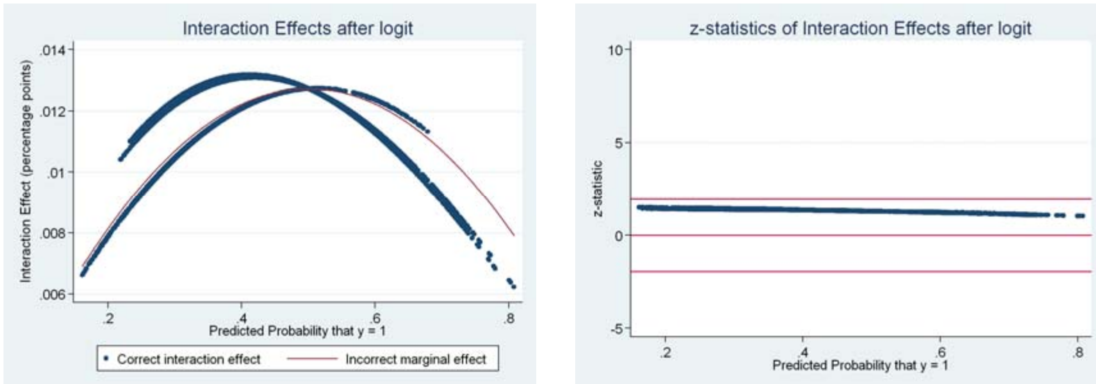
Despite the difficulties in the use of nonlinear methods in a DD model where the interaction term is crucial, it is nevertheless informative to find the results using nonlinear methods. The study uses logit regression for binary outcome variables and computes interaction effects and standard errors following inteff STATA command developed by Norton, Wang and Ai (2004). Since inteff command does not support two interaction terms (e.g. AFDC waiver – post and TANF – post), for this exercise, only one policy dummy variable is used; it equals one if either AFDC waiver or TANF is in place in state s in year t and zero otherwise.

Table A.8 The Effect of AFDC Waiver and TANF Implementations on Children's Health Status Using Logit Regression

	Excellent Health	Activity Limitation
AFDC waiver / TANF	-0.044	0.012
	(0.040)	(0.067)
Treatment	-0.451	0.349
	(0.030)	(0.047)
AFDC/TANF * Treatment	0.051	-0.075
	(0.039)	(0.064)
Observations	69149	69586
Wald Chi2	1458.60	1281.65
Pseudo R-squared	0.019	0.035
Interaction effect (IE)	0.012	-0.007
	(0.001)	(0.003)
SE	0.009	0.006
	(0.001)	(0.002)
Z-statistics of IE	1.330	-1.187
	(0.051)	(0.030)

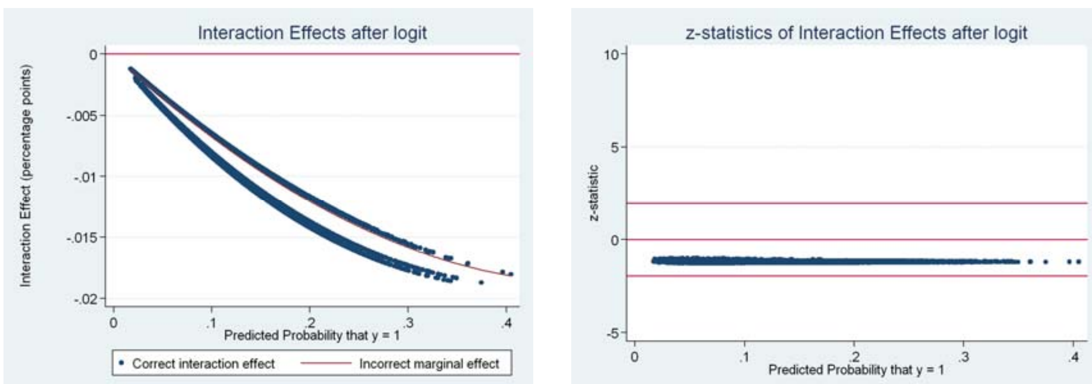
Notes: Robust standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$; See Table 5 for notes.

Table A.8 shows results from logit regressions. Main interaction term for excellent health is statistically insignificant at 0.05. However, when full interaction effect is calculated, the magnitude decreases by a fifth (mean effect). Full interaction effect varies moderately; for those children with predicted probability of being in excellent health is 0.2, interaction effect varies from 0.008 to 0.01 whereas for those with predicted probability of 0.5, interaction effect is around 0.012 (Figure A.2 Panel A). In general, the magnitude of the effect is smaller for children in the tails of predicted probability distribution but the effect is consistently statistically insignificant (Figure A.2 Panel B). For activity limitation, the main interaction effect is statistically insignificant at -0.075. Similar to the results found for excellent health, magnitude decreases when full interaction effect is calculated with the mean effect around -0.007 and varies to some extent. Figure A.3 Panel A suggests that children with higher predicted probability of having activity limitation have experienced greater negative effect of welfare reform. However, again consistent with the earlier results for excellent health, interaction effects for children in all predicted probabilities are statistically insignificant (Figure A.3 Panel B).



A. Interaction Effects B. Z-Statistics of Interaction Effects

Figure A.2 Interaction Effects and its Z-Statistics from Logistic Regression for Excellent Health



A. Interaction Effects B. Z-Statistics of Interaction Effects

Figure A.3 Interaction Effects and its Z-Statistics from Logistic Regression for Activity Limitation

When these results from logit regressions are compared with main results, general conclusions remain the same. Recall that the main results suggested that welfare reform had net positive effect (statistically significant) on the probability that the child is in excellent health and net zero effect on the probability (statistically significant) that the child has activity limitation. Although all findings from logit regressions are statistically insignificant, both linear and nonlinear models suggest that, if anything, the welfare reform had a slight positive effect on children's health.

