ESSAYS IN CHILDREN'S ACCESS TO HEALTH CARE

by

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TABLE OF CONTENTS

LIST OF FIGURES

FIGURE

LIST OF TABLES

TABLE

CHAPTER 1

Introduction

Access to medical and dental care is critical to the health and well-being of children. Greater access to health care has been associated with increased use of preventative services, fewer avoidable hospitalizations, reductions in costly oral health problems, and better health status. Ensuring that children have access to high quality, appropriate health services has long been a public concern and a national priority, with considerable resources committed toward achieving this goal. These investments can have long term aftereffects, as recent research has established a link between childhood health conditions and health and socioeconomic status in adulthood (Case, 2005; Johnson and Schoeni, 2007). This dissertation consists of three essays aimed at broadening our understanding factors and policies that inhibit or facilitate children's access to and use of medical and dental services.

In essays one and two, I look at how family context can play an important role in children's access to and receipt of health care. This is a meaningful endeavor for several reasons. Children are dependent on their parents or other adult family members to make decisions about the quantity, quality and frequency of healthcare received throughout their childhood. Parents and other family members invest in their children's health and human capital, but these investments are subject to monetary and nonmonetary resources that can be influenced by changes to a family's composition. Family members are also

responsible for the decision making that influences how often and what type of care a child receives. These decisions are likely to be influenced by experiences with children's health needs and prior contacts with the pediatric health care system. I focus on one aspect of changing familial structure, the number and order of a child's siblings, and any resulting consequences to children's access to health care. Figure 1.1 depicts the pathways in which we might expect siblings to influence a child's access to health care.

The first essay (Chapter 2) explores the relationship between the number of children in a family and children's access to medical and dental services. A key methodological challenge examined in this study is that estimates of the effect of family size may be biased due to correlation with unobservable factors that may themselves have an independent effect on care. I employ two strategies to address this potential issue. First, I attempt to mitigate the problem of unobserved heterogeneity by conducting the analysis on relatively homogenous subgroups. Second, I instrument for an increase in family size using parental preferences for a mixed sibling-sex composition. My results suggest that the relationship between family size and children's access to health care depend on the ability of parents to schedule and coordinate visits across multiple children. For care that is driven by unplanned events, such as emergency room visits, family size tends to decrease utilization, as fixed parental resources are likely to lead to less care for any one child. However, for care in which the search or time costs are largely fixed over children, like well-child visits or having a usual source of care, I find evidence of a positive relationship with family size.

An important contribution of this research is that it disentangles the multiple pathways in which siblings might impact the family context in which decisions are made

about a child's use of health care. Grossman's (1972) model of the demand for health capital provides the theoretical framework for this work. I use the model to predict how siblings might influence a child's receipt of services through their impact on the family's resources, parent's predisposition to use health services for their children, or on parents' perception of children's need for health services. I describe how a family's health care decision making is likely to be influenced by their experiences with early born children and thus, a child's placement (ordering) in the family may confound any relationship between the size of the family and the child's access to health care. I show that the estimated relationship between family size and several utilization outcomes is sensitive to the inclusion of birth order as an explanatory variable.

Building upon the framework developed in Chapter 2, the second essay (Chapter 3) uses longitudinal data taken from the Survey of Income and Program Participation (SIPP) to further explore the independent effects siblings have on a child's utilization of medical and dental care. By exploiting the longitudinal design of the data, I parse more plausible causal effects of family size and birth order from confounding family and individual factors. First, I estimate the change in a child's utilization of health care due to the addition of a newborn sibling to the family. I use a difference-in-differences approach to account for the potentially confounding influence of a child's age on health care utilization. I also investigate the independent effect of a child's birth order on their use of medical and dental care. To reduce any bias due to heterogeneity across families and individuals, I utilize information on multiple children in the family and 2 to 4 years of panel data to examine how children within the same family differ in their age-specific utilization by the order of their birth. The use of longitudinal, within family information

allows for a more rigorous test of the relationship between a child's birth order and health care access than previous studies which largely rely on cross-sectional analysis. With this approach, I'm better able to conclude that birth order has an effect on children's medical and dental use and the resulting estimates are not biased by unmeasured factors correlated with the variables of interest.

My results indicate the impact of siblings is context dependent, as both the type of care and age of the child matter to the magnitude and direction of the relationship. The addition of a newborn to the household reduces both the likelihood and the amount of medical care a child receives. For dental care, however, I find the opposite effect, with the probability of having a dental visit increasing in family size, although this effect may be limited to the youngest children. I also establish that second born children differ from first born children in their age-specific medical and dental utilization, with medical care decreasing and dental care increasing in birth order. These results lend support to the notion that parental decision making for their children's health care is a learned skill and parents apply their experiences with their first born children to subsequent children. Parents of first born children might therefore benefit from early education on the appropriateness and limits of pediatric medical care and the benefits of early dental services.

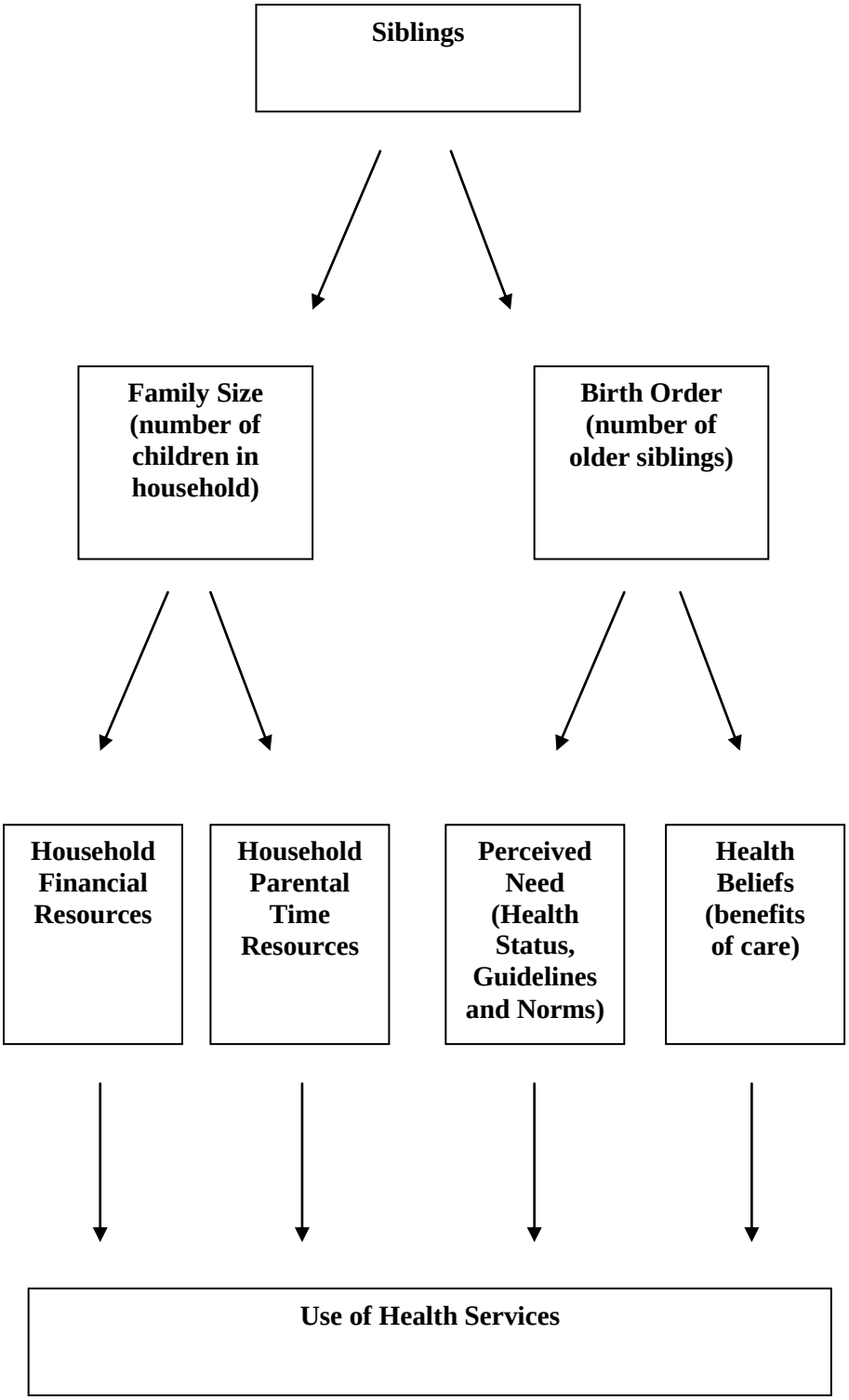
The final essay (Chapter 4), co-authored with Tom Buchmueller of the University of Michigan, uses data from the 1996, 2001, and 2004 Survey of Income and Program Participation (SIPP) to examine the effects of recent changes to states' public insurance programs on the insurance coverage of low-income children. This paper shifts the focus of the dissertation from exploring why children might differ in their utilization of health

services to evaluating the impact of strategies designed to reduce differences in children's access to health services. This essay is linked to the previous chapters in its focus on exploring factors that influence children's use of medical and dental care. The main objective of recent health insurance expansions has been to increase children's use of appropriate and timely health services as numerous studies have documented a positive relationship between health insurance coverage and various measures of access to and utilization of care. There exist large income related disparities in children's access to and use of care. Public health insurance has the potential to reduce disparities in health care and improve the health of low-income children, as preliminary findings from the ongoing Oregon study suggest.

This essay contributes to the broader literature on efforts to increase insurance coverage for low-income children by moving beyond the effects of the eligibility expansions and examining whether characteristics of state program designs impact states' ability to enroll and retain eligible children in public coverage. This is a particularly relevant health policy issue as states prepare to accept individuals made newly eligible under the expansions mandated in the Affordable Care Act (ACA). We find that SCHIP had effects on children outside those specifically targeted by the eligibility expansions and that this spillover effect was most pronounced in states that expanded existing Medicaid programs, relative to states that used separate programs. Furthermore, we find evidence that state's efforts to simplify the enrollment process and the adoption of continuous eligibility policies have significant effects on low-income children's insurance coverage. Taken together, these results suggest that some of the variation in

children's uninsured rates across states may be due to the specific design features of individual states' public insurance programs.

Figure 1.1: Conceptual Model – Family Size, Birth Order, and the Demand for Children's Health Care



CHAPTER 2

Family Size, Birth Order, and the Demand for Children's Health Care

Introduction

Because receipt of timely and effective health care is an important aspect of promoting good health outcomes, there is great interest among policy makers and health services researchers in the factors that influence the health care received by children. The family plays a critical role in determining the health care services that children receive. Children depend on their parents to make decisions about the health care inputs invested in them. Parents identify children's health care needs and determine if, and what type of, care is necessary. Constraints on parents' time or financial resources have the potential to influence the amount or type of care children receive. Similarly, parent's experience and knowledge of the uses and limits of pediatric health care may also influence the demand for their children's health services. Thus, a child's utilization of health care may depend on the total number of children in the family and his or her birth order.

The influence of a family's size on a child's social capital outcomes such as educational achievement, future earnings, and risky behaviors has been of particular interest to researchers as theory suggests a trade-off between the size of the family and child quality due to the role of resource limitations (Becker and Lewis 1973). Central to the "quality-quantity trade-off" is the assumption that parents' utility increases in both family size and child quality but is subject to a household budget constraint. Given the

constraint, the cost of increasing the quality of children (the shadow price of quality) will increase in family size as investments in quality apply to more children. Similarly, the cost of increasing the quantity of children will increase in a family's desire for "quality", as additional children will be more expensive (Becker and Tomes 1976). According to this framework, we should see a negative relationship between family size and child outcomes, as an exogenous increase in family size would decrease household investments in any one child. In fact, findings from the literature on educational achievement are consistent with this expectation – as the number of children in a family increases, academic performance declines. The most cited evidence comes from Blake's 1989 work in which she examined six national datasets over a long period of time and not only found an inverse relationship between family size and years of education attained, but the size of the effect was substantial relative to other predictors of education (Downey 2001).

While there has been considerable research on the effect of family structure on social capital outcomes, less is known about the relationship between sibling configuration and health outcomes, despite reason to expect investments in children's health capital follow a similar production function (Grossman 1972) and are subject to similar resource constraints. In this paper, I address this gap in the literature by examining the relationship between sibling configuration and children's use of various types of health services. A key methodological challenge is that the number of children in the family, which is the independent variable of primary interest, is correlated with other factors that may themselves have an independent effect of care. One potential confounder is birth order. I show that the estimated relationship between family size and several utilization outcomes is sensitive to the inclusion of birth order as an explanatory

variable. Even accounting for birth order and other observable variables, estimates of the effect of family size may be biased due to correlation with unobservable factors. I take two strategies to address this potential issue. First, I attempt to mitigate the problem of unobserved heterogeneity by conducting the analysis on relatively homogenous subgroups. Second, I implement an instrumental variable (IV) strategy, using parental preferences for a mixed sibling-sex composition as an exogenous increase in family size. An additional way that I extend the literature is by examining more carefully the mechanism by which family size may affect health care utilization by developing tests for the importance of financial constraints and parental time constraints. By examining the relationship between family size, types of pediatric care, and a family's resources available for receipt of care, I describe the extent that specific family-level factors affect the demand for children's health care and provide insight about the family resources devoted to children's health care.

Conceptual Framework

Both economic and sociological models of health care utilization predict that family size may contribute to the decisions about children's use of health care. Turning first to economic theory, according to Grossman's (1972) model of the demand for health capital, a person is born with an initial stock of health that depreciates with age but can be improved by investments in medical care or engaging in other health promoting behaviors. In this model, the demand for a child's health care is derived from the family's demand for health capital or "good health" for that child. The addition of more dependent children to a household increases the "shadow price" of providing health care

services for any one child, either through increasing the opportunity cost of parental time or by diluting the family's financial resources.

Likewise, sociological theories also predict that family size will reduce the resources available to any one child (Downey 2001; Steelman et al 2002). The "resource dilution hypothesis", as described by Steelman et al (2002), depicts the family as a unit which dispenses resources to its children, be it time spent with each child or other material resources. Each child's given amount of resources is influenced not only by the amount of resources each family unit possesses (such as household income or parent-child time), but also the number of children in the household. Given a set of finite parental resources, each additional child in the household would lead to the "dilution" of parental resources for any one child, potentially resulting in worse outcomes for each child in the household.

The extent to which this resource dilution effect reduces health care utilization by children is likely to vary across different types of health care services depending on the nature of the costs of those services. Relevant costs include not only the financial payments required but also the opportunity costs associated with identifying appropriate providers and the time cost of seeking treatment. The family will decide to incur the cost of the good as long as the marginal benefit for an addition unit is at least as great as the marginal cost of the additional unit. In some cases, it will be possible to spread certain costs across multiple children and over time. For example, the search costs associated with finding a provider will be fixed over all visits and all children in the household. Thus, we might expect a *positive* relationship between the number of children in a family and whether the children have a usual source of care. Similarly, because most pediatric

dental visits are preventive in nature and thus not time sensitive, parents with multiple children may be able to reduce the average time cost of a visit by scheduling visits for all their children on the same day. To the extent parental time is a constraint, we might expect that parents with multiple children might be more willing to take the time to schedule and take their children to an appointment when multiple children could benefit from the time spent. In contrast, such "economies of scale" will not generally be possible in the case of physician visits for acute illness and emergency room visits which are driven by unplanned, random events. Thus, for these types of services, fixed parental resources are more likely to lead to resource dilution effects. Well-child pediatric visits are likely to fall between these two extremes. Like dental visits, well-child visits are generally scheduled well in advance, so in principle parents are able to take multiple children to the doctor on the same day. However, such coordination is complicated by the fact that recommended check-up schedules vary with age and the timing is often tied to a child's birthday.

Economic and sociologic theories also suggest a possible relationship between a child's birth order and the demand for children's health care, independent of family size. The decision to seek health care for their children is influenced by parent's perception of the child's health status and the knowledge they have about pediatric medicine's uses and limits. Experience with early born children may increase parental knowledge about child health and the health care system, making them more efficient as "producers" of health for their younger children. Decisions on the type, timing or amount of health care for children born later in the family life cycle are likely to be influenced by these past experiences with children's illness or the health care system.

In theory, the knowledge gained from older children can either increase or decrease the demand for health care for younger children, though it seems most likely that the relationship between birth order and utilization will be negative. One reason is that parents of first born children might be anxious and ill prepared when it comes to the managing the health of their children, and therefore may have a lower threshold for taking a sick child to a doctor as compared to more experienced parents. Therefore, we might expect parents of later-born children to rely less on emergency room visits than parents of first-borns. Experience with older children may also affect the perceived benefit of some recommended pediatric services to rise or fall as parents accumulate experience over children. For example, pediatric guidelines recommend every child undergo a well-child visit each year. For healthy adolescents who have received their recommended vaccinations, parents might come to view these visits as providing little benefit, as they may consist of little more than finding out the child's height and weight. As they apply these experiences to later born children, the demand for well-child visits might fall.

Previous Literature

While health service researchers have examined the influence of family size on children's health care, for the most part this work has focused on the number of parents in the household (Cafferata and Kasper 1985; Case and Paxson 2001; Heck and Parker 2002; Cunningham and Ham 1994; Leininger and Ziol-Guest 2008; Gorman and Braverman 2008; Gaskin et al 2008), with only a handful of studies examining the impact additional siblings have on the care children receive.

Two recent studies by Chen and Escarce (2006, 2008) document an inverse relationship between the number of children in the family and some measures of children's use of health services. Specifically, they find that children from large families having fewer visits to a doctor or emergency room than children from smaller families. In their 2006 paper, Chen and Escarce condition on birth order. They find that higher birth order children have fewer visits and are less likely to use medications. Some studies on the determinants of childhood vaccinations also find that the probability a child has all vaccinations recommended for their age is negatively related to the number of siblings (Luman et al, 2003; Bates, 1999; Dombkowski et al, 2004a) and birth order (Bobo et al, 1987; Schaffer, 1995)

These results are consistent with the resource dilution hypothesis, though a number of questions concerning the relationship between family structure and health care utilization by children remain unanswered. First, previous studies have not sought to systematically determine how the impact of family size varies across different types of utilization behavior. As noted in the previous section, for certain types of care children from larger families may have higher utilization because their parents are better able to spread the relevant fixed costs. Second, the previous literature generally does not attempt to identify the mechanism through which family size affects children's utilization of services. In particular, to the extent that the relationship between family size and utilization reflects a resource dilution effect, it is not clear whether financial constraints or parental time constraints are most important. Third, what is often ignored in such research is that family size is correlated with difficult to observe family level factors likely to be related to children's health outcomes. Specifically, if pediatric care giving is

a socially learned behavior, health care experiences with early born children are likely to influence parent's perception of the benefits of medical and dental care and therefore, use of services for subsequent children. If parental experience has an independent relationship with children's health care use and is not empirically accounted for, the estimated relationship between family size and utilization might be biased. Additionally, all prior studies treat the number of children in a family as exogenous with respect to the demand for health care. To the extent that the same preferences and other unmeasured factors that determine parental choices about family size also influence the decision to seek health care, this assumption will not be justified and the observed correlation between family size and utilization will not represent a causal effect.

My analysis extends the literature by improving upon these limitations. First, I estimate the relationship between family size and birth order with a variety of medical services that differ in the relative importance of fixed and variable costs. Second, in addition to estimating the overall relationship between utilization and family structure, I estimate models on stratified samples in order to get at the relative importance of financial constraints and time constraints. Third, to control for parental experience, I empirically account for the correlation between family size and a child's birth order, a proxy for parental experience with pediatric health care. To allow for the potential endogeneity of family size, I estimate instrumental variables models that exploit parental preferences for a mixed sibling-sex composition as a source of identifying variation.

Data and Methods

Data Source and Study Sample

The data used in this analysis are from the National Health Interview Survey (NHIS), a continuing cross-sectional survey that collects information on the health status and use of health services by the civilian, non-institutionalized US population. The NHIS core questionnaire collects information on each living member of the sampled household and additional information is collected for one randomly selected child under the age of 18 years (Sample Child Core). It is from this section that detailed information on children's access and use of health care services (and child's health status) are obtained. For the main analyses, I combine the 1998-2005 waves of the NHIS and focus on children younger than 16 who were administered the sample child questionnaire (n=90,632). I then limited this sample based on several criteria. Because my objective is to describe how family size might impact children's access of medical and dental care by its effect on the child's primary caregivers, I excluded households in which resources or care giving duties might be shared across families. Specifically, I exclude 979 children living in multi-family households (n=89,653) and include only biological children whose mother or father was the household reference person for the survey (n=77,874). I exclude an additional 7,490 children whose mother (or father if in a father-only household) is older than 45 (n=70,384)[1] or who live in households with more than six children (n=70,228). Finally, for consistency across models, 1,189 children that have missing values for any of our four main health care measures that are available for children of all

[1] Because the NHIS does not allow me to match individuals across households, I use the age of children currently living in the household to construct birth order measures. In an effort to limit misclassification of this variable due to children aging out of the household, I focus on younger families. Adding children from these families to the sample does not materially change the results presented below.

ages are dropped.[2] These exclusions produce a final analytic sample of 69,039 children to assess the relationship between sibling configuration and health care access and use.

Sibling Configuration: Family Size and Birth Order

The NHIS does not include data on fertility history nor does it provide a measure of number of children in a family. To determine whether family size plays a role in determining the health care resources allocated to any one child in the household, I operationalize my measure of number of children by summing those individuals in the household under the age of 18 who are reported to be the child of the reference person or their partner. I do not count children aged 18 or older because their parents are less likely to be making health care decisions for them. To construct the measure of birth order, for each household I ranked by age and birth date those members who reported they were the biological child of the household reference person. Here, biological children over the age of 18 still living in the household *are* included to calculate a child's order of birth.

Table 2.1 provides information on the sibling configuration of our sample of children. A vast majority of children reside in families with one, two or three children, with two-child families as the most frequent (42 percent). Looking at larger family sizes, almost one of ten children live in families with four children while four percent of children are in families with five or six children.

Outcome Measures

The analysis focuses on several different measures of health care access and use. The American Academy of Pediatrics and the national *Bright Futures* initiative recommend a series of regular well-child visits from birth to age 21 (American Academy

[2] The four measures are well-child visits, office visits, emergency room visits and having an office based usual source of care. Dental visits are only available for children aged two or older (n=58,591). For those analyses, we drop an additional 282 children aged 2 or older who are missing this measure.

of Pediatrics 2009).[3] These recommendations include nine visits during the first two years of life followed by annual visits through adolescence. To assess access to basic primary care, I create an indicator for whether a child had at least one well-child visit over a 12 month period. Table 2.2 shows that roughly three-quarters of children meet this guideline. A second related measure is an indicator for having at least one office visit during a given time period. While not explicitly preventive, as is our first measure, having at least one visit can be interpreted as meeting minimum preventive guidelines. Almost 90 percent of children report at least one office visit in the past year. A third measure of realized health care utilization is whether the child had gone to a hospital emergency room (ER) in the past year. Not surprisingly, ER visits were much less frequent than well-child office visits, with just over 1 in 5 children having such a visit in the past year. For children aged two and older[4], I also assess whether having at least one dental visit in the past year is associated with family size. Like adult dentistry, pediatric dentistry recommends a check-up every six months for children starting at the age of one or their first tooth, although a single annual dental visit may be a more customary practice (American Academy of Pediatric Dentistry 2010). Among children aged two and older, 73 percent of children had least one dental visit during the prior year.

It is recommended that all children have access to a medical home, a family centered, coordinated, regular source of primary pediatric care (American Academy of Pediatrics 2002). Studies have found that an established medical home may positively influence a child's health outcomes: children who have a medical home are more likely to

[3] Prior to 2008, AAP recommendations did not call for an annual well child visit for children aged 7 and 9. The 2008 update to the AAP's G*uidelines for Health Supervision of Infants, Children, and Adolescents* added new recommendations for preventive care visits at 30 months and 7 and 9 years of age.
[4] The NHIS did not collect dental care access and use information for children younger than two years of age prior to 2000, therefore, we limit of analyses of dental care to children two or older.

be up to date with immunizations or have their immunization status reviewed (Briss et al 2000; Dombkowski et al, 2004b). Therefore, as an additional measure of access, I examine whether a child has an office-based usual source of care.[5] Approximately 75 percent of sample children were reported as having an office-based usual source of care.

Empirical Strategy

Typically, research on the relationship between family size and utilization of pediatric health care starts with a framework such as:

$$visits_{it} = \alpha + \delta^{*}(Family\ Composition) + \varphi^{1}X_{it} + \varepsilon \qquad (1)$$

where *visits* may be the count or probability of having health care visits for child *i* at time *t*; family size is typically specified as a linear term or as dummy variables indicating child *i* has no, one, two, etc. additional siblings at time *t*; and *X* includes family and child controls for child *i* at time *t*. As discussed above, one characteristic of the child that is often ignored is the child's birth order. As an empirical matter, because a child's use of health care may be influenced by a parent's accumulated experience gained with prior children, any model that explores the relationship between family size and health care and doesn't take into account birth order could be misspecified. Because larger families have proportionally more later-born children than smaller families, as shown in Table 2.3, a negative relationship between family size and a child's health care might be due to resource constraints, or it may be due to a change in parent's assessment of the benefit of such care. A model that omits birth order is unable to say which relationship is driving the results.

[5] The NHIS asks whether there is "a place that child usually goes when sick or you need advice about child's health?" Affirmative responses lead to a follow-up question regarding "What kind of place is it?".

Therefore, to estimate the effect of family size on a child's access to and use of health care, I use probit models of the form:

$$\text{Prob}\,(y_i=1) = \Phi(\beta_{1-5}*NKIDS_i + \phi_{1-5}*Birth\ Order_i + \gamma^1 F_i + \gamma^2 C_i) \qquad (2)$$

where

$$y_i = \begin{cases} 1 \text{ if child } i \text{ had outcome in previous 12 months} \\ 0 \text{ otherwise,} \end{cases}$$

and $\Phi(\cdot)$ is the cumulative normal distribution. The main explanatory variable is *NKIDS*, and will be entered into the model as dummy variables because the relationship between family size and a family's resources may be non-linear: children with no siblings lose a larger share of parental resources (e.g. from 100 to 50 percent) than children with many siblings (e.g. from 33 to 25 percent). β_{2-5} will therefore be our main coefficients of interest – the regression-adjusted difference in the probability of the outcome between the child of that family size and the referent group (one child household). To account for the potential independent effect of a child's birth order on our outcomes, birth order is included, also using dummy variables to indicate the child is the first, second, third, fourth or fifth or later child. Although the two sibling factors are correlated, since family size is defined by number of dependent children in the household (age less than 18), not the number of children ever born to the parents, the two are not perfectly related. As shown in Table 2.3, the sample provides adequate off-diagonal observations to consider both factors.

To control for observed household and individual factors, the vectors *F* and *C* includes the following covariates: age (entered as single year dummy variables), gender, race/ethnicity, whether the child was from a twin birth, whether the child has a sibling

who is a twin (not including own twin), family structure (two-parent, mother-only, father-only family), an indicator for at least one parent not having full time employment to measure parental-time available for accessing health care services, fair or poor health status, presence of a health condition that limits the child's activity, mother's age and education[6], measures of a household's income to the federal poverty threshold, health insurance status of the child (uninsured, private insurance, public insurance, missing insurance status), region of residence and year of the NHIS survey.

Testing for Unobserved Heterogeneity

One potential limitation with the approach just described is it treats family size as exogenous. The number of children in a family might be related to unobserved family-level characteristics that also affect the health care decisions parents make for their children. That is, parents who *choose* to have large families might also differ in how they demand health care services for their children. If family size is correlated with unobserved characteristics in the error term, ε, a naïve probit will yield biased estimates of the causal effect of family size. In fact, our data provide evidence that family size is related to some key *observed* individual or family-level characteristics. Table 2.4 provides evidence of a relationship between family size and four observable characteristics of the family: child's health status and age and parent's educational attainment and age. In our sample, children from smaller families tend to be younger and healthier and have more educated parents than children in larger families. While the regression models outlined above control for these factors, our results might be subject to omitted variable bias if, for example, older or more educated parents are different not just

[6] In cases when the mother is not in household or information on the mother is missing, I use father's age and education.

on family size but also in terms of unobserved traits that influence how they demand pediatric care.[7]

There are both theoretical and empirical reasons we might expect each of these factors to be related to children's use of health care. Child health is indelibly linked to age specific health care utilization. Children with serious health needs will tend to have higher utilization of care due to a greater frequency of illness events. Parents of such children may also be very careful to make sure that their children are up to date with preventive or follow-up care. Therefore, a primary concern is that some unmeasured component of a child's health status might bias the results. If children from larger families are less healthy than children from smaller families, we might worry that any relationship between family size and utilization might be confounded by child morbidity. Additionally, family level characteristics that are related to family size might also be importantly related to children's utilization of health care. For example, we might expect more educated parents to have fewer children, if only because they start families later than parents who do not attend college. If more educated parents are more trusting or knowledgeable of pediatric guidelines, we might expect them be more likely to arrange for timely and appropriate care. Similarly, parental age might also be a marker for experience or knowledge about the health care system. If this parental "knowledge" or health belief is not accounted for, we might expect a positive relationship with smaller family size and outcome measures targeted at pediatric guidelines (well-child visits,

[7] Another potential concern is the sample size. If the sample does not contain many observations where family size changes independently of these other important characteristics, it will be difficult to say how independent changes in the number of children in the household influence children's access to care.

dental visits, and usual source of care) and, perhaps, a negative relationship with emergency room visits, which might be a sign of unnecessary care.

To address concerns that unobserved factors that affect family size might also affect utilization, I examine the robustness of the findings by re-estimating the models on alternative samples, attempting to create more homogenous groups by the age of child, child health status, parental education and family structure. Specifically, I re-run the models on alternative samples based on (1) the age of study child, dropping the youngest children from our sample, (2) child health, limiting the sample to children in excellent or very good health, (3) splitting the sample by parental education (high school or lower educational attainment and some college or more educational attainment) and (4) parental age (younger than 35 and 35 or older). By subgrouping the sample among observed characteristics, I attempt to reduce unobserved heterogeneity and the likelihood that the results are being driven by omitted variables correlated with both family size and children's access to and use of health care.

IV Strategy

While stratifying the data in this way reduces the potential bias from heterogeneity, if there remain unobserved factors that affect both family size and parental demand for their children's health care, the results will still be subject to bias. For this reason, I supplement the probit results for the full sample with results using an instrumental variable (IV) strategy based on variation in family size that is more plausibly exogenous. I borrow an approach used in previous research estimating the effects of family size on parental labor market outcomes (Angrist and Evans 1998; Angrist et al 2005) and children's education outcomes (Currie and Yelowitz 2000; Black

2005; Conley and Glauber 2006; Black et al 2010) and exploit parental preferences for a mixed sibling-sex composition to instrument for variation in family size.[8] As described in Angrist and Evans (1998), if parents prefer a mixed sibling sex composition, those parents who have two same-sex children may be more likely to have a third child than similar parents with two opposite sex children.[9] Because sibling sex composition is essentially randomly assigned, for families with two children having two boys or two girls can be used as an instrument for family size. Specifically, I use the variable *same sex* – an indicator that the first two siblings were of the same sex – to instrument for family size.

Adopting this strategy requires changing the sample from one that includes all children in the NHIS to a subsample consisting of families with at least two children. In addition, to isolate the effect of having an additional child due to the sibling sex mix of the first two children, I limit the sample to families with two or three children.[10] Because of the use of a binary measure for the endogenous family size variable, to estimate the impact of moving from a family size of two to three children due to sibling sex mix, I use two-stage residual inclusion estimation as suggested by Terza (2008). In the first stage I use a probit model to regress the endogenous family size variable, more than two children, on the exogenous instrument, *same sex,* and the full vector of family and child characteristics described in the previous section.[11] I obtain the generalized residuals

[8] One other possible exogenous shock to family size is twin births, which are largely unplanned events that increase the size of the household. Unfortunately, my dataset does not contain enough twin births to make this approach feasible.

[9] A descriptive look at the data suggests this to be the case: parents with two siblings of the same sex are nine percentage points more likely to have a third child than parents of mixed-sex sibling (38 percent versus 29 percent).

[10] I also exclude an additional 2,755 families that have non-biologic children living in the household.

[11] In addition, to control for order of birth, an indicator variable for whether the child was the second (versus first) born is included.

(Gourieroux et al 1987) from the first-stage regression, û, and include as additional regressors in the second stage probit model, along with the endogenous family size variable and all the exogenous control variables. The endogeneity-corrected coefficients provide estimates of the average effect of having more than two children for parents affected by the sibling sex mix of their first two children. Because of the two-stage econometric approach using an estimated regressor, I use bootstrapping with 1,000 replications to obtain the standard errors.

Descriptive statistics for the instruments, exogenous measure of family size, and selected sample characteristics are given in Table 2.5. For these analyses, the family size variable of interest is the dummy variable for having more than two children (*More2*).

In order for sex composition to be a valid instrument and produce consistent estimates, sibling sex-mix must be uncorrelated with the error term in the second stage equation. While I am unable to estimate this relationship, I can compare the *observed* characteristics of household's sibling sex-mix (same sex versus opposite sex) by estimating the probability of having same sex siblings using our vector of control variables. For the most part, the results suggest no differences across groups (results not show). However, I do find that lower income households are more likely than higher income households to have same sex siblings and mothers aged 35-39 were more likely to have same-sex children than mothers aged 30-34 or 40-45, although these results are rather small in magnitude. The estimation strategy described above includes controls for all covariates, including household income and mother's age[12].

[12] The other requirement for a valid instrument is that sibling sex-mix must not directly affect our outcome measures. In the conceptual framework outlined above, I hypothesize that parents may accumulate knowledge the medical care experiences of their older children. If having, for example, a boy at first birth somehow influences how a parent would access health services for a boy at second birth *differently* than if

Testing for Resource Constraints

The conceptual framework suggests that both parental time and financial resources may be affected by family size. While the empirical approach outlined above can assess the impact of family size on the demand for children's health care, it is unable to provide an explanation for *how* size affects a family's decisions to seek care. To the extent there is a relationship, we would like to know whether family size influences the demand for care via its impact on a family's financial or time resources. In addition, if family size does impact health care utilization through a dilution of household resources, it is more likely to matter to children from families with less available resources. To provide insight on the relative importance of financial and time costs across measures of children's utilization of health care and test for heterogeneous effects of family size by observed family resource characteristics, I re-run the models, first testing whether family size has a differential effect on the outcomes by the child's insurance status (no insurance versus having insurance coverage), then by the family's parental structure (single parent versus two parent household).[13]

A key factor determining the financial cost of health care is insurance status. For insured children, the financial cost of a visit will likely be minimal as most insurance plans will cover a majority of the cost of most pediatric care visits. However, families of an uninsured child will face the full cost of such visits. To test for differential effects of family size by a family's available financial resources for health care services, I interact

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the second child was female, our IV estimates might be biased. However, potential differences by gender in general medical care needs are more likely to emerge as children age and reach sexual maturity. Because the sample is limited to children younger than 16, I expect that for this sample of children, health care services and needs are likely to be similar, regardless of gender.

[13] The specification is essentially the same as outlined in section III above, other than the addition of interaction terms.

the family size dummy variable with an indicator that the child is insured.[14],[15] In order to further isolate differences in family financial resources, I attempt to hold constant parental time constraints by re-estimating the interacted model for the sample of two-parent families. By doing so, I interpret a differential effect of family size by insurance to indicate that family size is linked to the outcome through its effect on financial, rather than parental time, constraints.

Single parent families are more likely than two-parent families to have a difficult time arranging and taking children to an appointment due to time constraints. To assess whether any link between family size and our outcomes can be explained by the amount of time parents have to devote to a child's health care, I interact family size with an indicator for two-parent families (versus mother-only or father-only family). Again, I try to isolate differences in parental time from potential differences in financial resources by re-estimating the model for a subsample, this time restricted to children with insurance. The idea here is to create a sample homogenous in their financial resources to isolate the differences in parental time. I interpret a differential effect of family size by parental structure as providing evidence that parental time is a relevant mechanism through which family size affects the demand for children's health care.

All data analyses were performed with STATA 10. In order to account for the NHIS complex sample design and accurately estimate variance, the complex design variables (STRATA, PSU and sample weights) provided on the public use files are

[14] Another possible strategy to test the impact of financial constraints would be to interact household income with family size. However, using income to differentiate families by their ability to pay for their child's medical care is not ideal due to the role of insurance and the availability of public insurance for low-income families.

[15] The insured group includes children with public insurance and children with private insurance, two groups that might differ in their out of pocket costs. However, the general results were unchanged in models which estimate the differential effect of family size separately for these two groups, therefore I combined the two.

incorporated in all analyses to allow for design, non-response and post-stratification adjustments (NHIS, 2005).[16] All statistical analyses were performed using STATA‟s survey data commands (svy); standard errors were calculated using Taylor-series linearization.

Results

Before turning to the multivariate regression results, it is useful to examine the unadjusted relationship between family size and birth order and the various access and utilization outcomes. Table 2.6 describes the proportion of children with each health care outcome by the number of children in the family (top panel) and a child's birth order (bottom panel). Looking first at number of children, there is a strong, negative monotonic relationship between family size and the outpatient health care outcomes – well-child, doctor office, and emergency room visits – with children from larger families being less likely to have a given health care visit. However, for dental care and having an office based usual source of care, there is suggestive evidence of a positive effect of additional siblings among smaller families. These results suggest that family size may have a meaningful effect on children's utilization of care. For example, a child with two additional siblings is approximately five percentage points less likely to have a well-child, office and emergency room visit than an only child. For dental visits, we see evidence that only children might be at a relative disadvantage as they are five to seven percentage points less likely to have a dental visit in the past year compared to children with one to three additional siblings. Turning next to birth order, there is some evidence of a negative relationship between birth order and use of health care. In general, other

[16] The results presented below are largely unchanged in models that do not account for the NHIS complex sample design.

than ER visits, the probability of each outcome declines in birth order, although the magnitude is not as large as that with number of children. While this table gives a good sense of the underlying trend between our outcomes and sibling factors, they do not control for individual or family characteristics that may affect children's health care use, nor does it address the potential confounding effects of family size and birth order. Next, I turn to exploring the relationship using multivariate regression models.

Table 2.7 presents the main results from probit models for each health care outcome. In order to facilitate interpretation, all results are reported as average marginal effects to convert results from the probit regression models into percentage point impacts.[17] Results in the odd columns are from models that do not control for birth order. Models that include birth order (full models) are reported in the even columns. The family size effects from these fully controlled models are also presented in Figures 2.1 and 2.2 to better observe the nonlinear and heterogeneous relationship between size and the five different health care outcomes.

When birth order is not controlled for in the model, the results for office visits (column 3) and ER visits (column 5) are consistent with the unadjusted results discussed above and support the resource dilution hypothesis: there is a significant negative relationship between the number of children in a family and the likelihood that a particular child will have either type of visit. Similarly, the multivariate results for well-child visits mirror the unadjusted relationship described in Table 2.6 and suggest a negative relationship with the number of children, though only the coefficient for the largest family size (5 or 6 children) is statistically significant. In contrast, the results

[17] In effect, I take the difference between the average predicted probability when all children are coded as living in the reference category (single child, first born) and the average predicted probability when the child is living in the category of interest (e.g., two child families or second born).

reported in column 7 indicate a positive relationship between the number of children in the family and the probability of at least one dental visit over the preceding 12 month period. These results are consistent with the idea that with care that can be easily scheduled in advance, parents can spread some of the time costs over multiple children. As noted previously, we might also expect similar economies of scale with respect to the search costs associated with finding a usual source of care. However, when I do not control for birth order the results for this outcome do not suggest a clear relationship with family size (column 9). The coefficient on the indicator for having three children is positive and significant but none of the other family size coefficients are significantly different from the omitted category, which consists of households with only one child.

The office and emergency room visit results are consistent with Chen and Escarce's (2006) findings that an increase in family size is associated with a reduction in office visits and emergency room visits. Similarly, studies on childhood vaccinations have found a negative relationship between the number of children and the probably of vaccination. While the measure of preventive care is more general than the outcomes examined in those previous studies, the results in column 4 are consistent with a negative effect of siblings on the likelihood of preventive care.

However, these broad conclusions do not consistently hold once I control for a child's birth order (columns 2, 4, 6, 8, and 10). Specifically, there is evidence that birth order has a negative and significant effect on three of our five outcomes: well-child visits, doctor office visits and having an office based usual source of care. The overall interpretation of the relationship between family size and these three outcomes change once the confounding between these two measures are taken into account.

Looking first at office-visits (column 5), among smaller families (four or fewer children), the observed negative coefficients on family size are significantly reduced and no longer statistically significant once the independent effect of birth order is accounted for, suggesting little difference in access to care among children with no, one, two, or three other siblings. For larger families, however, there is some evidence that size negatively affects utilization, with a small reduction in the likelihood of a visit for a child in five or six-child households (but only compared to only children). Looking at this result graphically (figure 2.1) the non-linear relationship between family size and office-visits is readily apparent, suggesting that it is only among the very largest families that we might see the number of children impacting a family's resources in a way that would lead to lower utilization.

The omission of birth order in models predicting well-child visits is also important in interpreting the effect of family size. Models that do not control for birth order might suggest that family size is associated with lower utilization. However, once I account for a child's birth order, there is weak but suggestive evidence that family size is associated with a small increase in the probability that a child had a visit in the past year. For example, compared to an only child, a child from a 3-child family is 1.4 percentage points more likely to have a well-child visit in the past year. This positive relationship across smaller families (less than five children) is shown graphically in Figure 2.1. This result suggests that smaller households may be able to achieve some economies of scale in scheduling preventive visits across multiple children, however, the magnitude of the effect is rather small (2-percent change).

Turning to emergency room visits, there is little evidence that a child's birth order has a meaningful effect on the probability of a visit. Comparing columns 5 and 6 in Table 2.7, the family size effects are robust to the inclusion of the birth order controls. Looking at the full model results in column 6, there remains a sizable, negative relationship between family size and the probability of a visit. For example, compared to only-children, children in three- and four-child households were 4.0 and 4.5 percentage points less likely to have an emergency room visit in the past year. The point estimates suggest that having additional siblings in the household reduced the likelihood of having an emergency room visit by 10-20-percent. Across all measures, the negative relationship between family size and emergency room visits was far and away the strongest, and the most consistent with the resource dilution hypothesis. Although, as seen in Figure 2.1, the effect of family size flattens out among larger families with no difference between three, four, five and six child families.

The effect of including the birth order controls in regressions predicting having an office based usual source of care is also notable. While one would interpret results from the unadjusted model as suggesting little or no influence of family size on access (column 9), after accounting for the observed negative relationship between birth order and having a usual source of care, there is now evidence of a positive effect of family size (column 10). Compared to only-children, children from two-, three- and four-child households were 1.2 to 2.8 percentage points more likely to have an office-based usual source of care, corresponding to a 2 to 4 percent increase in the likelihood of a visit. Shown graphically in Figure 2.2, the results also suggest the relationship is increasing through three child families: two child families are more likely to have an office-based usual

source of care than only child families and three child families more likely than two child families.

Lastly, like emergency room visits, there is little evidence that the relationship between family size and the probability of a dental visit is confounded by birth order. Looking at the dental visit results (column 8), the coefficients on the birth order dummy variables are all small and not statistically different from zero, and the family size effects are for the most part unchanged by the inclusion of the birth order variables. These family size effects in column 8 provide evidence that utilization is increasing in family size, consistent with an ability to spread time or search costs across multiple children. Specifically, in families with two-, three-, or four children, larger family size is associated with an increase in the probability of having a dental visit. Compared to only children, additional siblings increase the likelihood of a visit by 1.7 to 2.4 percentage points. However, as seen in figure 2.2, there appears to be a limit to parent's ability to "bundle" dental services across children, as there is no difference between families with two, three or four children. Having an additional child to spread the cost of a dental visit increases the likelihood that a child will have a visit, but having more than one additional child makes it no more likely.

Overall, the results suggest a statistical and meaningful relationship between family size and children's use of health care. For four of our five outcome measures, inclusion of family size improved the overall fit of the model ($p<0.10$). Additionally, while the magnitude of the effects might be rather small when compared to major access facilitators like health insurance and income household income, the family size effects

are similar in magnitude to the differences I find across racial groups and in one- versus two-parent families (results not shown).

The findings also illustrate that family size may impact different health service outcomes differently, as suggested by the conceptual model outlined above. For instance, utilization of some types of care may decrease as family size increases (emergency room visits), but use of other types of care may increase with family size (dental visits, having a usual source of care). Additionally, the results suggest a non-linear relationship between family size and utilization of health care, as the effect of increasing family size for relatively small families differs from that of larger families. Finally, the results from table 2.7 reveal the importance of taking a child's birth order into account when accessing the relationship between family size and children's health care outcomes.

Subgroup Analyses – Robustness Tests

To examine the robustness of the above findings, I re-estimate the models using four alternative subsamples. I do this to ease concerns that the models are picking up the effect of some important omitted variables correlated with both family size and children's use of health care. Appendix Tables 2.A1-2.A5 present marginal effects for the main models (results repeated from Table 2.7) and four subsamples: (1) dropping children aged 0-2, (2) limited to children with excellent or very good health status, splitting the sample by (3) parental education attainment (less than college, college or higher), and (4) parental age (less than 35, 35 or older). Using these subsamples yields very similar results to those of the full sample. I find evidence that family size is positively related to dental visits and having an office based usual source of care, negatively related to emergency room visits, and little evidence to suggest a robust relationship with office

based visits except in the largest of family sizes (a negative relationship with visits in families with five or more children). Overall, the consistency across estimates for these outcomes offers support for the full sample estimates on the relationship between family size and use of health care.

The one outcome that was somewhat sensitive to sample restrictions was well-child visits, with some samples suggesting a positive relationship between family size and the probability of a visit. Specifically, when the sample was limited to parents with low educational attainment and older parents, family size exhibited a positive relationship with the probability of having a visit. For example, in the sample limited to parents with high school or lower education, children with one, two or three siblings were 1.4 to 2.9 percentage points more likely to have a well-child visit than only children. Similarly, in the sample restricted to parents 35 or older, there is a positive monotonic relationship with family size and visits up to families with four children. However, in the high parental education or younger parent samples, there was little evidence of any meaningful relationship between family size and well-child visits.

IV Results

I next turn to models that exploit parental preferences for a mixed sibling-sex composition to instrument for family size. Table 2.8 presents the probit, 2SRI and first-stage coefficients for our sample of households with two or three children. It is important for the instrument to explain as much of the variation in our family size variable as possible or else the variance in the 2SRI estimates will be much larger than in the probit model and make it difficult to conclude much from the exercise. In addition, if the instrument is weakly correlated with the family size variable, the 2SRI estimates can be

vulnerable to substantial sample bias. Examining the first stage results in the top row of column (2), note that having two same sex children is strongly related to having a third child. The first stage results suggest that having two same-sex children increases the likelihood of having one more child by about 6.7 percentage points. The f-statistic of the null that the coefficient on the excluded instrument is equal to zero is over 140, well above the Stock-Yogo (2005) weak identification test critical values, suggesting the instrument is not weakly correlated with the measure of family size.

Turning first to the probit estimates, we note that the results estimating the impact of going from two to three children are consistent, both in direction and magnitude, with those given in table 2.7 for a similar change. Moving from a two to three child family is negatively related to the probability of having an emergency room visit and positively related to both the probability of having a well-child visit and having an office based usual source of care ($p<0.01$). Similar to the results reported in table 2.7, there is no significant difference in office or dental visits between children with one and two other siblings.

Turning to the 2SRI estimates, looking down column 2, one striking finding is that, while the 2SRI estimates are imprecisely estimated, the coefficients suggest a larger effect of family size on children's use of health care than those in column 1. For example, the result for well-child visits suggests that having a third child increases the probability of having a visit by approximately 15.6 percentage points, compared to a 1.2 percentage point estimate from the probit model. Likewise, probit estimates appear to significantly understate the effect of family size on emergency room visits and the effect of moving from a two to three child family on dental visits. For emergency room visits,

the 2SRI estimate of 8.3 percentage points is almost 4 times that of the probit estimate. Similarly, while the probit estimate in column 1 suggests no significant impact of an increase in family size on having a dental visit, the 2SRI estimate on dental visits suggests a strong, negative effect of moving from a two to three child family: the effect of having a third child due to same sex-mix decreases the probability of having a dental visit by 15.5 percentage points. While this result is contrary to the general finding above that dental visits were found to increase in family size, remember that we are comparing two to three child households. In the main models, there was a substantial increase in the probability of dental visits from one to two child families. While children from three and four child families were also more likely to have a dental visit than only children, they were less likely than two child families (although not statistically significant, as they are using the IV approach). The 2SRI estimate for office based usual source of care is also larger than the coefficient from the probit model, although it is imprecisely estimated. The 2SRI model also lends support to the conclusion that increasing family size by one child has little effect on the probability of office visits among smaller households (less than four children).

One issue with these results is they are not necessarily representative of the impact of family size on children's health care across all families since we are restricting the sample to families with at least two children and estimating the impact of going from two to three-child families. That said these results seem to lend support to the general conclusions on the direction of the relationship between family size and children's use of different types of health care and suggest that the magnitude of the coefficients from the multivariate regressions may understate the causal effect.

Subgroup Analyses – Testing for Resource Constraints

Unlike previous studies, I find little evidence of a negative relationship between family size and our outcomes of children's utilization of health care, other than emergency room visits. While this might be the case for the average family, certain families might be more vulnerable to the constraints family size can place on available resources. In particular, we might find a differential effect of family size by whether the child is insured or is in a single parent household, as these families might have less financial or parent time resources available for their children's health care. Additionally, while the previous results suggest that the number of children in a family affects some measures of children's health care use, they do not identify the nature of the cost of larger families. For example, while the results in table 2.7 indicate a negative relationship between number of children and the likelihood that a child will have an emergency room visit in the past year; it is unclear whether it is financial or parental-time resources that are constrained by additional children, leading to the observed relationship. To try to better understand the relative importance of financial constraints and time constraints, I run models on samples that are stratified first by the child's health insurance status and then by whether there are one or two parents in the household. Because the results from the IV model are imprecise and do not allow us to estimate the effect of family size across the full range of possible values, I return to estimating probit models that do not account for the endogeneity of family size.

Table 2.9 presents the impact of family size on the five outcomes by whether the child has health insurance.[18] Looking first at results for the full sample (left three

[18] The NHIS does not contain a separate question on dental insurance; therefore, I use health insurance to proxy for having dental coverage. For publicly insured children, this should match in almost every case as

columns) we see some evidence that among children without insurance, family size might negatively impact three of the five outcomes: well-child visits, office visits and dental visits. For example, looking at the relationship between family size and the probability of having an office visit, among children who have health insurance, there is no evidence that additional children reduce the likelihood of a visit as the point estimates for this group are all small and not statistically different from zero. If family size influences children's utilization of health care through its impact on financial resources, this result isn't that surprising, as insurance will generally pay a significant portion of the cost of each additional pediatric visit. However, among children without insurance, there is an indication that children in larger families are less likely to have office visits than those in smaller households. We see a similar negative effect of family size on well-child and dental visits among children without insurance. Unlike results for well-child, office and dental visits, there were no notable differential effects of family size by insurance on the probability of having an emergency room or the probability of having an office based usual source of care.

Results for well-child, office and dental visits are plotted in figures 2.3, 2.4 and 2.5 to better illustrate the effect insurance has on the relationship between family size and these outcomes. Coefficients on number of children in the family for each group of children are presented, with one-child households as the reference group. Although the results are imprecise, for each outcome family size has a negative effect for children

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dental care is included in the Early and Periodic Screening, Diagnostic, and Treatment (EPSDT) services that must be covered by the Medicaid program and, while not required under the original legislation, nearly every state included some coverage of dental services in its CHIP program. However, for privately insured children, this approach will overestimate the number of children with insurance, as take-up of dental coverage lags behind that of health coverage. According to the Bureau of Labor Statistics' Employee Benefits Survey, between 1999 and 2006 roughly half of all workers participated in an employer-sponsored health plan, but only about one-third participated in an employer-sponsored dental plan.

without insurance, but no effect for children with insurance. For example, looking at office visits in figure 2.4, the relationship with family size is relatively flat for children with insurance but suggests a negative gradient for children without insurance. Children with one sibling are two percentage points less likely to have a visit than only children; children with three siblings 3.2 percentage points (not statistically significant) and children with four or five siblings 9.3 percentage points less likely to have a visit than only children. Figures 2.3 and 2.5 show a similar negative relationship of family size and well-child and dental visits for children *without* insurance, but little evidence of a negative slope between family size and the probability of a visit for children *with* insurance.

For large families, these results suggest a difference in the effect of family size on the probability of a visit by a family's financial resources, as measured by the child's insurance status. However, there may be unobserved factors related to both insurance status and children's health care that might contribute to these differences, making it difficult to suggest that the observed negative relationship is due to the impact family size has on financial, rather than time, constraints. To better isolate financial differences across families, I re-ran the interacted models on the subsample of families more likely to be homogenous in parental time resources: two-parent families. Families with two-parents are less likely to have time constraints, and any differential effect of family size by insurance is more likely to be attributable to the effect of family size on a household's financial resources. Results from this subsample are reported on the right three columns in table 2.9 and are consistent with the findings from the full sample: there is evidence of a steeper decline by family size in the probability of having a well-child, office and dental

visit by insurance status. There is no evidence that the family size gradient by insurance coverage disappears once adjusting for potential differences in parental time, further suggesting that financial constraints matter for some families.

Taken together, these results suggest that among financially constrained households, increased family size may negatively impact some measures of utilization. For children without insurance, our results for well-child, office and dental visits are consistent with the resource dilution hypothesis, the more children in the household the less resources available to any one child. For the majority of our sample, having insurance may protect families from any impact that increasing family size might have on a family's financial resources devoted to their children's health care. For uninsured children, however, roughly one-quarter come from families with three or more children and may be at risk for reduced utilization of medical care.