3.15. More on Identification

To find out the effect of welfare reform on children's health care use and health, one cannot simply regress children's health care use and health on indicators for whether a state has implemented AFDC waivers or TANF using a dataset that has observations from different years and states. This is because the timing of welfare reform implementation may have been correlated with many things including state's anti-welfare sentiment, economic conditions and other policy changes. This study uses difference in difference method (DD) for identifying the effect of welfare reform on children's health care use and health. It includes state and year fixed effects and controls for state characteristics. Inclusion of state and year dummy variables and state level characteristic variables control for static and varying state level characteristics (e.g. state's anti-welfare sentiment and unemployment rate) as well as any correlates that are unique to each year (e.g. flu epidemic that happen across the country in particular year). Even with these control variables, however, there may still be unobserved varying state characteristics that may have affected welfare receiving children's use of health care and health after the welfare reform. To address this possibility, treatment and control groups are used. If unobserved varying state characteristics affected the treatment and the control groups in the same way, then comparing outcomes of two groups within each state provides estimates of the true effect of the welfare reform. One may naturally think of defining treatment and control groups using actual welfare status of children (i.e. participants vs. non-participants). However, this is problematic because even if they appear similar in observed characteristics, they may differ in many unobserved ways. There may be individual and welfare system level characteristics that may influence welfare participation such as the level of individual motivation and the degree of involvement by welfare officers. If these unobserved characteristics have any effect on children's health care use and health, then there will anyway be a difference in outcomes between the two groups in absence of the welfare reform (i.e. selection effect) which makes it difficult to tease out the true effect of welfare reform. Therefore to control for these selection effects (both self selection and systematic selection by welfare officers), treatment and control groups are defined as groups that are comparable in all possible ways (both observed and unobserved) other than their probability of receiving welfare without using actual welfare status that typically reflects difference in unobserved characteristics.

Identification from DD welfare reform studies comes from variation in the timing of welfare reform implementation across states. It essentially compares children before and after welfare reform for each state after controlling for other macro level changes that occurred around the same time as welfare reform by including year fixed effects and state characteristics. With the use of within state control groups, it controls for any other state level changes that may not be captured by state and year fixed effects and state characteristic controls if these changes affected both treatment and control groups in the same way.

Estimates of the effects produced by DD method are for children who were most likely affected by the welfare reform and not for the average child. The estimated effects are for the treatment group, those who were most likely affected by welfare reform, and not for the average child.

The main assumption on which the DD method is built is that children in the treatment and the control groups have the same trajectories in outcomes over time in the absence of the welfare reform. The only difference between the two groups should be that children in the treatment group were affected by the welfare reform whereas those in the control group were not. Therefore it is extremely important to select groups that are comparable to each other. Moreover, compositions of treatment and control groups should not change due to welfare reform. If group compositions change because of the effect the welfare reform had on the parameters that define the treatment and the control groups (i.e. mother's marital status and education), then comparing changes in outcomes of the two groups over time will not produce valid estimates. This is because if group compositions change, then for each group, one would be comparing outcomes of two different sets of children before and after welfare reform and therefore comparisons of these 'differences' would produce no meaningful results.

Despite the extensive measures taken to isolate the effect of welfare reform on children's health care use and health, DD estimates may still be biased for several reasons. First, assumptions of the method may be violated. The treatment and control groups may not have the same trajectories in outcomes in the absence of the welfare reform. Since there is no way to verify this, it is impossible to be completely sure that this assumption holds. Moreover, the welfare reform may have affected mother's marital status and education which changed the composition of the treatment and the control groups. Previous studies have not come to a consensus on the effect of the welfare reform on a mother's marital status. Whereas some studies show that the welfare reform decreased female headship (e.g. Schoeni and Blank 2000), some found no effects (e.g. Grogger et al 2002). There are not many studies on mother's education; one study found an association between welfare reform and decrease in dropout rates among minor mothers although this was only a correlation and not a causation (Koball 2007). Although this study follows previous studies in defining treatment and control groups, this would be a problem if welfare reform did indeed affect these key defining parameters.

Second, like all other studies that examine the effect of government policies, there is always a possibility of policy endogeneity (Besley and Case 2000). The timings of welfare reform implementation by state governments may have been intentional. States may have implemented the welfare reform with the intention of impacting children's health related outcomes. With some of PRWORA's stated goals involving improvement of children's wellbeing, it is difficult to eliminate this possibility. If implementation of welfare is indeed non-random, then estimates may not be capturing the effect of welfare reform per se but rather some other factor that is correlated with the timing of the welfare reform implementation.

Third, as mentioned earlier in the main text, there are also difficulties in isolating the effect of the welfare reform because many state level changes occurred during the welfare reform years. Even with all the controls used in this study, it is not completely clear whether AFDC waiver and TANF dummy variables will truly capture the effect of welfare reform without confounding effects from other factors. Another related issue to keep in mind is that since there may have been possible interactions that occurred between these economic and other policy changes and the welfare reform, the method used in this study will not capture these effects. Although these interaction effects by themselves may not necessarily affect the direct effect of the welfare reform, it is nevertheless important to keep in mind the possibility of other effects that welfare reform may have had in conjunction with other factors.

Fourth, if outcomes are serially correlated, without correction for the problem, DD estimates will have smaller standard errors which will lead to overestimation of t-statistics and significance levels (Bertrand, Duflo and Mullainathan 2004). This will lead to finding 'significant' effects when there is none, implying increase in type I errors.

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