Next, I test for whether parental-time constraints also play a role in the relationship between family size and children's health care. Results interacting family size by whether the child is in a one-parent versus two-parent household are presented in Table 2.10 with findings for the full sample shown in the left three columns. Looking at the first two panels, there is little evidence of a differential effect of family size by number of parents for either well-child or office visits, except perhaps for the largest of families and office visits, although the difference across estimates by the number of parents is not significant. However, for emergency room visits (third panel), the results suggest a steeper decline in the probability of a visit by family size for children in single-parent households compared to two-parent households. Specifically, the negative

coefficients for children in three- and four-child families are larger for children in one-parent households than those in two-parent households.

Turning to children's dental visits and having an office based usual source of care, the main results presented in table 2.7 above suggested that family size might be positively related to these two outcomes. In testing for differential effects of family size by parental time, the results presented in the fourth and fifth panels suggest that the positive effect of additional children may be limited to two-parent households. For example, among children from single parent households, family size has no apparent relationship with the probability of a dental visit. These families might be so particularly constrained by time that no amount of "bundling" dental visits is likely to change their demand for dental services of any one child in the household. However, family size does seem to influence two parent families in their decision to seek dental care for their children, with one parent available to schedule and facilitate the dental appointment, additional children (up to three children) increases the likelihood that any one child has had a visit in the last year. We see a similar result with having an office based usual source of care.

While these results provide some evidence that those families most likely to have time constraints – single parent families – are most affected by having two or three additional children in the household to care for, single parents are also likely to differ from two parent households in financial resources, not just parental time. If so, the results for emergency room and office based usual source of care might be more a function of financial constraints rather than parental time constraints. To better isolate differences in parental time, I again re-ran the interacted models on subsample of families

likely to be homogenous, this time in their financial resources for health care – children with insurance. Any difference in the effect of family size is likely to be due to differences in family structure, not available financial resources. These results are presented in the right three columns of table 2.10 and the general findings described above are robust to this sample restriction: family size has a negative relationship with emergency room visits and having an office-based usual source of care for children of single parent families. In general, these results suggest that parental time resources may be an important factor in the ability of parents to obtain emergency health care and for identifying a usual place of care for one's children.

Conclusions

While there is acknowledgement that large family size can play an important role in the social and health inputs devoted to children in developing counties, it has received relatively little attention in developed countries, as the income effects of additional children are thought to be relatively small. What that assumption ignores is the high cost of health care in the United States and the fact that a significant number of Americans have difficulty obtaining access to health care, because of both cost and time constraints. According to a new Health Affairs study, one in five people had serious problems paying or were unable to pay medical bills in the last year and 28 percent delayed or did not get recommended care because of cost reasons (Schoen et al 2010). While adults with below-average incomes were significantly more likely than adults with above-average incomes to report cost related access problems, these problems are not limited to those at the bottom of the income distribution: among individuals with above average income, one in five reported having at least one cost related access problem. Similarly, barriers to

timely health care can arise due to time constraints. Our health care system is poorly designed to accommodate working families, as almost two-thirds of respondents reported having difficulty obtaining care after hours. Children from large families may be especially vulnerable to cost and time-related access problems, as parents with many children may find their financial resources stretched thin by the high cost of care or find it difficult to schedule appointments around work, child care and other responsibilities.

By investigating the relationship between sibling configuration and children's access to and utilization of pediatric health care, this study attempts to shed new light on how the number of children in the household might impact the health care resources that are conferred to children. Using a large, nationally representative sample of parents and children, I tested multiple hypotheses regarding the relationship between family size and children's health care access and use. I found that the number of children in a household may play an important role in determining the type and amount of care children receive. In particular, I find support for the "resource dilution hypothesis" for emergency room visits, which are driven by unplanned, random events. For having an office based usual source of care, where parents are able to spread implicit costs across multiple children and over time, I conversely find a positive relationship with family size. For dental services, I find evidence that having an additional sibling increases the likelihood that a child will have a dental visit, but this efficiency gain is eroded with the addition of more children to the point where the ability to "bundle" dental care disappears for the largest families.

These results suggest that a child's family context has a meaningful relationship on their access to health care. While the OLS models find rather modest impacts of

family size on non-emergent care, ranging from a 1 to 4 percent increase in well-child and dental care and having an office based usual source of care, results from the IV models suggest a significantly larger effect on utilization. However, although small, the family size effects I find are generally larger than differences in one- versus two-parent families, a specific family context that has been the focus on much research focusing on the differences in economic and social resources available to children from these households.

One reason for the rather small magnitudes is our approach is limited to looking at the probability of having at least one event in the past year. The stresses of a large family might be more relevant to the intensive margin of care. To that end, we find a large, negative relationship between family size and having emergency room visit in the past year, suggesting that having a sibling reduces the likelihood of a visit by 10 to 20 percent. This result suggests that for non-critical, emergent health situations, children from larger families are at risk for delayed or forgone care, likely due to parental time constraints. This is not a trivial finding. Timely access to health care is an important aspect of good health outcomes for children.

Additionally, what these average findings hide is that the problems associated with increased family size might impact families most vulnerable for access related problems. In an effort to better understand *how* family size influences a child's utilization of health care and *who* might be particular vulnerable to the negative effects of family size, I examined whether additional children differentially affected households by their available financial and parental-time resources. I find evidence that both financial and parental-time constraints play an important role in the relationship between the

number of children and child's access to and use of health care and the results presented here suggest that access related problems brought on by family size are particularly acute for children without insurance coverage or children of single parents. Specifically, for children without insurance, family size has a negative relationship with use of well-child, office and dental visits, lending support to the hypothesis that additional children reduce the financial resources for health services available to any one child. Likewise, parental time is an important resource in facilitating some aspects of children's use of health care, such as emergency room visits and having a usual source of care.

This study has several important limitations that need to be acknowledged. First, and most importantly, because I use cross-sectional data measuring family size at a point in time, I have no way of knowing what the completed family size will be. In other words, I cannot identify families that will eventually become large, but are currently small. This is especially true when the parents and children are relatively young. Although my main objective is to measure whether *current* family size influences children's access to care through its impact on the family's available resources, families who choose to (eventually) have large families might also differ in in how they utilize care for their children. I am unable to account for completed family size using the NHIS.

Second, because I did not have access to fertility history data, I constructed birth order using information related to children's birthdates and their relationship to the household reference person. The birth order measure likely suffers from measurement error as I have no way of identifying children who reside out of household. While I limit analyses to younger families in an attempt to mitigate this problem, misclassification of birth order may have occurred for some children.

Third, one of the economic reasons why the number of children in the family may be related to health care use is by its impact on the family's insurance deductable. Within a year, health expenditures at different times are complements, because spending at one time can affect the marginal cost of spending at another time. The spending by one sibling affects the marginal cost of spending by another, because the deductible is more likely to be reached. Because the NHIS only includes utilization information on one child and does not include the timing of visits, I am unable to measure utilization within a family to test whether utilization for one child early in the calendar year increases the likelihood that other children in the family have subsequent visits.

Fourth, one factor that might play an important role in parental behavior is the health status of early born children. Having a child with a serious health condition might influence the family's decision to have another child, while also influencing parent's perception of the health needs of any subsequent children. Additionally, having a child with special health needs is likely to have important consequences on the family's available financial or parental time resources. Because I do not control for the health of older siblings in my models, I cannot parse out these potential effects.

Finally, in all models I treat insurance as endogenous to the demand for health care. Having insurance coverage is not random, but instead related to demand and supply factors, including a child's health status. Therefore, differences in utilization for insured and uninsured individuals will reflect both the causal effect of insurance on utilization and the effect of unmeasured characteristics that are correlated with insurance coverage, such as a child's health status. To minimize this concern, I control for a child's self-reported health status and the presence of a health condition that limits a child's activity,

however, it is unlikely that I have fully controlled for a child's health. Given this, differences across insured status are likely due not only to the effect of insurance but also unmeasured characteristics correlated with insurance. This might be especially important in the models stratified by insurance status, where the general findings that family size is unrelated or positively related to a child's access to care (unlike the negative findings for the uninsured group) might not be due to the protective effect of insurance per se, but on a greater demand for care for the insured group.

The results from this study suggest that health care providers need to be aware of family characteristics as they may impact a child receipt of services. Children are dependent upon their parents to obtain and initiate health care services, as such, health care practitioners may need to consider a child's family context when administering or planning their health care services. Families already faced with financial or time constraints are particularly vulnerable to the demands of large families, and their children may be at risk for reduced access to some types of care. To the extent that parental time constraints limit the ability of parents to arrange for timely and appropriate care for their children, pediatric practitioners may need to expand after hour's availability or implement reminders and follow-ups to increase and promote access to overburdened families.

Additionally, these findings provide further support for the need to continue to develop policies designed to increase access to care for children. Efforts to promote enrollment in health insurance remains a critical factor in increasing children's access to care, as children without insurance are more vulnerable to events that impact a family's financial resources. While I find that financial constraints can play a significant role in

children's utilization of care, I also find suggestive evidence that parental time resources also matter. In particular, I find that family size has significantly stronger negative relationship with emergency room visits for single parents than two-parent families, suggesting that single parents with multiple children may be under greater pressure to distinguish truly emergent from perhaps-emergent situations. While this pressure might lead to a deeper understanding of what needs to be evaluated today versus tomorrow (forgo unnecessary emergency room care), the delay or unmet emergency room care might have health consequences. Policies targeted at accommodating the needs of working families, such as mandating family leave time or increasing after-hours pediatric focused clinics or school-based health clinics, are also likely to increase access to care for time constrained families.

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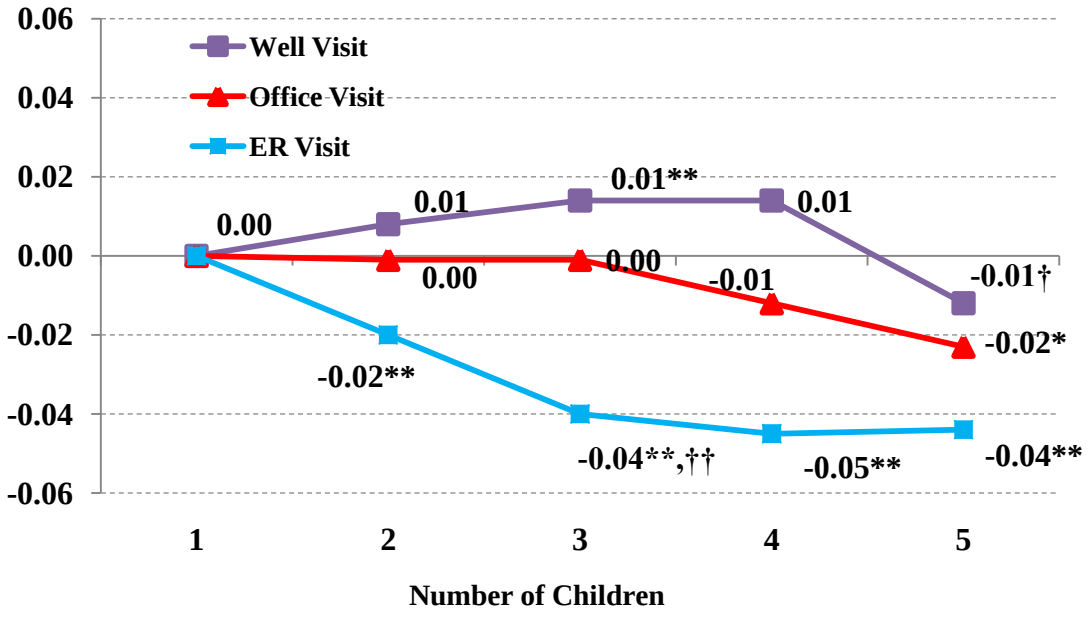
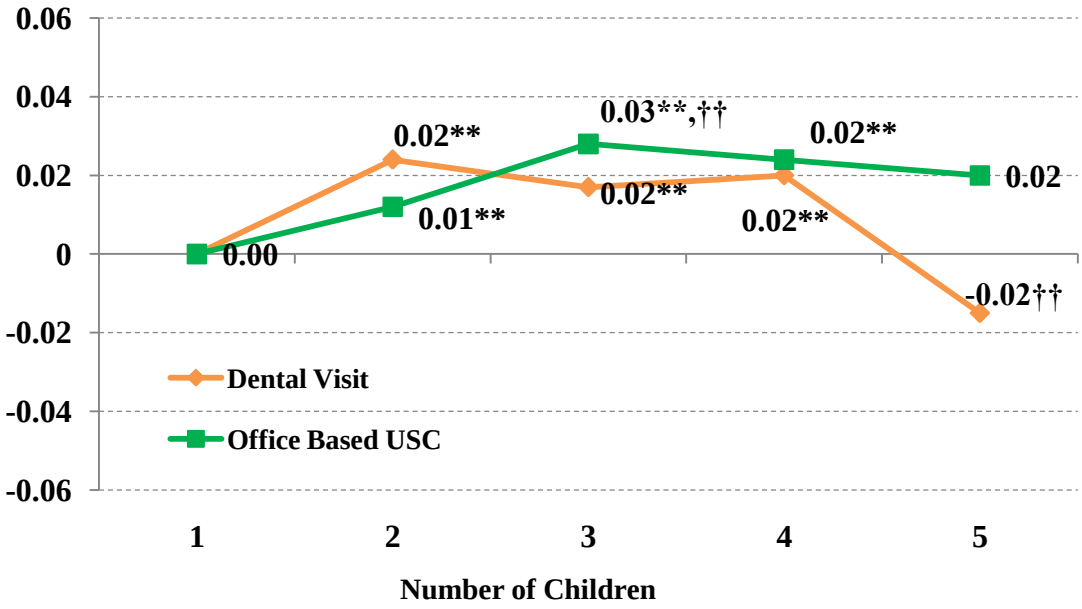
Figure 2.1. Regression Results: Number of Children in the Household and Well Child Visits, Office Visits and ER Visits



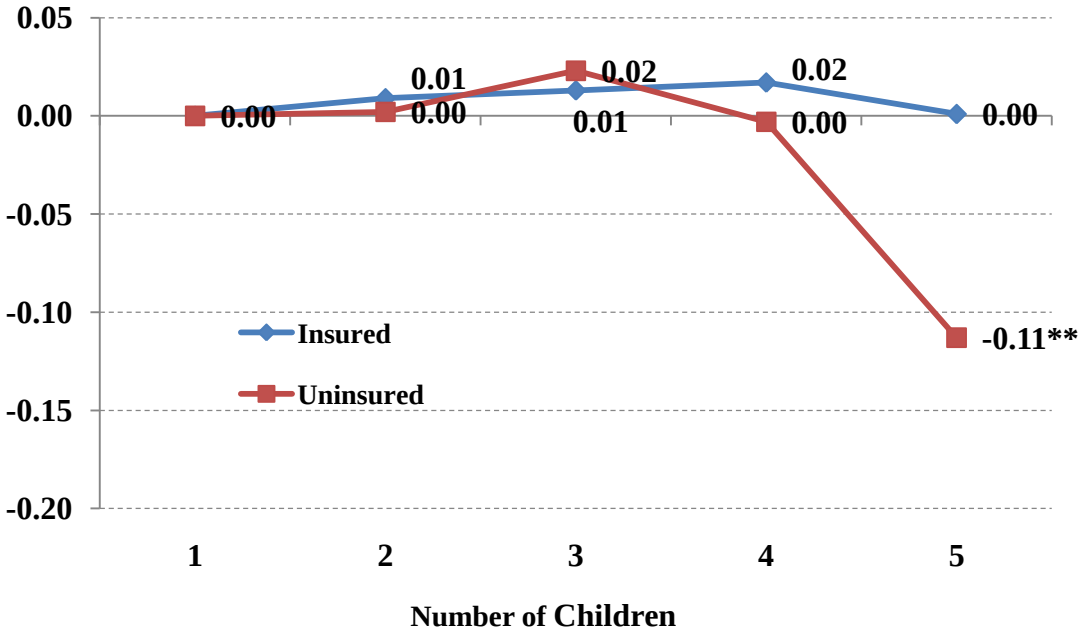
Results reported as average marginal effects (from Table 2.7, equation 2).
* indicates coefficient is statistically different from reference group at p<0.10; ** p<0.05.
† indicates coefficient is statistically different from *previous category* (e.g., number of children 3 vs. 2) at p< 0.10; †† p<0.05
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS.
Source: NHIS 1998-2005.

Figure 2.2. Regression Results: Number of Children in the Household and Dental Visits and Office based Usual Source of Care



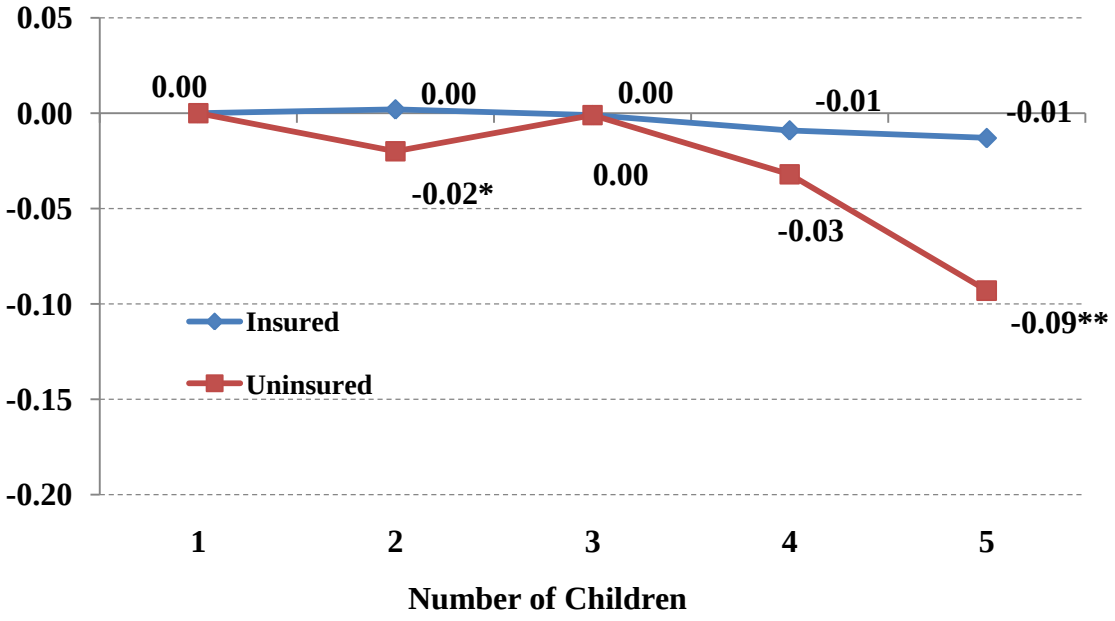
Results reported as average marginal effects (from Table 2.7, equation 2).
* indicates coefficient is statistically different from reference group at p<0.10; ** p<0.05.
† indicates coefficient is statistically different from *previous category* (e.g., number of children 3 vs. 2) at p< 0.10; †† p<0.05
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS.
Source: NHIS 1998-2005.

Figure 2.3. Differential Effect of Family Size on the Probability of a Well-Child Visit, by Child's Health Insurance Status



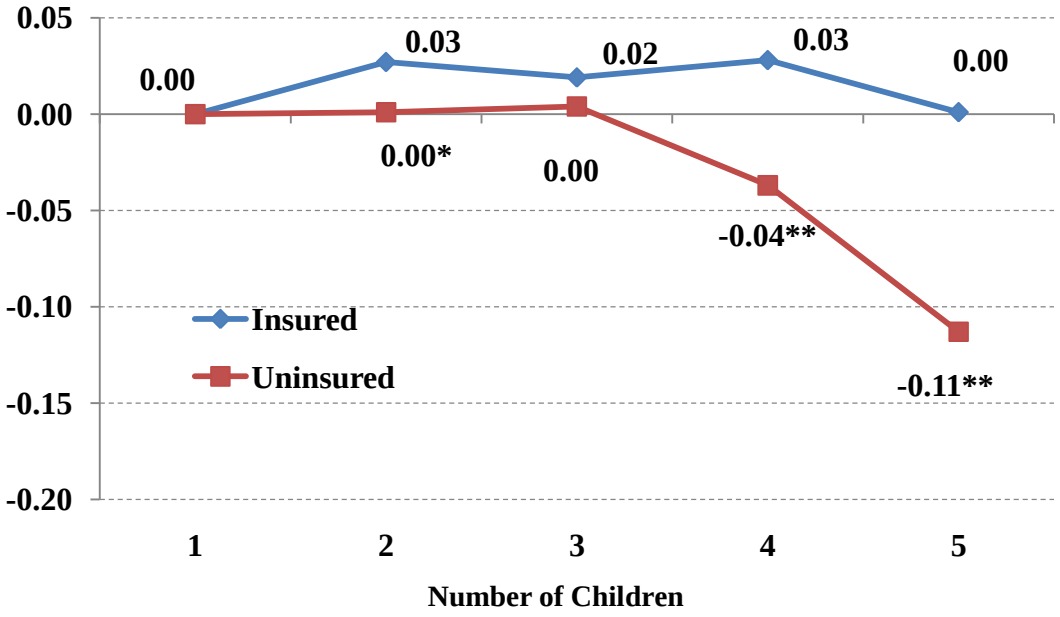
Results reported as average marginal effects.
* indicates uninsured*#children coefficient is statistically different from the insured*#children coefficient at p<0.10; ** p<0.05
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS.
Source: NHIS 1998-2005.

Figure 2.4. Differential Effect of Family Size on the Probability of an Office Visit, by Child's Health Insurance Status



Results reported as average marginal effects.
* indicates uninsured*#children coefficient is statistically different from the insured*#children coefficient at p<0.10; ** p<0.05
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS.
Source: NHIS 1998-2005.

Figure 2.5. Differential Effect of Family Size on the Probability of a Dental Visit, by Child's Health Insurance Status



Results reported as average marginal effects.
* indicates uninsured*#children coefficient is statistically different from the insured*#children coefficient at p<0.10; ** p<0.05
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS.
Source: NHIS 1998-2005.

Table 2.1. Sibling Configuration for Study Sample

	Mean
Number of children in household	
1 child household	0.186
2 child household	0.424
3 child household	0.258
4 child household	0.093
5-6 child household	0.038
Birth order of study child	
first child	0.470
second child	0.349
third child	0.133
fourth child	0.037
fifth or later child	0.011

Descriptive Statistics for 69,039 children < 16 years old (weighted).
Source: NHIS 1998-2005

Table 2.2. Descriptive Statistics

Category	Variable	*Mean*
	Outcome measures	
Access to care	Any well child visit in past year	0.742
	Any doctor visit in the past year	0.889
	Any visit to hospital ER in past year	0.205
	Dental visit in past year (age >= 2)	0.733
	Has office based usual source of care	0.751
	Covariates	
Age of child	Child Age (years)	7.21
	0-2	0.197
	3-6	0.263
	7-10	0.259
	11-15	0.281
Gender	Female	0.491
Race/ethnicity	White	0.641
	Black	0.139
	Hispanic	0.176
	Other race	0.044
Twin	Observation is a twin	0.025
	Observation is not twin, has twin sibling	0.013
Number of parents	Both parents	0.781
	Mother only	0.196
	Father only	0.022
Employment	One parent not working full time	0.576
Parent Age (years)	Age of mother (dad)	34.32
Mom (Dad) Education	< high school degree	0.159
	high school degree	0.276
	Some college	0.317
	>= college grad	0.243
	Education missing	0.006
Child health status	Fair or poor health	0.016
Child health problem	Has activity limiting condition	0.017
Household income	< 125% federal poverty ratio	0.176
	125-249% FPL	0.215
	250-399% FPL	0.197
	400+% FPL	0.220
	Household income missing	0.192
Health insurance	Uninsured	0.096
	Private health insurance	0.659
	Public health insurance	0.244
	Health insurance missing	0.002

Descriptive Statistics for 69,039 children < 16 years old (weighted).
Proportion of children with characteristic unless otherwise noted.
Source: NHIS 1998-2005

Table 2.3. Number of Children in the Household by Child's Birth Order

	Birth Order				
Number of children	first child	second child	third child	fourth child	fifth+ child
1 child household	22,035	1,795	317	28	2
	(91.1)	(7.4)	(1.3)	(0.1)	(0.0)
2 child household	12,859	14,008	880	107	18
	(46.1)	(50.3)	(3.2)	(0.4)	(0.1)
3 child household	3,654	4,293	4,067	225	33
	(29.8)	(35.0)	(33.1)	(1.8)	(0.3)
4 child household	804	871	939	870	56
	(22.7)	(24.6)	(26.5)	(24.6)	(1.6)
5-6 child household	182	248	239	242	267
	(15.5)	(21.1)	(20.3)	(20.5)	(22.7)

Number of observations for each number of children by birth order combination; row percents are reported in parentheses.
Descriptive Statistics for 69,039 children < 16 years old (weighted).
Source: NHIS 1998-2005

Table 2.4. Individual and Family Characteristics by Number of Children in the Household

	Child health: Excellent or Very Good	Age of Child (years)	Mom (Dad) Education: College +	Age of Mom (years)
Number of children in household				
1 child household	0.867	6.07	0.579	32.78
2 child household	0.861	7.19	0.598	34.29
3 child household	0.841	7.64	0.544	34.19
4 child household	0.815	7.81	0.476	34.19
5-6 child household	0.761	7.98	0.364	34.20

Descriptive Statistics for 69,039 children < 16 years old (weighted).
Source: NHIS 1998-2005
All means are adjusted for the complex survey design of the NHIS

Table 2.5. IV Descriptive Statistics

	Households with 2 or 3 children
Number of births	2.28
More than 2 children	0.282
Same sex (first two children are same sex)	0.506
Age of child (years)	7.61
Age of mother (years)	34.33
2-parent households	0.807
Sample size	33,428

Table 2.6. Child's Access to Health Care by Number of Children in the Household

	Well-child visit in past year	Doctor visit in the past year	ER visit in past year	Dental visit in past year (Age 2+)	Has an Office-Based USC
Number of children in household					
1 child household	0.782	0.914	0.245	0.678	0.765
2 child household	0.750	0.899	0.202	0.751	0.773
3 child household	0.725	0.877	0.186	0.745	0.744
4 child household	0.707	0.851	0.186	0.734	0.692
5-6 child household	0.662	0.821	0.197	0.674	0.629
Birth order of study child					
first child	0.746	0.894	0.211	0.742	0.763
second child	0.741	0.890	0.199	0.741	0.757
third child	0.733	0.874	0.197	0.705	0.726
fourth child	0.723	0.863	0.203	0.619	0.667
fifth or later child	0.755	0.872	0.200	0.580	0.658

Descriptive Statistics for 69,039 children < 16 years old (weighted), except for dental visits (n=58,309).
Source: NHIS 1998-2005
All means are adjusted for the complex survey design of the NHIS

Table 2.7. Regression Results: Number of Children in the Household and Children's Access to Health Care

	Well-Child Visit		Doctor Office Visit		ER Visit		Dental Visit		Office based USC	
	(1) w/o birth order	(2) Full Model	(3) w/o birth order	(4) Full Model	(5) w/o birth order	(6) Full Model	(7) w/o birth order	(8) Full Model	(9) w/o birth order	(10) Full Model
Number of Children										
1 child household	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
2 child household	-0.000	0.008	-0.004	-0.000	**-0.019***	**-0.020***	**0.027***	**0.024***	0.004	**0.012***
	(0.004)	(0.005)	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
3 child household	-0.004	**0.014***	**-0.009***	-0.001	**-0.038***	**-0.040***	**0.024***	**0.017***	**0.014***	**0.028***
	(0.006)	(0.006)	(0.004)	(0.005)	(0.005)	(0.007)	(0.006)	(0.006)	(0.005)	(0.006)
4 child household	-0.010	0.014	**-0.023***	-0.012	**-0.042***	**-0.045***	**0.027***	**0.020***	0.003	**0.024***
	(0.009)	(0.010)	(0.007)	(0.008)	(0.008)	(0.010)	(0.008)	(0.009)	(0.008)	(0.009)
5-6 child household	**-0.038***	-0.012	**-0.034***	-0.023	**-0.045***	**-0.044***	-0.009	-0.015	0.001	0.020
	(0.015)	(0.015)	(0.011)	(0.012)	(0.013)	(0.016)	(0.014)	(0.016)	(0.013)	(0.014)
Birth Order										
first child		*Ref*		*Ref*		*Ref*		*Ref*		*Ref*
second child		-0.017*		-0.008*		0.002		0.007		-0.016*
		(0.005)		(0.003)		(0.005)		(0.004)		(0.005)
third child		-0.035*		-0.019*		0.003		0.015		-0.025*
		(0.008)		(0.006)		(0.007)		(0.008)		(0.007)
fourth child		-0.049*		-0.018		0.005		0.007		-0.041*
		(0.014)		(0.010)		(0.014)		(0.014)		(0.014)
fifth or later child		-0.020		-0.008		-0.014		0.001		-0.009
		(0.027)		(0.019)		(0.027)		(0.028)		(0.023)

Coefficients reported as average marginal effects, standard errors are reported in parentheses.
n=69,039 for each regression except dental (limited to 2 or older): n=58,309
* indicates coefficient is statistically different from reference group at p<0.05.
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS. Source: NHIS 1998-2005.

Table 2.8. IV Estimates of Family Size Effects on Children's Access to Health Care

Regression	(1) Probit	(2) 2SRI
Outcome:		
First Stage		
More than two children	-	0.067**
		(0.006)
Second Stage		
Well-child visit	0.012**	0.156**
	(0.006)	(0.045)
Office visit	-0.004	-0.004
	(0.004)	(0.033)
ER visit	-0.022**	-0.083**
	(0.006)	(0.042)
Dental visit	-0.008	-0.155**
	(0.006)	(0.047)
Office based USC	0.015**	0.048
	(0.006)	(0.042)
Sample size	33,428 except for dental (30,124)	

Coefficients reported as average marginal effects, standard errors are reported in parentheses. All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS.
* indicates coefficient is statistically different from reference group at p<0.10; ** p<0.05.
Source: NHIS 1998-2005.

Table 2.9. Differential Effect of Family Size on Children's Demand for Health Care, by Child's Health Insurance Status

	Full sample			2-parent households		
	Insured	**Uninsured**	**Diff**	**Insured**	**Uninsured**	**Diff**
Well Child Visit						
1 child household	*Ref*	*Ref*	-	*Ref*	*Ref*	-
2 child household	0.009*	0.002		0.011*	0.007	
	(0.005)	(0.014)		(0.006)	(0.018)	
3 child household	0.013*	0.023		0.015*	0.025	
	(0.007)	(0.016)		(0.008)	(0.019)	
4 child household	0.017*	-0.003		0.016	-0.002	
	(0.010)	(0.026)		(0.012)	(0.030)	
5+ child household	0.001	-0.113**	††	0.004	-0.125**	††
	(0.016)	(0.049)		(0.019)	(0.050)	
Office Visit						
1 child household	*Ref*	*Ref*	-	*Ref*	*Ref*	-
2 child household	0.002	-0.020*	†	0.003	-0.020	
	(0.004)	(0.012)		(0.004)	(0.014)	
3 child household	-0.001	-0.001		0.001	-0.008	
	(0.005)	(0.014)		(0.006)	(0.016)	
4 child household	-0.009	-0.032		-0.009	-0.027	
	(0.008)	(0.023)		(0.008)	(0.025)	
5+ child household	-0.013	-0.093**	††	-0.006	-0.078*	†
	(0.012)	(0.041)		(0.014)	(0.044)	
ER Visit						
1 child household	*Ref*	*Ref*	-	*Ref*	*Ref*	-
2 child household	-0.020**	-0.018		-0.019**	-0.027**	
	(0.005)	(0.011)		(0.006)	(0.013)	
3 child household	-0.039**	-0.039**		-0.039**	-0.039**	
	(0.007)	(0.013)		(0.008)	(0.016)	
4 child household	-0.041**	-0.058**		-0.020**	-0.058**	
	(0.010)	(0.019)		(0.011)	(0.021)	
5+ child household	-0.041**	-0.033		-0.041**	-0.026	
	(0.017)	(0.034)		(0.019)	(0.041)	

Table 2.9, Continued

	Full sample			2-parent households		
	Insured	**Uninsured**	**Diff**	**Insured**	**Uninsured**	**Diff**
Dental Visit						
1 child household	*Ref*	*Ref*	-	*Ref*	*Ref*	-
2 child household	0.027**	0.001	†	0.034**	0.000	†
	(0.005)	(0.015)		(0.006)	(0.018)	
3 child household	0.019**	0.004		0.027**	0.007	
	(0.007)	(0.018)		(0.008)	(0.021)	
4 child household	0.028**	-0.037	††	0.039**	-0.041	††
	(0.010)	(0.027)		(0.011)	(0.030)	
5+ child household	0.001	-0.113**	††	-0.010	-0.098**	†
	(0.017)	(0.041)		(0.021)	(0.045)	
Office Based USC						
1 child household	*Ref*	*Ref*	-	*Ref*	*Ref*	-
2 child household	0.013**	0.005		0.018**	0.016	
	(0.005)	(0.014)		(0.005)	(0.017)	
3 child household	0.025**	0.044**		0.030**	0.054**	
	(0.007)	(0.018)		(0.007)	(0.020)	
4 child household	0.023**	-0.012		0.031**	0.011	
	(0.009)	(0.026)		(0.010)	(0.029)	
5+ child household	0.014	-0.035		0.034**	-0.042	
	(0.015)	(0.045)		(0.017)	(0.052)	
Sample size (Dental)	61,120 (51,328)	7,919 (6,981)		45,998 (37,789)	5,833 (5,061)	

Coefficients reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from group specific referent category at p<0.10; ** p<0.05.
† indicates uninsured*#children coefficient is statistically different from the insured*#children coefficient at p<0.10; †† p<0.05
All models control for the full set of covariates and are adjusted for the complex survey design.

Table 2.10. Differential Effect of Family Size on Children's Demand for Health Care, by Number of Parents in the Household

	Full Sample			Children with insurance		
	2-parents	**Single parent**	**Diff**	**2-parents**	**Single parent**	**Diff**
Well Child Visit						
1 child household	*Ref*	*Ref*	-	*Ref*	*Ref*	-
2 child household	0.008	0.006		0.007	0.006	
	(0.006)	(0.008)		(0.006)	(0.008)	
3 child household	0.011	0.020*		0.009	0.016	
	(0.007)	(0.011)		(0.008)	(0.011)	
4 child household	0.008	0.034**		0.010	0.031*	
	(0.011)	(0.017)		(0.011)	(0.018)	
5+ child household	-0.018	-0.002		-0.007	-0.002	
	(0.018)	(0.026)		(0.019)	(0.028)	
Office Visit						
1 child household	*Ref*	*Ref*	-	*Ref*	*Ref*	-
2 child household	0.000	-0.001		0.002	-0.001	
	(0.004)	(0.006)		(0.004)	(0.006)	
3 child household	-0.001	-0.002		-0.001	-0.003	
	(0.005)	(0.007)		(0.006)	(0.007)	
4 child household	-0.013**	-0.009		-0.013	-0.005	
	(0.008)	(0.013)		(0.008)	(0.013)	
5+ child household	-0.018	-0.038**		-0.014	-0.032	
	(0.014)	(0.019)		(0.014)	(0.020)	
ER Visit						
1 child household	*Ref*	*Ref*	-	*Ref*	*Ref*	-
2 child household	-0.020**	-0.019**		-0.020**	-0.025**	
	(0.005)	(0.008)		(0.006)	(0.009)	
3 child household	-0.038**	-0.045**		-0.041**	-0.048**	
	(0.007)	(0.012)		(0.007)	(0.013)	
4 child household	-0.033**	-0.085**	††	-0.036**	-0.092**	††
	(0.011)	(0.016)		(0.012)	(0.017)	
5+ child household	-0.045**	-0.042		-0.055**	-0.049*	
	(0.019)	(0.026)		(0.020)	(0.026)	

Table 2.10, Continued

	Full Sample			Children with insurance		
	2-parents	**Single parent**	**Diff**	**2-parents**	**Single parent**	**Diff**
Dental Visit						
1 child household	*Ref*	*Ref*	-	*Ref*	*Ref*	-
2 child household	0.033**	0.007	††	0.034**	0.005	††
	(0.005)	(0.008)		(0.006)	(0.008)	
3 child household	0.029**	-0.009	††	0.027**	-0.013	††
	(0.007)	(0.011)		(0.008)	(0.012)	
4 child household	0.031**	-0.006	†	0.036**	-0.009	††
	(0.010)	(0.018)		(0.011)	(0.020)	
5+ child household	-0.025	0.012		-0.019	0.022	
	(0.019)	(0.024)		(0.021)	(0.025)	
Office Based USC						
1 child household	*Ref*	*Ref*	-	*Ref*	*Ref*	-
2 child household	0.017**	0.005		0.017**	0.006	
	(0.005)	(0.008)		(0.005)	(0.008)	
3 child household	0.032**	0.021**		0.028**	0.017*	
	(0.007)	(0.009)		(0.007)	(0.010)	
4 child household	0.032**	0.002	†	0.034**	0.004	†
	(0.010)	(0.015)		(0.010)	(0.016)	
5+ child household	0.039**	-0.029	††	0.047	-0.037**	††
	(0.016)	(0.024)		(0.017)	(0.025)	
Sample size (Dental)	51,831 (42,850)	17,208 (15,459)		45,921 (37,728)	15,068 (13,493)	

Coefficients reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from the group specific referent category at $p<0.10$; ** $p<0.05$.
† indicates single parent*#children coefficient is statistically different from the 2-parent*#children coefficient at $p<0.10$; †† $p<0.05$
All models control for the full set of covariates and are adjusted for the complex survey design.

Appendix Table 2.A1. Well-Child Visit, Subgroup Results

	1.Main Model (table 2.7)	2.Child Health	3. Child Age	4. Parent Education: Low	5. Parent Education: High	6. Parent Age <35	7. Parent Age =>35
Number of Children							
1 child household	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
2 child household	0.008	0.008	**0.011****	**0.014****	0.005	-0.003	**0.016****
	(0.005)	(0.005)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)
3 child household	**0.014****	**0.017****	**0.020****	**0.021****	0.009	-0.001	**0.024****
	(0.006)	(0.007)	(0.007)	(0.010)	(0.009)	(0.009)	(0.009)
4 child household	0.014	**0.021****	**0.018****	**0.029****	0.006	-0.010	**0.032****
	(0.010)	(0.010)	(0.011)	(0.015)	(0.014)	(0.014)	(0.014)
5-6 child household	-0.012	-0.010	-0.014	-0.000	-0.024	-0.027	-0.009
	(0.015)	(0.017)	(0.017)	(0.021)	(0.024)	(0.022)	(0.022)
Sample size	69,039	57,869	53,533	32,485	36,119	36,008	33,031

Coefficients reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from reference group at p<0.10; ** p<0.05.
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS. Source: NHIS 1998-2005.

Appendix Table 2.A2. Office Visit, Subgroup Results

	1.Main Model (table 2.7)	2.Child Health	3. Child Age	4. Parent Education: Low	5. Parent Education: High	6. Parent Age <35	7. Parent Age =>35
Number of Children							
1 child household	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
2 child household	-0.000	-0.000	-0.002	0.009	-0.007	-0.003	0.000
	(0.004)	(0.004)	(0.004)	(0.006)	(0.004)	(0.005)	(0.005)
3 child household	-0.001	0.000	-0.005	0.008	-0.007	-0.001	-0.003
	(0.005)	(0.005)	(0.005)	(0.008)	(0.006)	(0.006)	(0.007)
4 child household	-0.012	-0.009	**-0.017***	0.000	**-0.020****	-0.015	-0.012
	(0.008)	(0.008)	(0.009)	(0.011)	(0.010)	(0.010)	(0.011)
5-6 child household	**-0.023***	**-0.024***	**-0.027***	0.010	**-0.036***	-0.021	**-0.031***
	(0.012)	(0.014)	(0.014)	(0.016)	(0.020)	(0.016)	(0.019)
Sample size	69,039	57,869	53,533	32,485	36,119	36,008	33,031

Coefficients reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from reference group at p<0.10; ** p<0.05.
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS. Source: NHIS 1998-2005.

Appendix Table 2.A3. Emergency Room Visit, Subgroup Results

	1.Main Model (table 2.7)	2.Child Health	3. Child Age	4. Parent Education: Low	5. Parent Education: High	6. Parent Age <35	7. Parent Age =>35
Number of Children							
1 child household	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
2 child household	**-0.020****	**-0.016****	**-0.025****	**-0.021****	**-0.022****	**-0.029****	**-0.015****
	(0.005)	(0.005)	(0.005)	(0.008)	(0.006)	(0.008)	(0.006)
3 child household	**-0.040****	**-0.038****	**-0.043****	**-0.033****	**-0.046****	**-0.054****	**-0.031****
	(0.007)	(0.007)	(0.007)	(0.010)	(0.009)	(0.011)	(0.008)
4 child household	**-0.045****	**-0.046****	**-0.050****	**-0.036****	**-0.055****	**-0.049****	**-0.043****
	(0.010)	(0.010)	(0.010)	(0.013)	(0.013)	(0.016)	(0.013)
5-6 child household	**-0.044****	**-0.039****	**-0.048****	-0.025	**-0.071****	-0.021	**-0.068****
	(0.016)	(0.018)	(0.016)	(0.022)	(0.023)	(0.025)	(0.017)
Sample size	69,039	57,869	53,533	32,485	36,119	36,008	33,031

Coefficients reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from reference group at p<0.10; ** p<0.05.
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS. Source: NHIS 1998-2005.

Appendix Table 2.A4. Dental Visit, Subgroup Results

	1.Main Model (table 2.7)	2.Child Health	3. Child Age	4. Parent Education: Low	5. Parent Education: High	6. Parent Age <35	7. Parent Age =>35
Number of Children							
1 child household	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
2 child household	**0.024****	**0.022****	**0.019****	**0.021****	**0.021****	**0.032****	**0.016****
	(0.004)	(0.005)	(0.004)	(0.007)	(0.006)	(0.008)	(0.006)
3 child household	**0.017****	**0.019****	**0.011****	0.011	**0.014***	0.009	**0.017****
	(0.006)	(0.007)	(0.006)	(0.010)	(0.008)	(0.012)	(0.007)
4 child household	**0.020****	0.016	0.011	0.004	**0.027****	0.018	0.010
	(0.009)	(0.011)	(0.009)	(0.014)	(0.013)	(0.017)	(0.009)
5-6 child household	-0.015	-0.005	-0.020	-0.025	-0.009	**-0.057****	0.006
	(0.016)	(0.019)	(0.015)	(0.020)	(0.028)	(0.025)	(0.019)
Sample size	58,309	48,469	53,262	27,758	30,236	27,104	31,205

Coefficients reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from reference group at p<0.10; ** p<0.05.
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS. Source: NHIS 1998-2005.

Appendix Table 2.A5. Office Based Usual Source of Care, Subgroup Results

	1.Main Model (table 2.7)	2.Child Health	3. Child Age	4. Parent Education: Low	5. Parent Education: High	6. Parent Age <35	7. Parent Age =>35
Number of Children							
1 child household	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
2 child household	**0.012****	**0.013****	**0.012****	0.003	**0.022****	**0.014****	0.008
	(0.004)	(0.005)	(0.005)	(0.008)	(0.005)	(0.007)	(0.006)
3 child household	**0.028****	**0.028****	**0.024****	**0.020****	**0.038****	**0.039****	**0.014***
	(0.006)	(0.007)	(0.006)	(0.010)	(0.008)	(0.010)	(0.008)
4 child household	**0.024****	**0.026****	**0.021****	0.001	**0.048****	**0.026***	0.016
	(0.009)	(0.010)	(0.009)	(0.014)	(0.012)	(0.014)	(0.012)
5-6 child household	0.020	**0.032****	0.017	-0.001	**0.043****	0.006	0.021
	(0.014)	(0.015)	(0.015)	(0.020)	(0.020)	(0.024)	(0.016)
Sample size	69,039	57,869	53,533	32,485	36,119	36,008	33,031

Coefficients reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from reference group at p<0.10; ** p<0.05.
All models control for the full set of covariates and are adjusted for the complex survey design of the NHIS. Source: NHIS 1998-2005.

CHAPTER 3

Family Size, Birth Order, and Children's Utilization of Health Care: Longitudinal Evidence from the Survey of Income and Program Participation

Introduction

Timely and appropriate health care is considered essential to promoting the health and development of children. Prior research has documented the many benefits of regular utilization of health care by children. Children who receive regular pediatric preventive care are more likely to receive recommended immunizations (Rodewald et al. 1995), have fewer emergency room visits (Piehl et al. 2000; Hakim and Ronsaville 2002) and are less likely to have avoidable hospitalizations (Gadomski et al. 1998; Hakim and Bye 2001). Likewise, access to dental care is essential to a child's oral health. Although tooth decay is largely preventable, it is the most common chronic disease among children. Tooth decay can lead to pain, which can hinder the social and intellectual development of children (United States GAO 2000). Left untreated, poor oral health and dental disease can progress and continue into adulthood, leading to tooth loss and gum disease, as well as contributing to heart disease and other health conditions (United States GAO 2000b; United States DHHS 2000). Age at first preventive dental visit has been found to have a positive relationship with both subsequent preventive dental care and (fewer) dental related costs (Savage et al. 2004). Therefore, understanding the factors that inhibit or facilitate children's use of medical and dental services is important for promoting good health outcomes in children.

The family is acknowledged as an important contextual factor in determining a child's access to medical care (Cunningham, 1990). Family members provide the necessary resources and undertake the decision making that influence how often a child utilizes health care. Therefore, the changes that occur to a family's composition are likely to play an important role in a child's access to and utilization of health services throughout their childhood. This study focuses on a specific family level dynamic that has the potential to influence a child's utilization of health care, the number and ordering of a child's siblings.

The influence of siblings on children's utilization of medical and dental care has not been widely studied. In general, studies on the relationship between number of children in the family or a child's birth order and utilization of health care reach two conclusions. Regarding the number of children in the household, the findings speak to an inverse relationship with a child's utilization of health care: children with more siblings tend to have lower utilization than children with fewer siblings (Bates 1999; Chen and Escarce 2006, 2008; Dombkowski et al. 2004; Luman et al. 2003). These findings lend support to the "resource dilution" hypothesis, children compete for a finite number of household resources, be it financial or parental time, therefore the addition of siblings will reduce any one child's share of such resources. Human capital or health capital investments in children that require household level resources (time spent with child, financial investments in education or health care), might be reduced for a child as the number of siblings increase. The relationship between a child's birth order and their utilization of health care has been studied far less than family size. The available data also tend to point to an inverse relationship in which early born children receive more

medical care than later born children (Bobo et al. 1987; Chen and Escarce 2006; Schaffer and Szilagyi, 1995), although this relationship is not well understood. One hypothesis is that a child's birth order serves as a marker for parental experience with the illness patterns of children and the pediatric care system. Parents of early born children might be more likely to take their child to see a physician at the first sign something is wrong, while parents of later born children might be better able to identify an illness that requires medical attention, and therefore might require less physician visits, in particular for very young children.

However, much of the research conducted on the influence of siblings on health care utilization has been cross-sectional. While results from these studies might document a correlation between family size or birth order and utilization of health care, it is likely that families that differ in size are also importantly different in other ways that may impact their children's utilization of health care. While researchers can and do control for observable individual and family level factors in an effort to account for these differences, bias resulting from unobserved between family heterogeneity remains a concern.

In an effort to shed light on how family size impacts children's utilization of health care, in earlier work (Chapter 2), I exploit parental preferences for a mixed sibling-sex composition to instrument for family size and estimate the impact of having one versus two other siblings in the household on children's extensive use of important health service outcomes. These results tend to support my correlational findings obtained by pooling multiple years of the National Health Interview Survey (NHIS) together.

While results from the study presented in Chapter 2 underscore the influence that siblings can have on a child's health care, it leaves some questions unanswered. Results from my preferred instrumental variables approach, while offering the first (to my knowledge) truly causal estimates of family size on children's utilization of health care, may not be generalizable outside of the change from a family size of two to three children. Given that the resource dilution hypothesis predicts a non-linear relationship between family size and a family's resources (children with no siblings lose a larger share of parental resources than children with many siblings with the addition of another sibling), the IV results might understate the effect of going from an only child to a two-child household. Additionally, while the NHIS allows us to estimate the relationship between family size and measures of children's access and use of services on the *extensive margin*, I am unable to estimate the relationship on the *frequency* of visits. Most importantly, with the sample available in the NHIS, I am not able to obtain reliable estimates describing differences in health care utilization across a child's order of birth. Given the nature of the relationship between a child's birth order and family size[19], specifications that do not estimate birth order conditional on family size are unable to separate out the effects of size from the birth order estimate (Kessler, 1991). Even with 8 years of NHIS data pooled together, the resulting sample of children is still not large enough to allow me to systematically examine birth order across different family sizes.

Therefore, in an effort to broaden our understanding of the relationship between family size, birth order and a child's utilization of health care, I address these additional questions using data from the 1996, 2000 and 2004 panels of the Survey of Income and Program Participation (SIPP). Because the SIPP is a longitudinal study, collected in a

[19] For example, third born children are only possible in a family size of 3 or more.

series of panels lasting between 2 and 4 years each, it offers methodological advantages over cross-sectional studies in examining dynamic changes in the family, such as changes in sibling configuration. Specifically, I exploit the panel nature of the data and utilize information on multiple children per family to examine how an increase in family size and the order of a child's birth impacts a child's health care use by running two experiments. To estimate the impact of the addition of a newborn on an (current) only child, I estimate the change in their utilization of health care after the addition of a newborn sibling to the household. I use the experience of a control group of similar first born, only children to net out the effects of aging or other competing trends and allow me to isolate the impact of gaining a sibling on a child's health care utilization.

To test for differences in health care across a child's birth order, I first take advantage of the large sample from pooling three panels of the SIPP to look at whether I find birth order differences conditional on the number of children in the household. Then, in an attempt to account for unobserved differences across families and the confounding influence of a child's age on utilization, I look across panels and across families to identify pairs of siblings for which I have their utilization information at the same age. With this sample of children I can offer a descriptive look at how children within the same family differ in their utilization due to the order of their birth, holding both age of the child and family size constant. In doing so, I try to answer the question of whether parents use more (or less) health care for their first child than they do their second child, at a given age.

Methods

Data from the Survey of Income and Program Participation

To analyze the relationship between sibling configuration (number of children and a child's birth order) and children's access to and utilization of health care, I use data from the 1996, 2000 and 2004 panels of the Survey of Income and Program Participation (SIPP). The SIPP is a series of longitudinal data sets lasting between two and four years each, collected for a national sample of the US population by the Census Bureau. At every interview, respondents are asked a series of "core" questions that elicit information about the previous four months, including questions on family status – such as the current household roster and each individual's relationship to the head of household – and insurance coverage. In addition to the "core" questions asked at every interview, additional topical questions are asked on a rotating basis. The topical module I use in this study is the Medical Expenses and Utilization of Health Care topical module[20] that includes several questions related to health care use. Parents are asked about the number of medical and dental visits[21] each of their children had during the prior year. I use these questions to measure children's access to and utilization of medical and dental services. I create two dichotomous variables indicating whether (1) the child had at least one medical visit, and (2) the child had at least one dental visit in the prior 12 months, which I operationalize as measures of access to medical and dental care. To assess the relationship between sibling configuration (number of children and a child's birth order) and the amount of care received, I also examine variables describing (3) the total number of medical visits and (4) the total number of dental visits a child had during the prior 12 months.

[20] Asked in the third, sixth, ninth, and twelfth waves of the 1996 panel, the third, sixth and ninth of the 2001 panel and the third and sixth of the 2004 panel

[21] For dental outcomes, I use only children aged 3 and older as the dental care questions are only asked for these ages.

My measure of family size is the number of children in the household under the age of 16 at the time of the topical module on health care utilization who are indicated to be a child of the reference person. To construct the measure of birth order, for each household in a SIPP panel I ranked by age and birth date those individuals who reported they were the biological child of the household reference person. Table 3.1 describes the unadjusted relationship between these measures and the health care outcomes of interest. The data suggest that utilization of these services decrease in both family size and birth order, with the probability of dental visits and family size perhaps being the only exception.

All data analyses were performed with STATA 10. For analyses that utilize multivariate regression, I use probit regression to model the dichotomous outcomes (probability of having a medical or dental visit), and negative binomial regressions to model the number of such visits over the prior 12 months. Control variables for the multivariate regressions come from both the core survey and the topical module. Variables from the core survey include child age, gender, race/ethnicity, family structure (two-parent, mother-only, father-only family), maternal education[22], household income[23], parental employment and the child's health insurance[24] while control variables from topical modules include the child's reported health status, measured on a standard five point scale, and child and maternal disability status. All regression results are reported as

[22] In cases when the mother is not in household or information on the mother is missing, I use father's education.
[23] I use the average monthly income over a year as our measure of income.
[24] I form three health insurance coverage categories measured at the time of the interview when the topical module on medical care utilization was administered: uninsured, private insurance, and public insurance. The private category includes employer-sponsored group insurance, which accounts for the vast majority of private coverage, and non-group insurance. CHAMPUS, the insurance program for dependents of military personnel, is coded as private coverage because it is employment related. The public category consists mainly of Medicaid and SCHIP plus a very small number of children who qualify for Medicare because of a disability or are covered by the Indian Health Service.

average marginal effects in order to facilitate interpretation**.** Standard errors are adjusted for clustering of observations within children or within families.

Sample

The focus of this study is on children who are dependent on their parents for health care utilization, the SIPP sample is therefore limited to those individuals younger than 16. I further restrict the sample to "traditional" families, specifically to include children living in single-family households and those households in which the only individuals under the age of 16 indicated they were children of the household reference person[25]. In addition, I exclude families which had twins, as I expect having twins will impact parents' available resources in a unique way and, due to their small numbers, children from extremely large households (households with 7 or more children). My available sample is thus based on an extract that consists of 148,853 observations on 64,275 children (66,842 observations on 23,207 children from the 1996 panel, 42,432 observations on 17,860 children from the 2001 panel and 38,579 observations on 23,208 children from the 2004 panel).

Table 3.2 provides summary statistics for the SIPP data. The observations are child-years, with each child contributing between one and four observations to the data. The data show that almost 7 in 10 children had at least one medical visit in a 12-month period, with the overall mean number of visits at just fewer than three per year. Overall, roughly two-thirds of children ages 3 to 15 had at least one dental visit in the past year with children averaging approximately one and a half visits per year.

Results

[25] For example, I exclude households that contain children who report they are young siblings or grandchildren of the household reference person.

Number of Children

I start by treating the SIPP as a cross-sectional dataset to estimate the "between-family" differences in children's utilization of health care by the number of children in the household. I do so using an approach similar to that outlined in Chapter 2 above (equation 2): regressing the health care outcomes on dummy variables for the number of children in the household, controlling for a child's birth order and a set of individual and family characteristics. The results are given in Table 3.3.

The cross-sectional models imply that the number of children is strongly correlated with children's utilization of health care, although the sign of the relationship is different for medical and dental visits. For medical visits, I find evidence of a negative, monotonic relationship with family size: an increase in the number of children in the household reduces both the likelihood of having a medical visit and the number of medical visits in the past 12 months. For example, compared to only children, children with one sibling were 3 percentage points less likely to have a medical visit and averaged approximately 0.27 less medical visits a year. Except for the difference in the probability of a visit across children with one compared to two siblings, there is evidence of a strong dose response of medical visits to family size: the more children, the less medical care a child will have over the course of a year.

For dental care, however, I find evidence of a more complex relationship. Looking first at the probability of having at least one dental visit in the past year, for all but the largest families, family size has a positive effect. The results suggest an inverse "u" relationship with the number of children. Across smaller families (one to three children), the additional siblings increase the likelihood of a visit, but, for larger families,

the additional siblings decreases and eventually reduces any positive effect of additional siblings. While these results suggest siblings might increase *access* to dental care, we find no evidence that an increase in the number of children in the household increases the *number* of dental visits over the course of the year, and evidence that a child who has many (3 or more) siblings will have fewer dental visits than those with fewer than 3 siblings.

Number of Children: Addition of a Newborn to the Family

Taken together, these results would suggest that family size is significantly related to utilization of health care. However, a concern with estimates from this cross-sectional approach is that unobserved factors related to health care utilization may differ systematically across family size, potentially biasing the estimates of number of children on health care utilization. The worry is that parents who decide to have many children are likely to be different in ways that may also impact their decisions on their children's health care utilization. While I control for observed characteristics in a multiple regression framework, bias from unobserved heterogeneity remains a concern.

Therefore, a more direct test of an increase in family size that might impact a child's utilization would be to focus on families that have a newborn. I can empirically test this by exploiting the longitudinal nature of the SIPP and estimating children's change in utilization after the birth of a sibling. To do so, I focus on first born children in families that had a birth subsequent to the first born child's initial observation in the data. I include in the sample the observation in which the focus child gained a sibling (post-sibling observation) and their observation from the prior wave (pre-sibling observation).

My objective is to examine how the first born child's health care utilization changed after a new child entered the household (pre-post sibling change in utilization).

A problem with this pre-post approach is that children's health care utilization is also related to a child's age; on average, younger children have more medical visits than older children, and older children may be expected to have more dental visits than younger children with the introduction of their permanent teeth. Therefore, is it useful to compare the changes over time of the "treatment group", first born children who gained a sibling in the second year, to a "control group", first born children *who did not* gain a sibling over the two year period, to account for the potentially confounding influence of age on health care utilization. To form my control group, I identify first born children who do not gain a sibling (newborn) over the SIPP panel and include their first observation (pre-sibling) and second observation ("post-sibling"). For both groups, I define a variable POST that indicates whether the child was observed in the "pre-sibling" or "post-sibling" period. I take the pooled sample of treatment and control children and run multivariate probit and negative binomial models as follows

$$y_i = \beta_0 + \beta_1 \, TREAT_i + \beta_2 \, POST_i + \beta_3 \, TREAT_i * POST_i + \omega \, X_i + \varepsilon_i \qquad (1)$$

With this approach, β_3 will be the coefficient of interest as it represents the change in utilization that occurred among children who gained a newborn sibling, net of the changes that would have occurred due to maturation (aging) of the child. The vector X representing the list of covariates discussed above. I compute the regression-adjusted "difference-in-differences" (DD) estimates for all ages (0-14) and stratified by the child's age, distinguishing between children aged 0 to 4, 5 to 9 and 10-14.

Table 3.4 presents data on the four health care utilization outcomes separately for the treatment and control groups, by age. For both groups, the table shows utilization in each time period and the change in utilization over the time period (T to T+1). In the last column, I report the unadjusted difference-in-difference estimator. The first two panels in the table show the change in the probability of a medical visit (panel A) and the number of medical visits (panel B). For children in the 0-4 and 5-9 age groups, although both the treatment and control group saw a decline in utilization over the two-year period, the DD estimate suggests that children who gained a sibling saw a greater decline than those who stayed an only child. Among teenagers, the trend over the two year period was an increase in utilization, with the children in the treatment group experiencing a larger gain in both the probability and number of medical visits than did children in the control group, although these estimates are rather small in magnitude. However, this result should be viewed with caution as most teens who gain a sibling are in larger families. Families that wait a long time between their first and second children are likely be different than those with shorter spacing in ways that may also impact their children's health care utilization.

For dental visits, the data tell a different story. Looking first at the probability of having a dental visit (panel C), for both treatment and control groups, the trend over the two year period was an increase in the likelihood of a visit. The data for the youngest children (age 0-4), suggest a possible positive effect of the addition of a sibling on access to dental care: children who gained a sibling had an 18 percentage point increase, compared to a 15 point increase for children who remained an only child. However, for children in both the 5-9 and 10-14 age groups, the increase in the control group was

larger than children who gained a sibling in period 2. For the number of dental visits, for all three age groups the addition of a sibling is associated with a smaller increase in utilization of dental care than seen among children who remained only children.

The data from Table 3.4 imply that first born children who gained a sibling saw a differential change in both medical and dental utilization than similarly aged children who remained an only child. The data also show that the direction of the "sibling effect" might depend on the child's age when they gained a sibling.

Table 3.5 presents the multivariate results from the regression adjusted DD models. Using the full sample of children (column 1), the results for medical visits indicate that the addition of a sibling decreased both the probability of having a visit (4.7 percentage points, $p<0.05$) and the number of visits (0.90 visits, $p<0.05$). These results are similar to what we noted in Table 3.4, where, for two of the three age groups we noted a greater decline for medical visits in our sample of children who gained a sibling, compared to the control group of children who did not. With respect to dental visits, the coefficient on the interaction term implies the addition of newborn resulted in a 4.8 percentage point increase in the probability of having a dental visits ($p<0.05$). For the number of dental visits, there is no evidence of a meaningful difference in the change in utilization between the two groups of children.

Turning now to the results from the age stratified models presented on the right side columns in Table 3.5, in general the estimates are imprecisely measured; therefore I cannot reject the null that there is no difference in the change over time for the treatment and control groups. However, the point estimates from these models, coupled with the full model results, would suggest that the negative impact of the addition of a sibling on a

child's utilization of medical services might be restricted to the youngest children. The one estimate that meets statistical significance in the age stratified models, number of medical visits in the 0-4 age group, suggests that on average, the number of visits declined by approximately 0.7 visits per year after a newborn child entered the household (pre-post sibling change). For dental visits, the full model results suggested that the addition of a newborn increased the probability of a visit by just less than 5 percentage points. The point estimates from the age stratified models suggest this impact be limited to young children (aged 0-4).

Birth Order

There are also reasons to expect that a child's birth order might be associated with their utilization of health care. As parents accumulate experience with early born children's health needs and pediatric care norms and guidelines, they might demand more, or less, care over their subsequent children. As we saw in Table 3.1, the unadjusted relationship between birth order and the utilization outcomes points to a pattern of lower access and use of health care for later-born children. However, the univariate relationship does not account for individual or family characteristics that may affect children's health care use, nor does it address the potential confounding effects of family size and birth order. Therefore, to investigate the relationship between a child's order of birth and their utilization of health care, I estimate multivariate regression models, once again starting with the pooled SIPP data and modeling the relationship as described in chapter 2 (equation 2). To determine whether the relationship varies systematically according to family size, I run separate regressions by the number of

children in the family.[26] Estimates from these models are reported in Table 3.6. Starting with medical visits, the estimated effects from these models suggest that, at least for children in two- and three-child households, a child's birth order is negatively related with the probability of a medical visit (panel 1) and the mean number of medical visits (panel 2). The results also suggest a dose response relationship, with the last born in two-, three- and four-child households having the least utilization.

Turning to dental care in the bottom two panels, unlike the univariate results, which suggested a negative relationship between the dental outcomes and a child's birth order, here we find some evidence that later born children have greater utilization of dental care than early born children. Looking at the probability of dental visits (panel 3), among two- and three-child households, the last child was 1.5 to 2.0 percentage points more likely to have a visit in the past year than a first born child ($p<0.10$). There is no evidence that this relationship extends to four-child households, as the birth order coefficients are actually negative, though they are far from being statistically significant. For the number of dental visits, for two-child families, second born children have slightly greater utilization than first born, again contrary to the univariate findings, although this pattern does not hold for children in three- and four- child households.

Birth Order: Sample of First and Second Born Children

The above results suggest that a child's birth order may have an important relationship with access to and utilization of medical and dental care. However, like the cross-sectional results for number of children, it is possible that the results are biased by omitted family-level characteristics. One possible way to control for potential unobserved family-level heterogeneity would be to estimate the models conditioned on

[26] I exclude families with five or more children from these analyses due to small cell sizes.

family fixed effects. However, the short panel of the SIPP coupled with the strong association between a child's current (sample) age, birth order, and utilization of care makes this approach problematic. While we can adjust for a child's age in the multivariate regression models, our results might be subject to omitted variable bias if, for example, children of different ages differ in both their birth order and their utilization of services. In fact, first born children are more likely to be older than later born children and younger children utilize more medical care, and less dental care, than older children.

The ideal dataset to assess within-family birth order effects on health care utilization would have a long panel, so as to measure each child's utilization at a certain age. For example, we would like to know the utilization patterns of the first born, second born and third born child of a three child family at age three, six, nine, etc. While the SIPP might not have same age observations for all children, there are a sizable number of two-child families where we do have observations from both children *at the same age.* Therefore, as a way to reduce potential heterogeneity that exists across families and across the age of the child I limit the sample to families that have exactly two children in the household throughout the SIPP panel (so as to hold family size constant) and both children were interviewed at least once at the same age. So, for example, using the 1996 SIPP with its four panels, for a two child family where the first child was aged 10 and the second child was aged 7 in the first wave, I include in my sample the first child's observation from wave 1 (at age of 10) and the second child's observation from wave 4

(when they were also 10 years old). Doing so gives me a sample of 8,160 observations on 5,770 children aged 1-14 from 2,938 families[27].

I start by looking at the distribution of the four outcome measures across the child's age at interview, for first and second born children separately. Results for medical visits are shown graphically in figure 3.1 with dental visits displayed in figure 3.2. Figure 3.1 shows how the relationship between medical visits and a child's birth order (first vs. second) differs over a child's age. The outcome in Panel A is the probability of having a medical visit in the past year. In Panel B it is the number of visits over the 12 month period. Both panels in figure 3.1 tell a similar story: among children aged 1 to 10, compared to second born children, first borns appear to have greater utilization of medical services. For teenagers (11-14), this pattern appears to reverse, as second borns are more likely to have a medical visit and, for two of the four age groups, have more medical visits over the past 12 months than first born children. Some of these differences are rather large. Looking at panel B, the differences in the mean number of visits for children aged 4 to 6 range from one-half to slightly over one visit per year.

Figure 3.2 plots the relationship between birth order and children's utilization of dental care, again conditional on a child's age at interview. As we saw with the cross-sectional models above, we find dental utilization generally increasing in a child's birth order. The data show that, among young children, second born children have greater access to dental care (panel A) and greater utilization of dental care, then first born children. Although, like medical visits, this is not necessarily true of teenagers, especially for the number of dental visits, as first borns have greater utilization than

[27] I am unable to assess children in their first year of birth while also holding the number of children constant at two. I also exclude children aged 15 from the sample at this time to be consistent with the family size analyses that cannot assess 15 year olds over two periods.

second born children at ages 12-14. These unadjusted differences appear to be meaningful, especially among youngest children. Looking at the likelihood of a dental visit (panel A), for children aged 3 to 6 years old, second borns were 5 to 11 percentage points more likely to have a visit than first borns.

To provide a more precise estimate of how a child's birth order might affect their utilization of medical and dental care, I turn to estimating a set of multivariate regression models on my sample of first- and second-born children. Here, my estimates will be the average effect of being a second child (vs. first child) across all age groups. To better hold a child's age constant and investigate the differential relationship between birth order and utilization across age, in addition to estimating models on the full sample, I stratify the sample by age, distinguishing between children aged 1 to 4, between 5 and 9, and 10 to 14.

Results from these models are reported in Table 3.7. Looking first at the full sample results (column 1), other than dental visits, we find little evidence to support the descriptive findings from the above figures. For the two medical visit outcomes, the parameter estimates are essentially zero and nowhere near statistically significant. For the dental outcomes, while there is evidence of positive relationship between birth order and the likelihood of having a dental visit – second borns are 3.4 percentage points more likely ($p<0.05$) – the coefficient on the number of dental visits is small and statistically insignificant. What these average effects hide is that the association between the health care outcomes and birth order varies by the child's age. Turing to the age stratified results on the right side of the table, for medical visits, while not all the results are statistically significant, young (age 1-9), second born children have less utilization than

similarly aged first borns. The point estimates indicate a meaningful difference in visits by a child's birth order across a child's early years. Looking at the number of medical visits (row 2), the coefficients imply that while second born children on average would have approximately 4 fewer medical visits than would first borns over the ages of 1-9. However, among older children (10-14) the results suggest this difference in use of medical care by birth order disappears, and if anything the estimates suggest that second borns have *more* utilization than do first borns in this age group, although the results are far from statistically significant.

The age stratified results for our dental outcomes imply that the association between utilization and birth order is also conditional on child age. In the two younger samples (1-4, 5-9), we see that second borns have greater access to dental care and more overall dental visits per year than do first borns. The coefficients imply rather large differences across birth order, in particular for the likelihood of having a dental visit in the past year. Second born children are 4.8 to 8.0 percentage points more likely to have had a dental visit than are first born children of a similar age group. Coefficients on birth order for the number of dental visits are also positive and significant for children in the middle age group (5-9), although the magnitude of the effect is somewhat small (0.19 more visits per year). For the oldest sample, the coefficient for the second born variable are now negative and not statically significant, suggesting the positive relationship with utilization of dental care does not extend to older children.

Discussion

Despite the interest in understanding factors that are related to children's use of health care and the fact that the family plays such a key role in a child's health care

decision making, the influence of siblings on this relationship has not been well understood. This is likely due to the many pathways in which siblings might indirectly affect a child's use of health care. Siblings have the potential to compete for available household financial or parental-time resources and therefore might negatively influence the amount of health care received. On the other hand, for care that is not time sensitive, having a sibling to "share in the cost" of these services might have a positive impact on the amount of these services a child receives. Additionally, older siblings might affect the care a child receives through their influence on parent's decision making and propensity to use health care.

In this paper, I document different ways in which siblings indirectly influence a child's use of health care. My analysis is based on panel data from the 1996, 2001 and 2004 panels of the Survey of Income and Program Participation, data which are well suited to separate out these many potential effects. By exploiting the longitudinal design of the data, I can estimate the changes in a child's utilization of health care due to changes in family size. Having observations on multiple children in a family over a multi-year period also allows me to examine how children within the same family differ in their age-specific utilization by the order of their birth. These analyses produce several interesting findings. My results suggest children's use of medical services is generally decreasing in family size. While this general finding is not new, I document how the relationship is context dependent, as both the type of care and age of the child matter importantly to the magnitude and direction. I find that younger children are more likely to be affected by an increase in family size brought on by a newborn to the household. Although not all the results reach statistical significance, the age stratified models suggest

that for children under the age of 10, the addition of a newborn to the household reduces both the likelihood and the amount of medical care a child receives. These results suggest that some of the health care needs of children in households with newborns might be unmet due to the stressors and time constraints faced by parents during this period. Pediatric providers might do well to target families during this demanding time with reminders of scheduled visits and educational efforts on the importance of well-child visits and immunizations for the first born child.

For dental care, however, I find the opposite effect. The overall impact of a newborn sibling on the probability of having a dental visit is positive, suggesting a "bundling" of visits across multiple children, which is somewhat surprising given that we would not necessarily expect parents to take a newborn to the dentist. However, there are reasons we might expect such a result. The age stratified point estimates suggest the positive impact is limited to younger children, although these results are imprecise. Utilization of dental care is lowest among younger children, as parents might perceive there being little need for services prior to the introduction of his or her permanent teeth. With the addition of another child, however, parents who now have two young children might have more of an incentive to identify a dental home for their children, resulting in an earlier first dental visit for the older child.

The results also indicate a significant and meaningful relationship between a child's utilization of health care and birth order. To reduce any bias due to heterogeneity across families, I utilize information on multiple children in the family and the 2 to 4 years of panel data to examine how children within the same family differ in their age-specific utilization by the order of their birth. For young children, the results show that,

compared to first born children, second born children receive less medical care and more dental care. These results lend support to the notion that parental decision making for their children's health care is a learned skill and parents apply the lessons from their experiences with their first born children to subsequent children. Parents likely learn that pediatric care has its limits and are therefore less likely to take a child to the doctor for perceived illnesses or problems than a parent would with their first born child. However, a reduction in parental worry about the health of their children might also put subsequent children at risk for forgone preventive care or delayed immunizations. Pediatric providers might therefore be advised to reiterate the importance of timely and appropriate pediatric care to parents over each of their children.

Regarding dental care, second born children are likely to benefit from the initial search cost of establishing a dental home for their children. Additionally, parents of first born children might feel it's unnecessary to take a child to the dentist until their permanent teeth arrive. At their first contact with their child's dental provider, they might learn that pediatric guidelines recommend a child see a dentist as early as their first birthday, and therefore be more likely to have their subsequent children's first visit at an earlier age. These results suggest better outreach to parents of first borns may be necessary to ensure that parents are aware of the benefits of early dental services. Efforts to link pediatricians and other children's providers, who often serve as the first health care contacts for parents of first born children, with dental providers might help promote early contact with oral health professionals.

While these findings help further our understanding of how changes to a family's sibling composition impact the medical care children receive, they should be interpreted

with the following additional limitations in mind. First, I focused my attention on smaller families with young children; therefore, these results might not necessarily be generalizable to later born children from large families. Second, because the SIPP only offers a 2 to 4 year panel, in estimating the effect of a newborn, I am limited to examining a one-year change. It is unclear what happens to a child's health care after this initial period. Finally, the short panel also limits the generalizability of the birth order findings, as my sample only includes children who are at most three years apart in age. It could be that the birth order effects I find disappear with longer spacing between children, as the lessons and contacts learned by tending to the first born are forgotten over time.

References for Chapter 3

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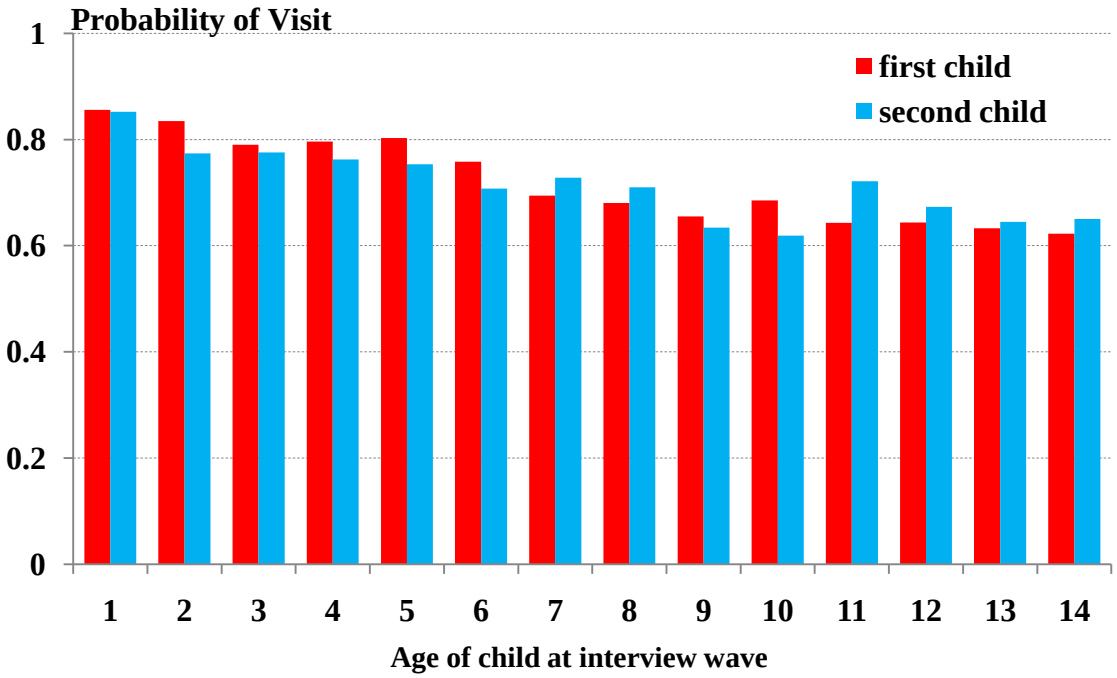
Figure 3.1. Birth Order and Medical Visits, by Age



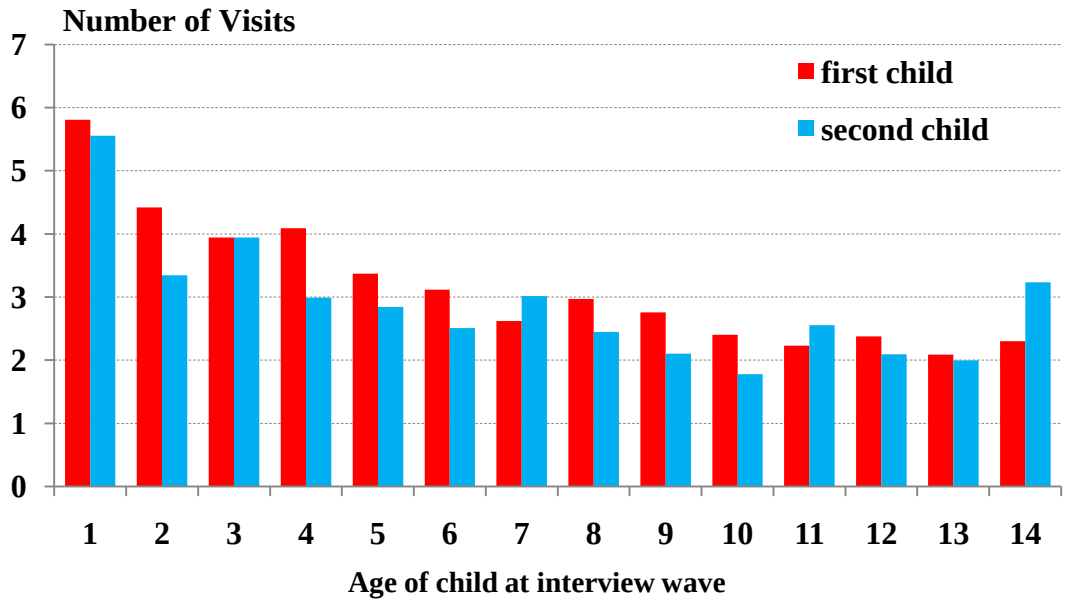
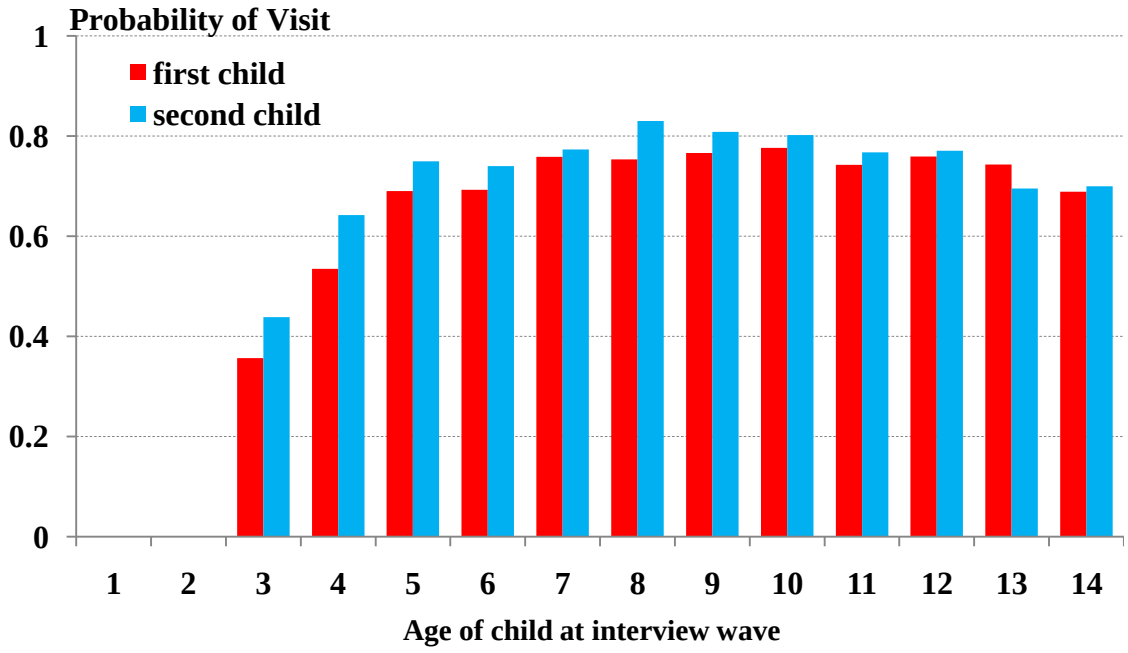


Figure 3.2. Birth Order and Dental Visits, by Age



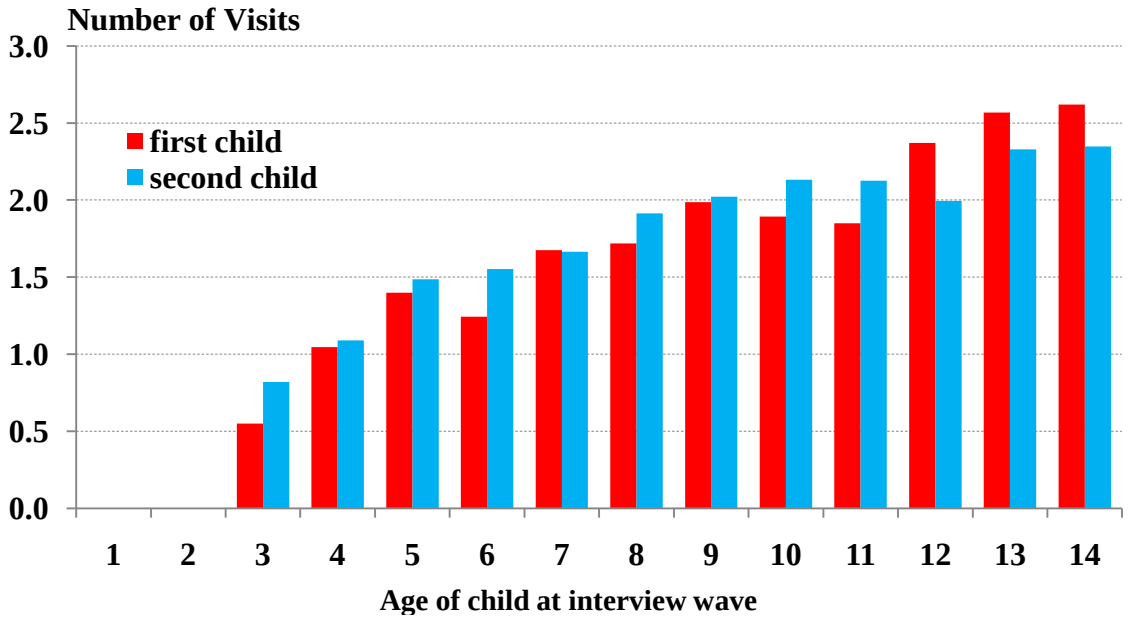


Table 3.1. Children's Medical and Dental Care by Number of Children and Birth Order

	Medical Visits		Dental Visits	
	Probability of visit (%)	**Total visits (per child)**	**Probability of visit (%)**	**Total visits (per child)**
Number of Children				
1 child household	75.1	3.55	67.8	1.89
2 child household	70.0	2.89	69.9	1.75
3 child household	66.5	2.57	67.2	1.59
4 child household	60.2	2.12	62.4	1.39
5-6 child household	52.8	1.65	54.2	1.14
Birth Order				
first child	71.1	3.00	68.8	1.82
second child	68.2	2.78	68.3	1.64
third child	65.0	2.67	63.3	1.34
fourth child	61.1	2.29	57.6	1.10
fifth or later child	56.8	2.12	48.7	0.90
Sample size	148,853	148,853	127,253	127,253

Descriptive Statistics for children under the age of 16 years old, except for dental visits (aged 3-15).
Source: 1996, 2001, 2004 SIPP

Table 3.2. Descriptive Statistics

	Mean	(SD)
Medical care outcomes:		
At least one physician visit in past year	0.689	
Number of physician visits in past year	2.86	(7.14)
Dental care outcomes (age 3-15):		
At least one dental visit in past year	0.632	
Number of dental visits in past year	1.58	(2.50)
Child characteristics:		
Age (years)	8.27	(4.66)
Female	0.488	
Race: White	0.798	
Race: Black	0.139	
Race: Other	0.063	
Hispanic	0.147	
Health status excellent	0.571	
Health status very good	0.283	
Health status good	0.126	
Health status fair	0.018	
Health status poor	0.003	
Health status imputed	0.018	
Child disabled	0.043	
Child disabled missing	0.030	
Health insurance: uninsured	0.137	
Health insurance: public coverage	0.168	
Health insurance: private coverage	0.695	
Family characteristics:		
Single mother household	0.217	
Single father household	0.039	
Mom (dad) education: < high school degree	0.123	
Mom (dad) education: high school degree	0.302	
Mom (dad) education: >= some college	0.575	
Mom disabled	0.087	
Mom disability missing	0.091	
Stay home parent	0.348	
Average income as percent of poverty level	312.90	(309.82)

Source: Calculations from 1996, 2001, 2004 SIPP.

Table 3.3. Regression Results: Number of Children and Children's Medical and Dental Care

	Medical Visits		Dental Visits	
Number of Children	***Probability of Visit***	***Number of Visits***	***Probability of Visit***	***Number of Visits***
2 child	-0.031**,††	-0.268**,††	0.027**,††	0.004
	(0.004)	(0.054)	(0.004)	(0.023)
3 child	-0.025**	-0.385**,††	0.038*,††	0.014
	(0.004)	(0.060)	(0.005)	(0.027)
4 child	-0.050**,††	-0.642**,††	0.021**,††	-0.073**,††
	(0.006)	(0.079)	(0.006)	(0.036)
5-6 child	-0.080**,††	-0.964**,††	-0.012**,††	-0.223**,††
	(0.009)	(0.098)	(0.009)	(0.053)
Sample size	148,853	148,853	127,253	127,253

Results reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from reference group (only child) at $p<0.10$; ** $p<0.05$.
† indicates coefficient is statistically different from *previous category* (e.g., number of children 3 v 2, 4 v 3, 5 v 4) at $p<0.10$; †† $p<0.05$
All models control for the full set of covariates

Table 3.4. Medical and Dental Utilization by Newborn Sibling in the Family and Child's Age

Age group	Treatment			Control			DD
	T	T+1	Difference	T	T+1	Difference	
Medical Visits							
Probability of Visit							
0-4	0.854	0.801	-0.053	0.844	0.821	-0.023	-0.030
5-9	0.712	0.663	-0.048	0.737	0.725	-0.011	-0.037
10-14	0.654	0.692	0.038	0.672	0.701	0.028	0.010
Number of Visits							
0-4	4.906	3.362	-1.545	5.185	4.487	-0.698	-0.847
5-9	2.663	2.043	-0.620	3.261	3.228	-0.033	-0.587
10-14	1.910	1.962	0.051	2.697	2.697	0.000	0.052
Dental Visits							
Probability of Visit							
0-4	0.383	0.563	0.180	0.418	0.567	0.149	0.030
5-9	0.635	0.635	0.000	0.687	0.718	0.031	-0.031
10-14	0.654	0.641	-0.013	0.700	0.726	0.026	-0.039
Number of Visits							
0-4	0.665	0.961	0.296	0.684	1.045	0.361	-0.064
5-9	1.308	1.216	-0.091	1.511	1.617	0.106	-0.198
10-14	1.603	1.321	-0.282	2.271	2.312	0.041	-0.323

Source: Calculations from 1996, 2001, 2004 SIPP.
Sample: Children that enter SIPP panel as only child and (T) gained a newborn sibling in subsequent waves or (C) remained only child throughout SIPP panel.
Each child has exactly two observations in the data, T-pre-sibling observation, T+1, observation with newborn sibling.
DD is the unadjusted differences-in-differences estimator.

Table 3.5. Impact of a Newborn Sibling on Children's Utilization of Medical and Dental Care

Dependent Variable	All children (aged 0-14)	by Age		
		Age 0-4	Age 5-9	Age 10-14
Medical Visits				
Probability of Visit	-0.047**	-0.020	-0.031	0.006
	(0.017)	(0.018)	(0.039)	(0.061)
Number of Visits	-0.900**	-0.731**	-0.602	-0.162
	(0.185)	(0.260)	(0.483)	(0.489)
Dental Visits				
Probability of Visit	0.048**	0.036	-0.029	-0.033
	(0.023)	(0.038)	(0.038)	(0.064)
Number of Visits	0.138	-0.021	-0.213	-0.460
	(0.134)	(0.108)	(0.144)	(0.474)
Sample size (dental)	17,918 (13,130)	7,064 (2,276)	4,312	6,542

Sample: Children that enter SIPP panel as only child and (T) gained a newborn sibling in subsequent waves or (C) remained only child throughout SIPP panel.
Results reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from reference group (only child) at $p<0.10$; ** $p<0.05$.
All models control for the full set of covariates

Table 3.6. Pooled Regression Results: Birth Order and Children's Medical and Dental Care

	Two-child household	Three-child household	Four-child household
Medical Visits			
Probability of Visit			
Second child	-0.022**	-0.010	-0.007
	(0.005)	(0.007)	(0.015)
Third child	-	-0.045**,††	-0.007
		(0.009)	(0.017)
Fourth child	-	-	-0.017
			(0.020)
Number of Visits			
Second child	-0.283**	-0.192**	-0.075
	(0.063)	(0.085)	(0.147)
Third child	-	-0.338**,††	-0.215
		(0.098)	(0.142)
Fourth child	-	-	-0.402**,††
			(0.164)
Sample size	56,388	32,646	10,992
Dental Visits			
Probability of Visit			
Second child	0.020**	0.011	-0.012
	(0.005)	(0.007)	(0.015)
Third child	-	0.015*	-0.024
		(0.009)	(0.017)
Fourth child	-	-	-0.013
			(0.020)
Number of Visits			
Second child	0.103**	-0.025	-0.048
	(0.026)	(0.038)	(0.066)
Third child	-	-0.001	-0.095
		(0.041)	(0.073)
Fourth child	-	-	-0.097
			(0.078)
Sample size	48,973	28,580	9,653

Results reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from reference group (only child) at $p<0.10$; ** $p<0.05$.
† indicates coefficient is statistically different from *previous category* (e.g., number of children 3 v 2, 4 v 3, 5 v 4) at $p<0.10$; †† $p<0.05$
All models control for the full set of covariates

Table 3.7. Birth Order Effects on Children's Utilization of Medical and Dental Care: Second Born v. First Born

Dependent Variable	All children (aged 1-14)	by Age		
		Age 1-4	Age 5-9	Age 10-14
Medical Visits				
Probability of Visit	0.002	-0.018	-0.006	0.021
	(0.011)	(0.018)	(0.018)	(0.018)
Number of Visits	-0.068	-0.472**	-0.293**	0.166
	(0.123)	(0.243)	(0.159)	(0.154)
Dental Visits				
Probability of Visit	0.034**	0.080**	0.048**	0.005
	(0.011)	(0.030)	(0.017)	(0.016)
Number of Visits	0.080	0.110	0.189**	-0.101
	(0.060)	(0.074)	(0.068)	(0.10)
Sample size	8,160	2,150	2,814	3,196
(dental)	(7,234)	(1,224)		

Sample: Children from families with exactly two children over the SIPP panel. Restricted to first and second born children in those families where both children were surveyed at same age.
Results reported as average marginal effects, standard errors are reported in parentheses.
* indicates coefficient is statistically different from reference group (first born) at $p<0.10$; ** $p<0.05$.
All models control for the full set of covariates

CHAPTER 4

The Effect of State Insurance Design Features on Children's Insurance Coverage

Introduction

The past three decades has seen a dramatic expansion in public health insurance for children. Most recently, as part of the Balanced Budget Act (BBA) of 1997, the US Congress created the State Children's Health Insurance Program (SCHIP).[28] Under SCHIP, the federal government provided funds to states to increase income eligibility limits. The extent of the eligibility expansion varied across states, but in most states the implementation of SCHIP meant that children in families with incomes 100 and 200 percent of the federal poverty level gained access to public insurance. By the time SCHIP was fully implemented, children in families with incomes up to 200 percent of the Federal Poverty Level (FPL) were eligible for public insurance in nearly every state[29] (Cohen-Ross et al. 2009) and more than half of all children nationally were eligible for public health insurance (Currie 2009).

However, eligibility for public insurance does not ensure coverage. More than 7 million children were without health insurance in 2008 and it is estimated that roughly six in ten uninsured children were eligible for public insurance (Kenney et al. 2010).

[28] After the Children's Health Insurance Program Reauthorization Act of 2009, the program is now referred to using the acronym CHIP. Since the period of our analysis pre-dates this change in nomenclature, we refer to the program using the prior acronym, SCHIP.

[29] Only four states—Alaska, Idaho, North Dakota and Oklahoma—have an eligibility ceiling below 200 percent of the FPL.

Although the implementation of SCHIP greatly reduced the variation across states in public insurance eligibility standards, there remains considerable variation in the proportion of low-income children without insurance coverage. The rate of uninsurance for children in families with incomes below 200 percent of poverty ranges from 20 percent in New Hampshire to 53 percent in Mississippi (DeNavas-Walt et al. 2009). These statistics suggest the relevant policy issue is no longer expanding eligibility but increasing efforts to reach eligible children who are uninsured and preventing them from losing coverage once enrolled (Currie 2009).

A number of studies have analyzed changes in insurance coverage for children during the SCHIP era. This literature focuses mainly on estimating how much of this increase in public coverage was directly caused by the eligibility expansions and determining the extent to which the increase in public coverage was offset by reductions in private insurance coverage. The results generally indicate that the marginal take-up for SCHIP and the amount of "crowd-out" were comparable to the effects of the Medicaid expansions of the late 1980s and early 1990s (Rosenbach et al. 2001; Zuckerman et al. 2001; Cunningham, Reschovsky, and Hadley 2002; LoSasso and Buchmueller 2004; Hudson, Selden, and Banthin 2005; Gruber and Simon 2008).[30]

This literature provides little evidence on two other important aspects of the SCHIP expansions. One is the fact that after SCHIP went into effect coverage increased not only for children in the target income group, but also for lower income children who would have been eligible for Medicaid under the pre-SCHIP rules (Dubay and Kenney

[30] Generally speaking, these studies suggest the expansions resulted in a 5-20 percentage point increase in public coverage, of which 20-40 percent of the increase in public coverage came from private coverage crowd-out.

2009). Because they focus on the effect of the changes in income eligibility limits, most prior studies are not able to explain why coverage increased among these "always eligible" children. There are two possible (overlapping) explanations for this gain in coverage. One is that it represents a spillover effect of state marketing and outreach efforts targeted at newly eligible children. The other possible explanation is that the gains in coverage among very low income children were not the result of SCHIP per se, but rather were caused by administrative reforms that states introduced in their Medicaid programs around the same time that they were implementing SCHIP. A second important feature of the SCHIP expansion is that states were given unprecedented flexibility in implementing the program. States had the option of expanding their Medicaid programs, establishing a new stand-alone program, or doing both. They also had the option of adopting alternative strategies for improving take-up and retention and limiting crowd-out. The existing evidence on these program design features is limited and inconclusive.

In this study we investigate the relationship between program design features and increases in public insurance coverage. Understanding the relative effectiveness of different strategies adopted by states is important for future policy development. The 2009 Children's Health Insurance Reauthorization Act (CHIPRA) gives states incentives to implement program features and administrative reforms first introduced under the original SCHIP legislation. Specifically, the Federal government will award states with an annual bonus payment if they implement certain program simplification measures and exceed Medicaid targets for enrollment. As states move forward with their adoption of these measures in an effort to achieve the current administration's goal of ensuring that

all uninsured are enrolled in insurance programs for which they are eligible (Sebelius 2010), it is useful to examine how state's experience with these policies during the early years of the SCHIP program affected children's coverage.

Background and Previous Literature

The State Children's Health Insurance Program

The Balanced Budget Act of 1997 authorized $40 billion in Federal funds to assist states in providing public coverage to children targeted under the SCHIP program. States were given three broad options for doing so: they could expand their Medicaid programs by either increasing income eligibility thresholds or extending coverage to age groups that were not previously eligible; they could create a new separate health insurance program for children, or they could expand Medicaid eligibility up to a certain income level and create a separate program for children with family incomes above that level. There are pros and cons with each approach.

The main arguments in favor of the Medicaid expansion strategy have to do with simplicity. States could easily build on existing infrastructure for administration, enrollment, and claims processing. There are also simplicity arguments related to communications and eligibility determination. A single program is likely to be less confusing to families and to program administrators. With respect to eligibility, the key question is whether a family's income is below a single threshold. In the case of retention, the potential advantage of the Medicaid expansion approach is that children do not have to change programs when their incomes rise above the Medicaid limit but remain below the SCHIP eligibility threshold. States that expanded Medicaid may in fact be in better position to reduce any "churning" due to the fact that there may be less

confusion among families around eligibility and renewal decisions in states with only one program. This issue might be particularly acute for children in families around the income cutoffs between traditional Medicaid and the SCHIP expansion group. One disadvantage of expanding Medicaid is that it requires conforming to existing federal rules that some states view as burdensome and inefficient. In addition, any negative reputation or stigma associated with Medicaid would apply to the expanded coverage. A greater ability to minimize stigma was one argument for establishing a new, separate program. States that chose this option also had somewhat greater flexibility to tailor their programs to better meet the needs of children in a particular state. Potential downsides of this strategy include higher start-up costs and a more complicated process for determining eligibility. By 2000, after all state SCHIP programs were up and running, 17 states had chosen to expand Medicaid, 16 established separate programs and 18 states had done a combination of the two approaches (Cohen-Ross and Cox 2000). [31]

Because it was estimated that roughly three of five children made eligible by the SCHIP expansions already had private health insurance, there was great concern that extending public insurance eligibility to children whose parents work and have the option of employer sponsored insurance could induce them to drop this private coverage. While all states had to enact policies mitigate crowd-out in their SCHIP programs, they were allowed to choose their own strategy for addressing this issue. In an effort to reduce the problem, many states required that children must be without insurance for some period (typically three to twelve months) prior to enrolling. For the most part, use of waiting periods was limited to separate state programs, as Federal regulations prohibit the use of such policies in state Medicaid programs, although waivers to establish waiting periods

[31] Three states (Maryland, South Dakota, and Idaho) added a separate program after 2000.

for children eligible under Medicaid expansion income limits have been given to a few states.

States were also encouraged to use the flexibility allowed under the BBA to simplify application procedures in their Medicaid and SCHIP programs as a strategy for boosting enrollment. The BBA created the option of adopting a policy of "presumptive eligibility" for children, whereby "qualified entities" such as health care providers and other social service agencies are allowed to enroll children temporarily in Medicaid if their family income appears to be below state guidelines, giving families time to complete the often burdensome application process. In addition to allowing traditional providers like clinics and hospitals the ability to help families with the application process, states have the option of also allowing community based organizations like primary or secondary schools and organizations that determine eligibility for Head Start, the Special Supplemental Program for Women, Infants, and Children (WIC), and the Child Care and Development Block Grant program to grant temporary eligibility based on information they collect for those programs. Studies have shown that a majority of uninsured children are from families that participate in other government programs such as the National School Lunch Program, the WIC program and the Food Stamp program (Kenney, Haley, and Ullman 1999). Bringing the Medicaid application to a variety of access points increases opportunities for low-income families to apply for their child's coverage. It is possible, however, that a policy of presumptive eligibility could actually decrease program enrollment if parents feel less inclined to enroll healthy eligible children if they believe they can enroll them at the point of care when the health need arises.

States were also allowed to simplify the enrollment process by reducing the amount of documentation families needed to apply for public insurance and by accepting family's self-declaration of income. Allowing self-declaration of income makes it more likely that a family will complete the application process and subsequently enroll their children in coverage, as studies have documented that difficulty gathering necessary documentation serves as a major reason that families fail to complete applications or are denied coverage (Cox 2001, Cohen-Ross and Cox 2000).

Presumptive eligibility and self-declaration of income are intended to increase take-up of public insurance. Another way to increase coverage is to improve program retention. Toward this end, states were given the option of providing up to 12-months of continuous eligibility to children enrolled under state Medicaid or SCHIP programs (Morreale and English 2003). Under a policy of 12-month continuous eligibility, states provide a year of guaranteed coverage after enrollment, regardless of whether changes in income or family structure affect a child's eligibility for the program. While continuous eligibility is unlikely to significantly increase *take-up* of coverage, it is expected to increase the stability of children's coverage by reducing discontinuous coverage due to temporary fluctuations in income or the complexities associated with ongoing eligibility verification processes (Irvin et al. 2001).

Previous Research

Several studies estimate the effect of the SCHIP program on insurance coverage (Rosenbach et al. 2001; Zuckerman et al. 2001; Cunningham, Reschovsky, and Hadley 2002; LoSasso and Buchmueller 2004; Hudson, Selden, and Banthin 2005; Gruber and Simon 2008). For the most part, the question asked by these prior studies is narrowly

focused on how the change in eligibility limits affected public and private coverage among children in the target group and pays little attention to how these effects are related to specific program design features.[32] In addition, the econometric approach used in the majority of these studies identifies coverage gains directly associated with the change in eligibility – assuming away any effect of the program on children who would have been eligible previously. There is reason to believe that the marketing and outreach efforts and administrative reforms of SCHIP would have also increased public coverage for children already eligible for Medicaid. A recent study by Dubay and Kenney (2009) estimate a sizable "spillover" effect of SCHIP implementation on children eligible for Medicaid under the income thresholds in place prior to SCHIP, on par with the range of estimates they find for children directly targeted by the expansion. The identification approach used in these studies is unable to test for this effect. Additionally, because part of the identification comes from differences across states in the timing of eligibility expansions, these papers assume that the program effects happen instantaneously.

There are fewer studies examining the effects of state program design on coverage rates. Three recent studies use national data from the March Current Population Survey (CPS) to test for differences across SCHIP program type and shed light on how specific program characteristics other than income eligibility limits are related to public insurance coverage. In an analysis of data from the March 2001 CPS, Kronebusch and Elbel (2004)

[32] There are two exceptions to this statement. LoSasso and Buchmueller (2004) test for the difference in the marginal take-up rate between states that expanded Medicaid and those that established separate programs. Their point estimates imply a slightly larger effect for stand-alone programs, though the difference by program type was not precisely estimated. They also test for an effect on state requirements that children be uninsured for a specified number of months before enrolling in SCHIP. They find that longer waiting periods are negatively related to public insurance take-up and positively related to private coverage. Gruber and Simon (2008) separately test the effect of two anti-crowd-out methods – waiting periods and cost-sharing requirements – and find some evidence that each reduce the take-up of public insurance.

find that public insurance coverage is positively related to enrollment simplifications (presumptive eligibility and self-declaration of income) and negatively related to anti-crowd-out policies (waiting periods and premiums). They also find higher rates of public insurance in states that expanded their existing Medicaid program compared to states that established new stand-alone programs. The main limitation of this analysis stems from the cross-sectional nature of the research design: with data from a single year it is not possible to disentangle the effect of particular design features from underlying differences in the states that took different approaches.

Subsequent studies by Wolfe and Scrivner (2005) and Raphael and Bansak (2006) each use two years of CPS data to examine the effect of SCHIP program design on insurance coverage. Wolfe and Scrivner use 2000 and 2001 data, which describe insurance coverage in calendar years 1999 and 2000. While some of their results are similar to Kronebusch and Elbel—for example, they find that waiting periods reduce public insurance take-up and increase the probability of being uninsured—other results differ. In particular, their pooled cross-sectional estimates imply a higher take-up rate in states with separate programs compared to Medicaid expansion states.

Bansak and Raphael (2006) compare insurance outcomes in 2001 to outcomes in 1997, just before SCHIP implementation. To estimate the differential effect of state policy choices, they estimate regressions in which program design variables are interacted with an indicator variable that differentiates the pre-SCHIP and post-SCHIP periods. They estimate the models with state fixed effects to account for unobserved state characteristics that may be correlated with both baseline levels of insurance coverage and program features. Like the two other studies, they find that waiting periods

designed to prevent crowd-out reduce the probability a child has public insurance. Their results also suggest that policies allowing for continuous enrollment increase public coverage. With respect to differences between Medicaid expansion states and those with separate SCHIP programs, their results are closer to Kronebusch and Elbel than Wolfe and Scrivner.

Like the studies focused on estimating the effect of the eligibility expansions, these studies generally do not attempt to distinguish between program take-up among children who gained public insurance eligibility as a result of the SCHIP expansions and the potential spillover effect of SCHIP on children already eligible for Medicaid. It is possible that program design effects may differ across these two groups. For example, we might expect the type of SCHIP program to have different effects depending on a child's household income. While separate programs may in fact be more attractive to families higher up the income distribution, establishing a separate program might lead to coordination issues that hinder their ability to capture spillover effects on Medicaid eligible children. In addition, states could apply administrative simplification measures to children eligible under Medicaid thresholds, SCHIP thresholds or both. Previous studies have generally not attempted to disentangle the targeting of these policies. Furthermore, all three prior studies assigned program design characteristics based on whether the policy was in effect at a point in time (eg, in April 2003). This strategy assumes that these policies were static when in fact states added, removed and changed these policies over the period. We identify state policies as they evolve over time to better attribute changes in coverage to the policies in effect when the child was observed. Since our data span the years 1996 to 2005 we have several years of post-SCHIP data to

better disentangle state's the effect of the initial implementation of state SCHIP programs and states' adoption of administrative simplification policies.

As mentioned above, two of the three studies identify program effects based on post-SCHIP differences. A limitation of this approach is that because all (or at least nearly all) of the data is for a period after SCHIP implementation, these studies are not able to assess the effectiveness of different program design features in increasing insurance coverage relative to pre-SCHIP levels. By using several years of pre- and post-SCHIP, our study is better able to estimate the effect of different program characteristics on children's insurance coverage by looking at changes in insurance coverage relative to the pre-SCHIP period.

Data and Measures

Data Source and Study Sample

Our analysis uses data from 1996, 2001 and 2004 panels of the Survey of Income and Program Participation (SIPP). [33] Combining data from the three panels gives us a data set that spans the SCHIP implementation period and gives us several years of data after all states had their programs in place. We limit our sample to children who were under age 15 at the start of a SIPP panel.

SIPP respondents are interviewed every four months. Each four month period is referred to as a wave. Each wave, there are a set of core questions that elicit basic information about family status, employment, income, program participation, and health insurance coverage for every month since the last interview. We combine data from three adjacent waves to form variables summarizing health insurance coverage and other

[33] The 1996 panel spans the period December 1995 to February 2000; the 2001 panel covers the period October 2000 to December 2003; and the first six waves of the 2004 panel covers the period October 2003 to December 2005.

economic variables over a 12 month period.[34] We drop observations where there is a gap in this information because the household missed an interview during the 12 month period. We also must drop observations from five states – Maine, North Dakota, South Dakota, Vermont and Wyoming – because these states are not uniquely identified in the 1996 and 2001 SIPP. Our sample thus consists of 63,233 observations on 22,577 children from the 1996 panel, 39,002 observations on 17,147 children from the 2001 panel, and 35,535 observations on 21,380 children from the 2004 panel.

The health insurance questions in the core data ask about the type of coverage held in each of the four months since the last interview. We form three health insurance coverage categories: uninsured, private insurance, and public insurance. The private category includes employer-sponsored group insurance, which accounts for the vast majority of private coverage, and non-group insurance.[35] The public category consists mainly of Medicaid and SCHIP plus a very small number of children who qualify for Medicare because of a disability or are covered by the Indian Health Service.

By combining responses from several waves, we are able to create variables that describe each child's insurance coverage experience over a 12-month period. Specifically, we construct variables capturing whether the child had various types of insurance all year (e.g., 12 continuous months of public coverage), whether the child ever had a particular type of insurance during the year (e.g., whether the child spent any part of the last 12 months without coverage), and the months of coverage for each type over the year. This continuous variable is preferable to the dichotomous measures that are

[34] The one exception to this selection criterion is that for children under a year old, we use all available months since the child was born.
[35] We code CHAMPUS, the insurance program for dependents of military personnel, as private coverage because it is employment related.

available in the CPS or other cross-sectional surveys like the National Health Interview Survey that measure coverage at a single point in time. The single point in time approach has the potentially important limitation that it fails to account for changes in insurance status over the course of the year nor does it account for the duration of coverage. This difference is particularly important for analyzing program design features that are intended to increase the length of insured spells or reduce the amount of "churning" – children moving on and off public insurance roll – of children eligible for Medicaid or SCHIP. Research has shown that insurance coverage is very fluid for low-income families and stability of insurance coverage is importantly related to children's access and utilization of health care (Buchmueller, Orzol, and Shore-Sheppard 2010; Leininger 2009; Olsen et al 2005; Aiken et al. 2004), providing more reason to focus on duration of coverage.

Control variables come from both the core survey and a topical module on health and medical care utilization that is administered annually. Because our methodological approach requires us to identify children in the SIPP that are eligible for public coverage, a key variable for our study is family income. We measure family income as the mean monthly income calculated over the same 12 month period used to construct the insurance variables. We combine this measure with family size to calculate a household's income to the federal poverty threshold.

Measures: Program Design Features

The state-level policy design features we investigate include SCHIP program type; the number of months a child has to be uninsured before enrolling in SCHIP (waiting periods); the policy of allowing families to self-declare their income;

presumptive eligibility; and 12-month continuous enrollment policies in Medicaid and SCHIP programs.[36] The waiting periods apply only to SCHIP programs. The other administrative reforms could be implemented in Medicaid or SCHIP or both. Table 4.1 summarizes the cross-state variation in these policy design features and how these have evolved over time.

As noted by Table 4.1, states were relatively consistent throughout the time period regarding their administrative approach to their SCHIP program. Roughly two-thirds of states created a separate program for some or all of the newly eligible children with the remaining states using their existing Medicaid program to serve these children. To test the relative merits of the different approaches, we create two variables, one representing whether the state covered their newly eligible children by expanding Medicaid and one for whether the state established SCHIP plans that are separate from their Medicaid program to cover some or all of their newly eligible children.

Policies focused on simplifying enrollment and redetermination procedures have become a focus of efforts to increase public coverage for children, although none of the three we examine had been widely adopted during our study period. Even the most frequent of the three, 12-month continuous eligible, was enacted by less than half of the states in our sample. Policies allowing for presumptive eligible and self-declaration of income are even more uncommon. To account for the three administrative simplification

[36] Multiple sources were used for establishing a summary of SCHIP program design characteristics, by year, for all states over the period 1997 to 2005, but information in the current draft was predominately obtained from the following four sources: Cohen-Ross and Cox (2000); Cohen-Ross (2002); Irvin (2001); and Rosenbach (2003).

policies, we create dummy variables for each, where each variable indicates whether the state had enacted the policy at the time the child was observed.[37]

In an effort to minimize "crowd-out" private insurance, a majority of states required children be uninsured for a period of time before being eligible for their SCHIP expansion programs, although some states later dropped this restriction. Using the waiting period policies states had in at the time the child was observed, we created a categorical measure based on the length of the waiting period: none, one to five months, six months or more.

Trends in Children's Coverage

Figure 4.1 shows how the relationship between income and insurance coverage changed between the years just prior to when any state implemented SCHIP (1996-1997), and the post-SCHIP period when all state programs were in place (2000-2005). We define ten income categories using monthly income as a percent of the FPL over the prior 12 months: less than 50% of the FPL, 50-100%, 100-150%, 150-200%, 200-250%, 250-300%, 300-350%, 350-400%, 400-500% and above 500%, which allows us to distinguish between groups that are likely to have been affected by the SCHIP implementation and groups that are likely to have been unaffected.

The outcome in Panel A is months without health insurance over a 12 month period. In Panel B it is months with public coverage. The main effect of the SCHIP expansion can be seen by focusing children with incomes between 100 and 200% of the

[37] To determine if the policies should enter our models as separate independent variables or whether we needed to define categories of states according to how these policies are bundled, we looked at two-by-two tables of the within state adoption of these programs (results not shown). If the variation was due to some states being highly generous (and others never generous) or whether policies were used as substitutes for each other, we would have expected to see a clustering of observations on these dimensions. Instead, there seems to be a fairly low correlation between the two dimensions of generosity, therefore, we treat the adoption of these policies as independent from one another.

FPL. For these children in the "SCHIP target group," months without insurance decreased and months with public insurance increased. It is interesting to note that we see a similar pattern for children with family incomes below the FPL. Since these children would have qualified for Medicaid under the pre-SCHIP rules, this gain in public coverage cannot be a direct effect of the eligibility expansion. Rather, this result is either explained by a spillover effect of SCHIP marketing and outreach efforts or an effect of the administrative reforms that were enacted along with the SCHIP expansion.

One of our main objectives is to test for differences between states that established a separate SCHIP program and those that expanded their Medicaid program. There are reasons to expect that these differences will vary across the income distribution. Because expanding Medicaid should have led to less administrative complexity and more streamlined procedures related to eligibility determination and enrollment, we hypothesize that there should be greater spillover in states that took this approach. That is, to the extent that SCHIP outreach and marketing efforts draw in children who are eligible for Medicaid it is less likely that these children will "slip between the cracks" and not successfully enroll in Medicaid when there is a single program. Coordination between Medicaid and SCHIP is less of an issue for children in the SCHIP target group. Furthermore, one argument for establishing a program separate from Medicaid is the social stigma associated with public programs. By establishing their programs outside of Medicaid and designing them to look more like private insurance, states hoped to attract families who might be unwilling to enroll in Medicaid.

Figure 4.2 presents preliminary evidence on these differences. As hypothesized, for children with family incomes below the FPL we see larger changes in coverage in the

Medicaid expansion states. In the pre-SCHIP period, poor children in these states had slightly fewer months with public insurance than those in states that went on to set up separate programs. In the post-SCHIP period, however the difference is reversed. Comparing changes in coverage we see that poor children in Medicaid expansion states gained an average of 1.5 months of public coverage compared to a gain of 0.5 months for children in separate program states. We see a similar pattern for months without coverage. In the pre-SCHIP period average months without coverage was roughly the same across administrative type, approximately three months without coverage. However, looking at lines depicting the post-SCHIP period, months without insurance coverage fell to approximately two months in Medicaid expansion states while separate program states saw a decline by approximately half that amount, to roughly 2.5 months. On the other hand, among children with incomes between 100% and 200% of the FPL it is hard to discern a differential change by program type. While the data suggest that perhaps states with Medicaid expansion saw a greater increase in public coverage, if so the differences are quite small and likely not statistically different.

Methods

Taken together, the figures provide evidence that the SCHIP program has increased months insured not only among children targeted by the SCHIP eligibility expansions, but also children who were already eligible for public coverage under pre-SCHIP Medicaid thresholds, although because the SCHIP expansions did not change the program eligibility of these children it is difficult to precisely test this hypothesis. There is also suggestive evidence that state's administrative approach to their SCHIP program may impact changes in coverage, in particular among children eligible for Medicaid

under the old eligibility guidelines who might have been indirectly affected by SCHIP implementation. However, before drawing that conclusion, it is important to be sure that this apparent spillover effect is not driven by changes in the characteristics of low income children or that differences related to state program type are not the result of other policies enacted around the same time as SCHIP was implemented. Similarly, these policies might importantly influence state's success and enrolling and retaining children made newly eligible for public coverage in much the same way.

As noted earlier, prior research on the SCHIP program has largely focused on its success in enrolling newly eligible children in public coverage. To estimate the impact of the implementation of state SCHIP programs on insurance coverage, one typical approach starts with the following equation estimated on a sample of children who were likely affected by the SCHIP eligibility expansions:

$$Coverage_{it} = \alpha + \beta_1 * PostSCHIP_t + \gamma X + \varepsilon_{it} \quad (1)$$

where *Coverage* indicates a measure of insurance coverage for child *i* at time *t*. The coefficient on the *PostSCHIP*, β_1, provides an estimate of the effect of SCHIP on children's insurance coverage. There are two shortcomings with this approach. Low income children who were already eligible for public coverage do not contribute to the identification in these studies, thus any potential impact of SCHIP on their insurance coverage is assumed to be zero. Likewise, these and other studies of the SCHIP program have largely ignored any role that states' program design and administrative reforms might have on their success in increasing insurance coverage among low-income children.

To account for these potentially important policy changes and assess the effects of state's program design features adopted during the study period, we incorporate state's adoption of policies allowing for 12-month continuous eligibility, presumptive eligibility and self-declaration of income by estimating the following model:

$$Coverage_{it} = \alpha + \beta_1 * PostSCHIP_t + \gamma PD_t + \lambda X + \varepsilon_{it} \quad (2)$$

where *PD* is our vector of program design variables, each entering into the equation as a dummy variable indicating whether child *i*'s state has adopted the simplification policy in their Medicaid or SCHIP program at time *t*. The vector *X* includes a set of individual, household and state level control variables. To separately account for states implementation of their SCHIP program, we use data on the timing of SCHIP implementation in each state to delineate pre- and post-SCHIP periods (*PostSCHIP*).[38]

The approach outlined assumes that the impact of the implementation of SCHIP on insurance coverage is the same for states that expanded Medicaid as those states that established a separate program. As discussed above, we might not expect this to be the case. Therefore, we rerun models (1) & (2) replacing *PostSCHIP* with two variables – *SEP* and *MEXP* – indicators for the type of administrative model for their SCHIP program adopted by the state[39].

$$Coverage_{it} = \alpha + \beta_1 * MEXP_t + \beta_2 * SEP_t + \gamma X + \varepsilon_{it} \quad (3)$$

$$Coverage_{it} = \alpha + \beta_1 * MEXP_t + \beta_2 * SEP_t + \gamma PD_t + \lambda X + \varepsilon_{it} \quad (4)$$

[38] By doing so the pre- and post-periods will differ by state. As an example, for children residing in the earliest adopting states (end of 1997), the first calendar year in the post-SCHIP period would be 1998. For children residing in the last states to implement their SCHIP programs (Hawaii and Washington began enrollment in Jan, 2000) 1998 and 1999 would be in pre-SCHIP period and 2000 would start the post-SCHIP period.

[39] States that used a combination approach are categorized as states with a separate program.

These models allow us to test for a differential effect on insurance coverage between the two administrative approaches.

Because we are interested in estimating the effect of SCHIP and the newly adopted program design features not only on children newly eligible for public coverage, but those already eligible under old Medicaid guidelines, we estimate models separately by income. Specifically, we create two samples of low-income children: (1) children whose family income indicates they were eligible for Medicaid under their state's age specific pre-SCHIP Medicaid eligibility thresholds and (2) children whose family income is above the pre-SCHIP Medicaid threshold but at or below their state's age-specific SCHIP maximum thresholds as of July 2005.[40] The key distinction between these two categories is that children in the first category were already eligible for public insurance prior to SCHIP, while those in the second gained eligibility as a result of the SCHIP expansions.

Testing for Policy Endogeneity

As noted earlier, findings on the relationship between program type and insurance coverage from previous studies are decidedly mixed. One possible explanation for the conflicting results may be differences in how different studies addressed the potential problem of policy endogeneity – whether state decisions concerning their choice of program, or other program design decisions, were based on pre-SCHIP trends in insurance coverage. Some of the prior studies assume that there were no pre-existing differences between states by their choice of SCHIP plan. If that assumption is wrong

[40] This threshold ranges from a low of 150-percent of the FPL in Montana and South Carolina to 350-percent in New Jersey.

and in fact choice of program was in part based on anticipated trends in coverage, estimates from models that ignore prior state differences might be biased.

To determine whether this might be cause for concern, we want to examine the distribution of insurance coverage across states prior to the SCHIP program. We use state specific SCHIP implementation dates to limit our sample to include only children observed in the years prior to their state's implementation of their SCHIP program (1996-1998). Using linear regression models, we examine the relationship between states' choice of SCHIP program and our measures of insurance coverage in the period preceding states' implementation of the program. Results are reported in Table 4.2. The results suggest that states that expanded their Medicaid programs were measurably different in terms of children's coverage in the years preceding SCHIP than states using a separate program approach. Looking at months of coverage, in states that subsequently established separate programs children had slightly more months of public insurance (0.66 months) and less time without insurance (-0.34 months) than similar children in states that would go on to expand Medicaid under SCHIP. Comparisons based on measures capturing coverage at any point over the period reveal similar differences in access to public coverage between the administrative types. These comparisons also show that in the pre-SCHIP period the percentage of children with private coverage was between 2 and 3 percentage points lower in states that went on to established separate programs compared to states that only expanded Medicaid.

These pre-SCHIP differences in coverage may confound an analysis attempting to compare the effectiveness of alternative expansion strategies. To isolate program effects from these pre-existing differences, in addition to controlling for a set of observed

household and individual factors in our regression specifications we condition on state fixed effects to capture time-invariant characteristics of the state that could influence decisions on the type or timing of these program design decisions. In addition, to control for state specific trends that are most likely to influence states in their choice of administrative model or other program design policies, we include state unemployment rates and state child poverty rates.[41]

Results

Spillover Effects on Medicaid Eligible Children

Table 4.3 presents results from regressions on our sample of children from households whose yearly income indicates there are eligible for public coverage under old Medicaid thresholds. We start with the overall pre-post comparison (column 1). Similar to descriptive evidence in the figures and Table 4.1, the results suggest that the rollout of the state SCHIP programs had significant spillover effects on Medicaid eligible children. Months of uninsurance (panel 1) declined by almost a quarter-month (-0.24 months) and months of public coverage (panel 2) increased by over a full month (1.04 months) compared to the pre-SCHIP period. Results using alternative coverage variables tell a similar story. For example, using indicators for whether the child ever had a particular type of insurance during the year such as whether the child was ever without coverage (panel 4), or had at least one month with public coverage (panel 5), we find a 5.0 percentage point decline in the percentage of Medicaid eligible children who were ever uninsured and a 6.0 percentage point increase in the percent with public coverage at

[41] Covariates include indicator variables for age (16 categories), health status (5 categories), maternal education (12 categories), gender, race, Hispanic ethnicity, family headed by a single mother, mother disabled and whether the maternal disability indicator is missing, whether health status was imputed, average income as percent of poverty (categorical), state of residence, yearly state unemployment rate, and yearly state child poverty rate.

some point, suggesting increased take-up among children who were eligible for Medicaid but not enrolled. In addition, the eligibility expansions under SCHIP might have had an effect on retention. After SCHIP was in place the program's higher income eligibility limits for public coverage may have reduced the likelihood that children would lose eligibility and coverage during periods of income or family structure fluctuation. While not a direct test of retention, we estimate the change in the percent of children with public coverage over a continuous 12-month period (panel 7) and find an 11.8 percent increase over the pre-SCHIP period, suggesting that retention (as measured by continuous coverage over the year) did improve for this sample of children after the implementation of the SCHIP programs.

One explanation for this apparent "spillover" result is that SCHIP-related outreach efforts were effective in increasing awareness among families of children who were previously eligible for Medicaid but uninsured. Alternatively, administrative reforms enacted by states during the study period might be driving these results. To test, we augment the model by including indictors for our three program design features – allowing for 12-month continuous eligibility, presumptive eligibility and self-declaration of income. The results are reported in column 2.

First, establishing a policy of 12-month continuous coverage significantly increased overall time on public coverage (0.30 months, panel 2), with a corresponding decrease in the amount of time children are without insurance (-0.21 months, panel 1). Likewise, continuous eligibility policies led to a gain in the number of children who had public coverage over the entire 12-month period observation (2.2 percentage point increase, panel 6). Next, we find suggestive evidence that allowing for presumptive

eligibility in Medicaid increased months of public coverage by a little over a quarter-month (0.26 months, panel 2), but with no corresponding decrease in months without coverage (panel 1) Instead, we find a corresponding decrease in private coverage (-0.21 months, panel 3), suggesting that allowing for presumptive eligibility may result in some private coverage crowd-out. Regarding length of public coverage, we find a large effect of presumptive eligibility policies on the change in children with a full-year of public coverage (4.1 percentage points, panel 6).

Lastly, we examine the effect of policies allowing families to self-declare their income when applying for Medicaid. As expected, adoption of these policies does appear to result in children's public coverage gains, as states that enacted this policy had a significant increase in both months of public coverage (0.33 months, panel 2) and the proportion of children with at least one month of public coverage (2.9 percentage points, panel 4) .

To test whether either administrative type was better positioned to capture spillover effects on Medicaid eligibility children, we replace the post-SCHIP dummy with variables indicating the administrative type in effect at the time the child was observed – Medicaid expansion or establishing a separate SCHIP program. These results are reported in columns 3 and 4. Results from models that allow for different effects of SCHIP by program type suggest that there were larger spillover effects in states that expanded Medicaid than states that chose to set up a separate state program. Looking first at models without inclusion of other program design features (column 3), in states that chose to expand Medicaid, months without coverage decreased by 0.35 months, compared to 0.19 in states that established separate programs (panel 1), although this

difference is not statistically significant. However, Medicaid expansion states did see a significantly larger increase in months with public coverage than did states with separate programs (1.34 v 0.92 months), although some of this might have been offset by a larger decrease in private coverage among children in Medicaid expansion states (-0.71 months v -0.47 months). Regressions using insurance measures for whether the child had the type of insurance at some point over the year tell a similar story. Compared to states with separate programs, Medicaid expansion states had a greater increase in the likelihood of having at least one month of public coverage (panel 5) and a larger decline in ever having private coverage (panel 6).

One hypothesis for larger effects in states that chose to build upon their Medicaid program rather than establishing a program separate from Medicaid is that children from families with income near the cutoff between the two programs might be less likely to lose public coverage. Using our measure of a full year of public coverage as a proxy for retention (panel7), we see evidence that this might be the case, as Medicaid expansion states had a significantly larger increase in the proportion of children with full year public coverage than did states with a separate program (14.5 versus 9.5 percent). Regressions including our three program design features (column 4), yield similar orderings and offer further support for the general conclusion that expansions of Medicaid led to larger spillover effects among children already eligible for public coverage than did establishing programs separate from Medicaid.

Newly Eligible

We now turn to exploring how SCHIP implementation impacted a second group of children – those explicitly targeted by the eligibility expansions. These are children

made newly eligible for public insurance under the eligibility expansions under SCHIP. Table 4.4 presents estimates of the impact of SCHIP implementation and the adoption of our three program design characteristics on our seven main outcome variables for this group of children. The layout of the table is the same as Table 4.3.

The first column is the estimate of the change in coverage due to the implementation of SCHIP. These results indicate the eligibility expansions resulted in a sizable, statistically significant increase in public insurance and a decrease in months without insurance. The model examining the months of coverage suggests an increase of over one-and-a-half months of public insurance due to SCHIP (1.67 months, panel 2) with a smaller, but still notable, decline in months without insurance (-0.34 months, panel 1). Panel 3 presents the change in months with private insurance and indicates that private coverage fell by almost 1 month over the SCHIP period. While this result might suggest considerable crowd-out due to the SCHIP eligibility expansions, private coverage also declined for children above the SCHIP eligibility thresholds (results not shown), suggesting that low-income children would have seen their private coverage rates decline even in the absence of SCHIP. Similar conclusions – large increase in the proportion of children with public coverage, statistically significant but much smaller decline in the proportion of children with a spell without coverage, can be taken from the results in panels 4 and 5.

Results in the second column are from models in which we include indicators for our program design features. We again start by noting the change in the coefficient on the post-SCHIP indicator after accounting for the three program design features. Unlike the results with the already eligible sample, here we note that inclusion of these variables

notably reduced the post-SCHIP coefficients, although the estimated impact of SCHIP is still sizable and significant. For example, looking first at months of coverage, we see the coefficient on months without coverage declining by over 50-percent, from -0.34 to -0.16 months, and a decline from 1.67 to 1.39 months for months of public coverage, a 17-percentage decrease, suggesting that some of the apparent impact of SCHIP implementation can be explained by these other aspects of state's public insurance program design.

Turning to the coefficients on the program characteristics, we find a significant impact of these policies on several of our outcome variables. Similar to the results from the "already eligible" sample, policies allowing for 12-month continuous eligibility led to an increase in months with public coverage by almost a quarter month (0.20) and decreased months without coverage by a slightly larger amount (-0.38 months). Looking at months with private coverage, we also find suggestive evidence that continuous eligibility policies increased months with *private* coverage, although it is unclear why this would happen. One possible explanation is that insurance coverage could be mis-measured – respondents thinking they have private coverage when in fact they have public coverage. We might expect this problem to be particularly acute for this sample of children, families with children newly eligible for public coverage, as one of the selling points of the SCHIP program was that it wasn't an entitlement program and the insurance programs of many states were designed to look like private insurance. In addition, some state SCHIP programs require some enrollees to pay premiums. These features of separate SCHIP programs may cause confusion and some families may believe their child in fact has private insurance. In terms of whether policies of continuous eligible

increased the length of public insurance spells, we find those states that enacted such policies had an increase in the proportion of children with full-year public coverage (2.0 percentage points, panel 7).

Moving to our two policies associated with ease of enrollment – presumptive eligibility and self-reporting income – we find evidence of a significant positive effect on public coverage. Looking at months of public coverage (panel 2), presumptive eligibility is associated with a 1.10 month increase and allowing for self-reporting of income, approximately a half-month increase (0.42). Similarly, looking at the proportion of children having a spell of public coverage (panel 4), these simplification polices increase take-up of public coverage by 9.3 and 4.8 percentage points, respectively. However, unlike with policies allowing for continuous eligibility, we do not find a correspondingly meaningful decrease in uninsurance.

As before, we want to test for differences between the administrative structure of state's SCHIP programs. Unlike the findings for the "already eligible" sample, here the results across our six outcome measures are more decidedly mixed. Whereas results from the "already eligible" sample strongly suggested that states with Medicaid expansions experience larger gains in public coverage and a correspondingly greater decrease in uninsurance, among the newly eligible children, the administrative type has a more complex relationship with coverage.

Starting first with months of coverage, the first notable finding is that controlling for the other program design features (column 4) influences the differences we see in administrative type. This is likely due to the fact that states with separate programs were more likely to adopt these policy features. For example, in a model without adjusting for

policy features, we find no evidence of a difference between expanding Medicaid and establishing a separate program in months without coverage (panel 1). However, using our preferred approach and simultaneously adjusting for these three policies, we now find a significant difference, as expanding eligibility using a Medicaid expansion approach is associated with significantly greater decrease in months without coverage (-0.39 vs. -0.03 months; panel 1). Moving down to the next two panels, while we do not find evidence that administrative type was associated with any difference in months of public coverage (1.41 vs. 1.38 months, column 4, panel 2), we do find a significant difference in months with private coverage, with separate programs having a greater decrease than Medicaid expansion states (-1.05 months vs. 0.70 months, panel 3). One interpretation of these findings is that separate programs were more attractive to families familiar with private coverage than were Medicaid programs, leading to families in these states being more likely to drop private coverage for subsidized public coverage.

Interestingly, when looking at take-up of public coverage (panel 5), there is suggestive evidence of a difference between the administrative types, but we now find that separate programs were stronger at enrolling eligible children than were Medicaid expansion programs, as children living in states with separate programs experienced a 15.8 percentage point increase, compared to 13.3 percentage points among those in Medicaid expansion states (p=0.08). One again, however, the Medicaid expansion approach is associated with a reduction in periods of uninsurance (panel 4) – 2.9 percentage point decline in ever having a month without coverage, while we see no evidence of that among separate programs (1.0 percentage point increase, not significant).

One potential factor that we have not taken into account in looking at administrative type is that states that established separate programs were required to adopt methods to reduce crowd-out. To comply, most states used waiting periods – requiring children to be without insurance for a period of time before enrolling in the separate state program. Because waiting periods are not permitted in Medicaid programs without a waiver, the children most vulnerable to this policy are those made eligible for coverage through the SCHIP eligibility expansion. Because waiting periods will likely dissuade many families eligible for coverage from actually enrolling, we would expect that the change in public coverage outcomes will be smaller than in states without waiting periods. Similarly, to the extent that eligibility for public coverage lead to private coverage crowd-out, we would expect that states with waiting periods would see a smaller decline in private coverage.

To estimate the impact of waiting periods, we interact the separate SCHIP program indicator with the waiting period policy for that state program at the time the child was observed, using a categorical variable indicating the length of waiting period – none, 1 to 5 months and 6 or more months. In Table 4.5, we present results for our three months of coverage outcomes and our three take-up outcomes for the full newly eligible sample (first panel) and a subsample limited to children from states that chose to expand coverage using either Medicaid expansion or separate programs (second panel).[42]

[42] We remove combination states in our subsample analysis to better isolate children affected by waiting periods. In combination programs, some children in this broad income group will be eligible for Medicaid (through the Medicaid expansion portion of the program) and some eligible for the separate program. Those eligible for Medicaid would not be subject to the waiting period. In this draft we do not try and differentiate between the two groups of children, so we conduct our analysis on waiting periods a second time without those states.

Starting with the models in which all programs are included, we find some suggestive evidence that this policy not only had an impact on public coverage, but also on private coverage crowd out. While states without waiting periods or relatively short waiting periods (1 to 5 months) had a pre-post increase in public coverage of approximately 1.5 months, those states with longer waiting period had an increase in public coverage of approximately two-thirds that amount. We find a similar pattern when looking at having any spell of public coverage (15 to 19 percentage point increase compared to 11-percentage point increase). However, this might be in part due to doing a better job of deterring crowd-out of private coverage, as states with longer waiting periods saw a smaller decline in both months of private coverage and having any period of private coverage, than did states with shorter waiting periods.

The results when limiting our analysis to Medicaid expansion and separate program states are in general similar to the full sample results. One interesting finding among this sample of children is that states with relatively short waiting periods saw an increase in the proportion of children with a period of uninsurance. This result might be due to families dropping private coverage for public coverage but having to endure a spell without coverage due to the waiting periods. Coupling this result with the finding of little difference in private coverage between states with no waiting periods suggests that little is gained from having relatively short waiting periods with the unintended side effects of increasing rates of uninsurance.

Conclusions

This paper uses data from the 1996, 2001 and 2004 SIPP to examine how variation in SCHIP program design was related to states' effectiveness in increasing

children's insurance coverage. We note several important findings. First, we find that SCHIP had effects on children outside those specifically targeted by the eligibility expansions. We document large spillover effects on children who were eligible for Medicaid under the pre-SCHIP eligibility thresholds. We also find that decisions on program design are importantly related to children's coverage. Specifically, the spillover effect was most pronounced in states that expanded existing Medicaid programs, relative to states that used separate programs. Results also suggest an effect of program type on higher-income children made eligible by the expansions. However, the story is somewhat different. We find some evidence that separate programs were more attractive to newly eligible children, as our results suggest that states that established stand-alone programs may have had greater take-up of public coverage, although we did not find a difference in months of public coverage between the two administrative types and did find that Medicaid expansion states had greater gains in lowering uninsurance rates.

Taken together, the program type results suggest that the administrative type matters differently for the newly eligible children than children eligible for public insurance under Medicaid guidelines. This suggests that states face a trade-off in choosing between the two administrative structures – expand coverage via Medicaid to better capture spillover enrollment among the already eligible or establish a separate program to better attract the newly eligible. While we might have expected the later result – the conventional wisdom behind establishing separate SCHIP programs was that they would be more appealing to the target families than Medicaid programs and their associated social stigma – we are less clear about *why* we see a difference in spillover across administrative type. While we cannot precisely say why, we hypothesize that it is

possible that expanding Medicaid offers better integration both for administrative staff overseeing applications for eligibility and renewal, and for families of children eligible for public coverage whose income or family circumstance might require navigating children between two programs. If so, states have many options available to them to better align enrollment procedures across children's insurance programs to avoid complications that could potentially result in lower enrollment, strategies such as keeping a consistent program name, having a joint application, and having one program administrator. More research is needed to assess whether these or other coordination strategies explain (away) the "spillover" gap we find across administrative types.

We also find that that other state insurance program features do matter to coverage of low-income children, our results show significant positive effects on coverage of state simplification strategies– policies of 12-months continuous coverage, presumptive eligibility and self-declaration of income – even after accounting for the rollout of SCHIP implementation. We also note that waiting period policies enacted to prevent crowd-out had a statistically significant effect on both public and private coverage.

These results suggest that some of the variation we see between states in children's uninsured rates is likely in part due to the specific design features of individual states' public insurance programs. Efforts to streamline enrollment and redetermination procedures remove barriers to coverage and make it more likely that children enroll and retain coverage they are eligible for. Under policies of presumptive eligibility, states attempt to ease the burden of enrollment by expanding the number of access points for enrollment or allowing temporary eligibility while processing the applications of children

who appear eligible. Our results suggest that these policies increase the likelihood that children eligible for public coverage enroll, although perhaps at the expense of private coverage. Likewise, we hypothesize that allowing individuals to self-declare income would increase the likelihood families would complete the application process. The results support this hypothesis, suggesting that income declaration can be a burdensome process, especially among low-income families who work multiple or non-traditional jobs and may have a difficult time gathering the necessary documentation. Finally, our results lend support to the hypothesis that policies allowing for continuous enrollment reduce enrollment cycling due to income fluctuation and the likelihood of eligible children dropping coverage due to the burden around more frequent redetermination.

There are several limitations of this study that deserve mention. The main concern with our empirical approach is that potentially confounding trends that occurred during the study period might bias our results. For example, job losses suffered during the recession of the early 2000's might have made more children eligible for public coverage. In estimating the pre- and post-SCHIP change on coverage, we might attribute a rise in public coverage to the implementation of SCHIP, when in fact it was due to larger economic trends that are not captured in our models, such as a shock to employment. The approach we use to estimate the change in coverage associated with the implementation of SCHIP and the adoption of program design features are largely descriptive, and thus our results must be interpreted with caution. An additional limitation is we are unable to directly measure eligibility for public coverage. To assess eligibility, we use information on the child's age and household income and the eligibility rules specific to the state at that particular point in time. However, this approach is likely

to suffer from some misclassification as we use the average household income over the year, rather than income at the time of enrollment or redetermination. Finally, while we look across a range of insurance coverage measures, our vector of outcome variables was not exhaustive. In particular, without use of administrative data, we cannot directly assess insurance coverage take-up. While our results are suggestive that program features increased months of coverage by inducing take-up, they also might have increased length of coverage spell. Caution should be used in generalizing these results specifically to take-up or disenrollment.

Despite these limitations, this study helps shed new light on the role that specific program characteristics, outside of the income eligibility limits, have in facilitating children's enrollment in public insurance coverage. Efforts by the federal government to encourage adoption of enrollment simplification policies are likely to help ensure that uninsured children are enrolled in insurance programs for which they are eligible. While not all strategies might be appropriate for each individual state, many of the strategies can be implemented with relative ease. A good first step for many states that have adopted the program for either Medicaid or SCHIP would be to carry the policies over to the other program. In particular, use of these policies among the Medicaid-eligible population is especially promising, as there appears to be little or no effect of these policies on the private coverage, suggesting that adoption of these simplification policies will lead to net insurance coverage gains among this sample of children, who are unlikely to have many private insurance options.

Our results indicate that both populations benefit from the simplification policies examined. The findings suggest that, as a starting point for states, policies allowing for 12-month continuous eligibility policies may be especially effective at reducing periods without coverage. In both the already eligible and newly eligible samples, we find that continuous eligibility policies significantly increased public insurance while correspondingly reducing rates of uninsurance, suggesting that, in the absence of this policy, children eligible and enrolled in public coverage would encounter some spell without coverage, likely around the period of redetermination. As we note in a companion paper, these spells can result in significant access problems for children (Buchmueller, Orzol, and Shore-Sheppard 2010).

Over the past 15 years, states have been given an unprecedented amount of flexibility in how they design their public insurance programs. Our results suggest that some of the variation we see between states in children's uninsured rates may be due to the specific design features of individual states' public insurance programs. As we move forward with more children and adults becoming eligible for public coverage under the expansions mandated in the Patient Protection and Affordable Care Act (PPACA), states should give careful consideration to the design and administrative features of their insurance programs as they may affect states' success in the enrollment and retention of these individuals. Progress in addressing barriers to enrollment and retention and a better understanding of the program features that promote disincentives to enroll are essential to maximizing program effectiveness.

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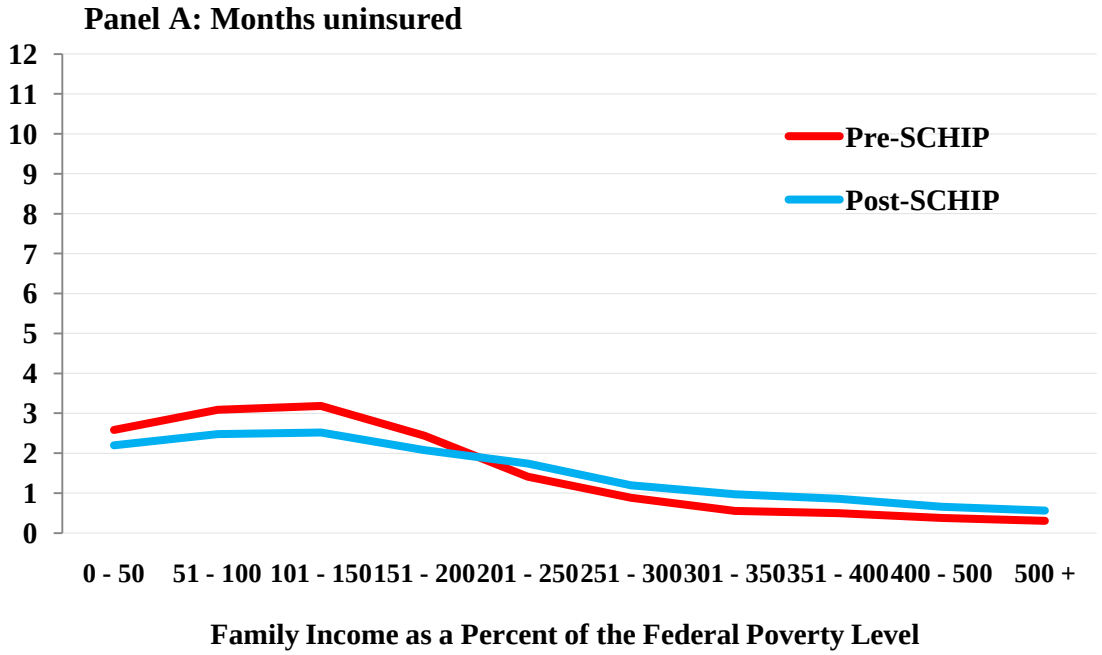
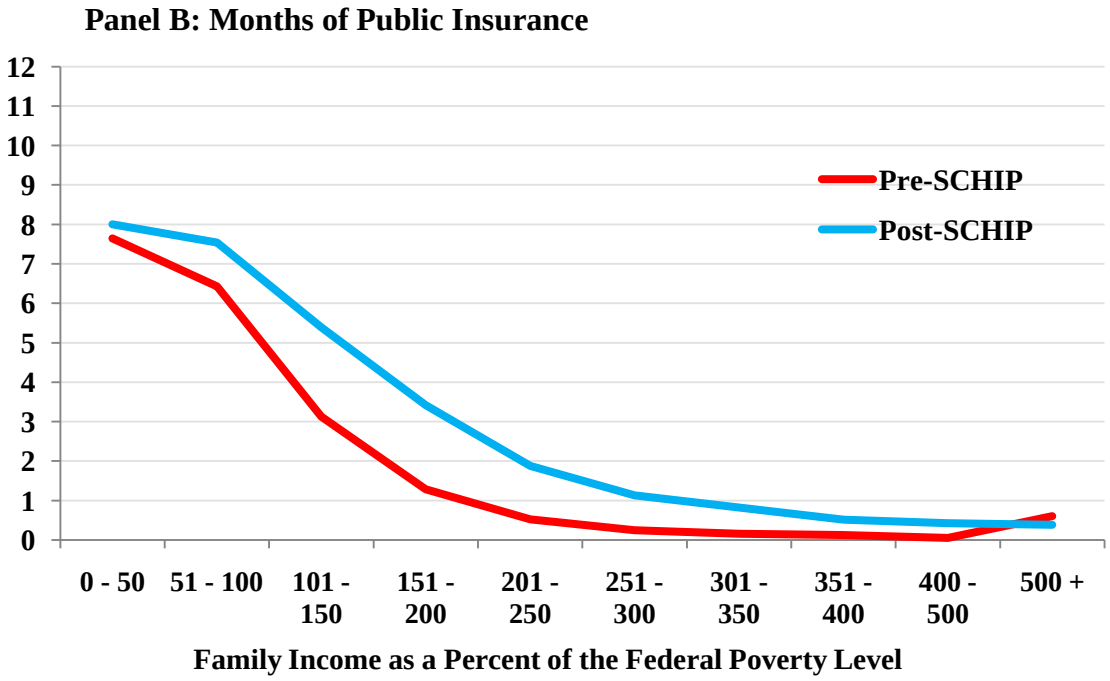
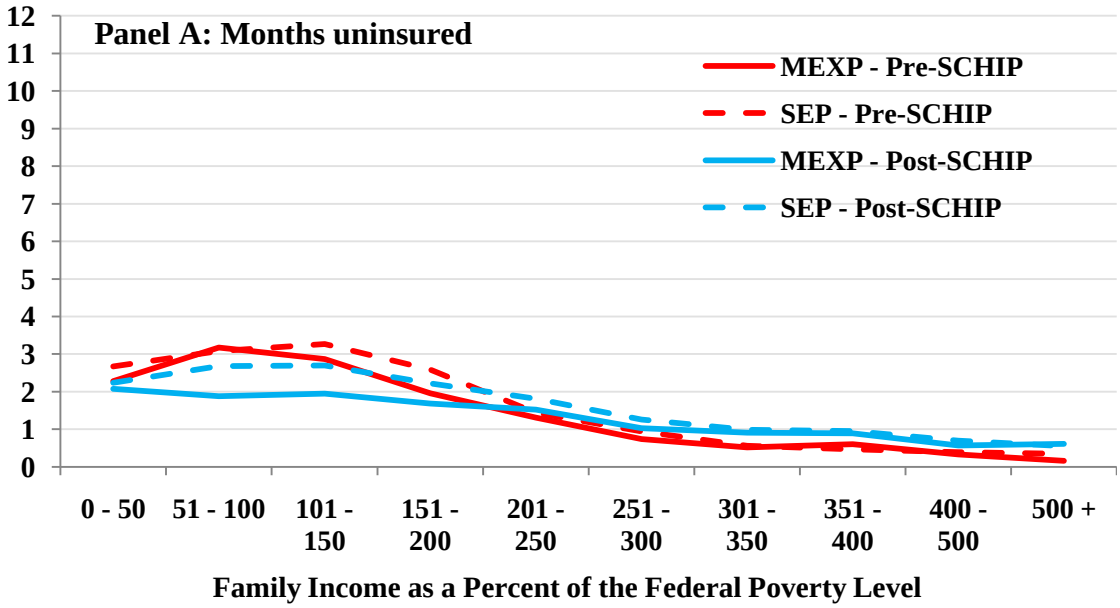
Figure 4.1. Insurance Coverage by Income and Time Period

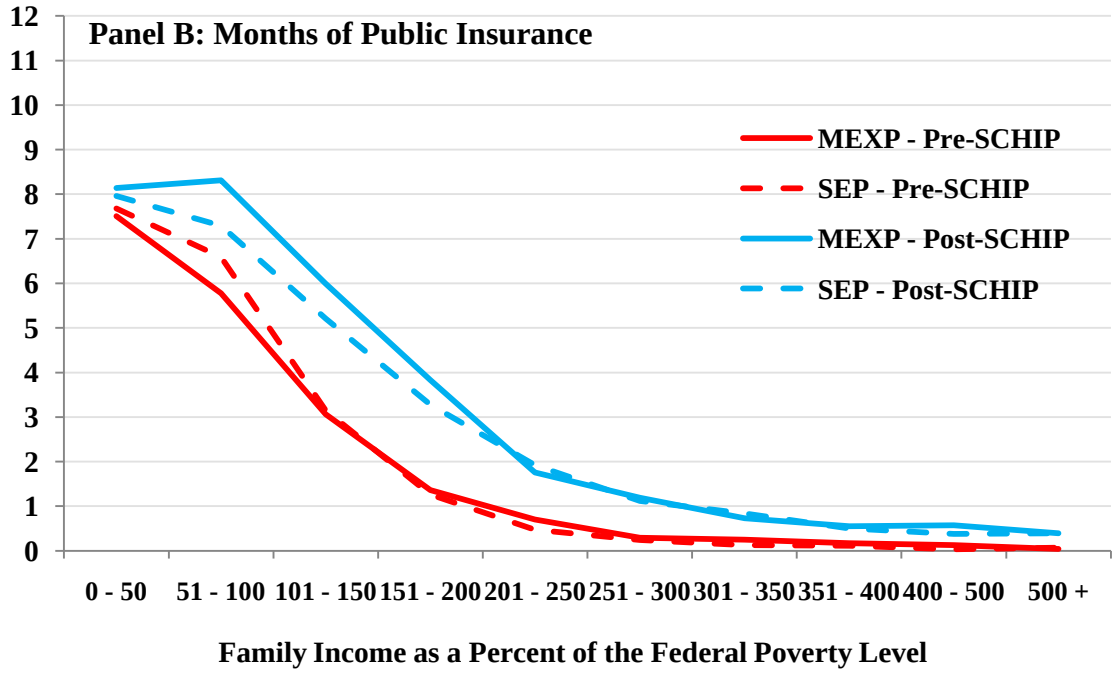




Source: Calculations from 1996, 2001, 2004 SIPP.

Figure 4.2. Insurance Coverage by Income, Time Period and Program Type





Source: Calculations from 1996, 2001, 2004 SIPP.

Table 4.1. Trends in Children's Public Health Insurance Design Features

	1997	2000	2003	2005
SCHIP program type: Medicaid expansion	NA	17	16	15
SCHIP program type: separate program	NA	29	30	31
12-month continuous eligibility in Medicaid	NA	13	12	15
12-month continuous eligibility in SCHIP	NA	20	18	21
Presumptive eligibility in Medicaid	NA	5	6	8
Presumptive eligibility in SCHIP	NA	3	5	6
Self-report income	1	6	12	10
Waiting Period (y/n)	NA	30	26	26

Policies in effect in July 1997, July 2000, July 2003 and December 2005.
Sample – 46 states

Table 4.2. Testing for Pre-SCHIP Differences in Insurance Coverage, by SCHIP program

	State Choice of SCHIP Program	
	Medicaid Expansion	**Separate Program**
Insurance Coverage Past Year—Months		
Months Uninsured	.*REF*	-0.34**
Months Public	.*REF*	0.66**
Months Private	.*REF*	-0.33**
Insurance coverage at some point in year —percentage points		
Uninsured	.*REF*	-1.9
Public Coverage	.*REF*	4.8**
Private Coverage	.*REF*	-2.8**

Sample: 9,249 children aged < 15 residing in households with average income indicates they are eligible for Medicaid based on the rules in place prior to passage of the BBA in 1997.

Notes: Covariates include indicator variables for age (16 categories), health status (5 categories), year indicators, maternal education (12 categories), gender, race, Hispanic ethnicity, family headed by a single mother, mother disabled and whether the maternal disability indicator is missing, whether health status was imputed, and average income as percent of poverty (categorical).

* $p \leq 0.10$; ** $p \leq 0.05$

Table 4.3. Regression Results: Spillover Effect of SCHIP Implementation and State Program Design on Children's Insurance Coverage

	(1)	(2)	(3)	(4)
Months Uninsured (Change in Months)				
Post-SCHIP	-0.24**	-0.18**		
Medicaid expansion			-0.35**	-0.31**, †
Separate program			-0.19**	-0.12**
12-month continuous eligibility		-0.21**		-0.23**
Presumptive eligibility		0.08		0.08
Self report income		-0.05		-0.03
Months Public Coverage (Change in Months)				
Post-SCHIP	1.04**	0.90**		
Medicaid expansion			1.34**, ††	1.22**, ††
Separate program			0.92**	0.77**
12-month continuous eligibility		0.30**		0.34**
Presumptive eligibility		0.26*		0.25*
Self report income		0.33**		0.29**
Months Private Coverage (Change in Months)				
Post-SCHIP	-0.54**	-0.50**		
Medicaid expansion			-0.71**, †	-0.68**, ††
Separate program			-0.47**	-0.42**
12-month continuous eligibility		-0.07		-0.09
Presumptive eligibility		-0.21*		-0.20*
Self report income		-0.06		-0.04
Uninsured at some point in year (percentage point change)				
Post-SCHIP	-5.0**	-4.3**		
Medicaid expansion			-6.3**	-5.8**
Separate program			-4.4**	-3.7**
12-month continuous eligibility		-2.0**		-2.2**
Presumptive eligibility		0.5		0.6
Self report income		-0.9		-0.7

Public coverage at some point in year (percentage point change)				
Post-SCHIP	6.0**	4.9**		
Medicaid expansion			7.7**, †	6.7**, †
Separate program			5.3**	4.2**
12-month continuous eligibility		2.5**		2.7**
Presumptive eligibility		1.4		1.3
Self report income		2.9**		2.7**
Private coverage at some point in year (percentage point change)				
Post-SCHIP	-5.3**	-5.2**		
Medicaid expansion			-6.8**	-6.8**, †
Separate program			-4.7**	-4.5**
12-month continuous eligibility		0.1		-0.1
Presumptive eligibility		-3.2**		-3.2**
Self report income		0.9		1.1
Public insurance full-year (percentage point change)				
Post-SCHIP	11.8**	10.7**		
Medicaid expansion			14.5**, ††	13.5**, ††
Separate program			10.7**	9.5**
12-month continuous eligibility		2.2*		2.5**
Presumptive eligibility		4.1**		4.0**
Self report income		2.0		1.6

Sample: 36,208 children aged < 15 residing in households with average income indicates they are eligible for Medicaid based on the rules in place prior to passage of the BBA in 1997.

Notes: Covariates include indicator variables for age (16 categories), health status (5 categories), maternal education (12 categories), gender, race, Hispanic ethnicity, family headed by a single mother, mother disabled and whether the maternal disability indicator is missing, whether health status was imputed, average income as percent of poverty (categorical), state of residence, yearly state unemployment rate, and yearly state child poverty rate.

* $p \leq 0.10$; ** $p \leq 0.05$
† indicates Medicaid expansion coefficient is statistically different from the Separate program coefficient at $p<0.10$; †† $p<0.05$

Table 4.4. Regression Results: Direct Effect of SCHIP Implementation and State Program Design on Children's Insurance Coverage

	(1)	(2)	(3)	(4)
Months Uninsured (Change in Months)				
Post-SCHIP	-0.34**	-0.16*		
Medicaid expansion			-0.48**	-0.39**, ††
Separate program			-0.28**	-0.03
12-month continuous eligibility		-0.38**		-0.44**
Presumptive eligibility		-0.03		-0.04
Self report income		-0.07		-0.03
Months Public Coverage (Change in Months)				
Post-SCHIP	1.67**	1.39**		
Medicaid expansion			1.66**	1.41**
Separate program			1.68**	1.38**
12-month continuous eligibility		0.20**		0.21**
Presumptive eligibility		1.10**		1.10**
Self report income		0.42**		0.42**
Months Private Coverage (Change in Months)				
Post-SCHIP	-0.96**	-0.92**		
Medicaid expansion			-0.82**	-0.70**, ††
Separate program			-1.01**	-1.05**
12-month continuous eligibility		0.25**		0.31**
Presumptive eligibility		-0.89**		-0.88**
Self report income		-0.29*		-0.32*
Uninsured at some point in year (percentage point change)				
Post-SCHIP	-1.8**	-0.4		
Medicaid expansion			-3.5**	-2.9**, ††
Separate program			-1.1	1.0
12-month continuous eligibility		-3.0 **		-3.7**
Presumptive eligibility		0.3		-0.4
Self report income		0.5		0.9

Public coverage at some point in year (percentage point change)				
Post-SCHIP	16.8**	14.9**		
Medicaid expansion			15.4**	13.3**, †
Separate program			17.4**	15.8**
12-month continuous eligibility		0.6		0.1
Presumptive eligibility		9.3**		9.2**
Self report income		4.8**		5.0**
Private coverage at some point in year (percentage point change)				
Post-SCHIP	-6.6**	-6.3**		
Medicaid expansion			-5.5**	-4.6**, †
Separate program			-7.0**	-7.2**
12-month continuous eligibility		2.1**		2.6**
Presumptive eligibility		-8.4**		-8.4**
Self report income		-0.7		1.0
Public insurance full-year (percentage point change)				
Post-SCHIP	11.4**	9.0**		
Medicaid expansion			11.8**	9.8**
Separate program			11.3**	8.6**
12-month continuous eligibility		2.0**		2.2**
Presumptive eligibility		9.5**		9.6**
Self report income		1.8		1.6

Sample: 30,806 children aged < 15 residing in households whose average income indicates they were made newly eligible for public coverage due under the SCHIP eligibility expansions.

Notes: Covariates include indicator variables for age (16 categories), health status (5 categories), maternal education (12 categories), gender, race, Hispanic ethnicity, family headed by a single mother, mother disabled and whether the maternal disability indicator is missing, whether health status was imputed, average income as percent of poverty (categorical), state of residence, yearly state unemployment rate, and yearly state child poverty rate.

* $p \leq 0.10$; ** $p \leq 0.05$
† indicates Medicaid expansion coefficient is statistically different from the Separate program coefficient at p<0.10; †† p<0.05

Table 4.5. Regression Results: Effect of Waiting Periods on Insurance Coverage

	Change in Months			Percentage point change		
	Mths Unins	**MthsPub**	**MthsPriv**	**Any Unins**	**Any Public**	**Any Private**
Full Newly Eligible Sample (n=30,806)						
Medicaid Expansion	-0.38**	1.40**	-0.70**	-2.9**	13.2**	4.6**
Separate Program						
No WP	-0.09	1.43**	-0.88**	-0.6	15.4**	5.5**
1-5mo.WP	-0.13	1.61**	-1.20**	1.4	19.4**	9.5**
6-+mo.WP	0.18	1.00**	-1.00**	2.3	11.0**	6.0**
MEXP & SEP prog States only (n=20,370)						
Medicaid Expansion	-0.38**	1.29**	-0.97**	-2.7*	11.4**	-4.8**
Separate Program						
No WP	0.19	1.34**	-1.04**	0.7	14.6**	-12.2**
1-5mo.WP	0.09	1.63**	-1.29**	4.9**	19.7**	-11.5**
6-+mo.WP	-0.06	1.34**	-0.66**	-0.7	14.5**	-7.0**

Sample: Children aged < 15 residing in households whose average income indicates they were made newly eligible for public coverage due under the SCHIP eligibility expansions.

Covariates include indicator variables for age (16 categories), health status (5 categories), maternal education (12 categories), gender, race, Hispanic ethnicity, family headed by a single mother, mother disabled and whether the maternal disability indicator is missing, whether health status was imputed, and average income as percent of poverty (categorical), state of residence, yearly state unemployment rate, yearly state child poverty rate, and dummy variables indicating whether the state had policies allowing for 12-month continuous eligibility, presumptive eligibility and self-reporting of income.

* $p \leq 0.10$; ** $p \leq 0.05$

CHAPTER 5

Conclusion

In this dissertation I have examined how certain factors influence a child's access to and utilization of health care. One theme that guided this work is the focus on how decisions about a child's access to health care are made in the context of the family and are therefore, subject to the constraints, the biases, and the beliefs of parents or other adult guardians, which are not necessarily time invariant.

In my first chapter, I explored the relationship between family size and a child's access to and use of health services, positing that size might influence a child's care through its impact on available resources. Obtaining credible estimates is made difficult by the fact that family size is an endogenous choice variable, and that unmeasured factors that determine parental choices about family size might also influence decisions to seek health care. For this reason, I supplement cross-sectional results with estimates from an instrumental variable strategy based on variation in family size, which is more plausibly exogenous. While other researchers have used this strategy to test for how family size impacts human capital investments in children, this essay is the first (to my knowledge) to use exogenous variation in family size to offer a causal estimate on the effect of family size on children's health care. My findings suggest that family size has a meaningful impact on children's care, although perhaps not always in the direction the conventional wisdom would suggest.

In my second chapter, I exploit panel data to derive estimates of both family size and a child's order of birth on their utilization of medical and dental care. I find that both matter importantly to a child's access to and use of services, but again, the direction of the relationship is not uniform across care. I hypothesize that parent's predisposition to seek care for their child is a function of their experiences with a child's older siblings. While the data require me to sacrifice some generalizability by restricting my sample to young children from small families, by using within-family information and focusing on age-specific utilization, the birth order estimates on physician and dental utilization are arguably the best evidence that a child's ordinal position in the family is a relevant factor in children's receipt of health care.

With my third chapter, I shift the focus to evaluating the impact of state policy decisions on children's health insurance, an important factor in facilitating access to health care. I examine whether differences across states in the adoption of administrative reforms to public insurance programs explain the considerable between-state variation we find in the proportion of low-income children without insurance coverage. These reforms were targeted at reducing the enrollment and retention barriers or program stigma carried over from the link between Medicaid and welfare. I find that decisions on program design can have a large impact on children's insurance coverage. Increases in public coverage among Medicaid eligible children was most pronounced in states that expanded existing Medicaid programs, relative to states that used separate programs. While we cannot precisely say why this is the case, it is possible that expanding Medicaid offers better integration for families of children eligible for public coverage whose income or family circumstance might require navigating children between two programs.

I also find evidence that state's efforts to simplify the enrollment process and the adoption of continuous eligibility policies have significant effects on low-income children's insurance coverage, lending support to the notion that the enrollment and retention process for public insurance may place undue burdens upon eligible families.