The Effects of Maternal Employment on the Health of School-Age Children*

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Abstract

Over the past several decades, an increasing number of women with children participated in the labor force. This has led researchers from a variety of disciplines to consider the impact of maternal employment on children, including the effects of maternal employment on children's health. The net effects are theoretically ambiguous given that maternal employment increases family income and access to health insurance but places additional burdens on a mother's time. Empirical identification is difficult because a mother's choice to participate in the labor market is endogenous. For example, a child's health may directly affect a mother's labor supply decision, or a mother's choice to work may be indicative of the mother's preferences and skills. In this paper, I implement an instrumental variables strategy using pooled data from the restricted version of the National Health Interview Survey (1985-2004). I identify the effects of maternal employment on overnight hospitalizations, emergency room visits, asthma episodes, and injuries and poisonings for children ages seven to seventeen. The conditional correlations between maternal employment and each of these four health events are zero or negative, suggesting that, if anything, having a mother that works is associated with a lower risk of a child having a bad health episode. I measure the causal effect of maternal employment on the health of children by using exogenous variation in each child's youngest sibling's eligibility for kindergarten as an instrument. I show that having a mother that works actually increases the probability a child will have a negative health episode. The results point to a consistent effect for all four outcomes and are statistically significant for overnight hospitalizations, injuries and poisonings, and asthma episodes. I provide evidence that this effect is not a reflection of a non-representative local average treatment effect and is robust to specification checks.

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1 Introduction

Over the past several decades, an increasing number of women with children participated in the labor force. According to a Bureau of Labor Statistics report (2006), in 1975 54.9 percent of women with children ages six to seventeen were in the civilian labor force. By 2001 that number had risen to 79.4, although it fell slightly to 76.9 in 2005. The economic impact of women's labor force participation cannot be completely characterized without understanding all of the costs and benefits involved. In particular, a woman's labor force participation might impact the health and well-being of her children. Not only does poor child health have contemporaneous economic consequences, such as health care expenditures and utilization, but poor health may also hinder a child's cognitive development (see, e.g., Blau and Grossberg, 1992). In addition, a growing amount of research finds that experiences during childhood can affect adult health,[1] adult economic and social well-being,[2] and even longevity,[3] so a woman's participation in the labor market might have long lasting effects on her children.

The direction and magnitude of the effect of maternal labor supply on child health is theoretically ambiguous. The clearest mechanism through which maternal employment might positively impact children is through an increase in family income. There is a well established income-health gradient, which has been shown to exist for children as well as adults (see Case, Lubotsky, and Paxson, 2002, and Currie and Lin, 2007). More income allows families to increase investments in health for their children, including better diet and better health care. In addition, some mothers acquire or improve their family's health insurance coverage due to their employment. However, maternal employment imposes a burden on a mother's time and may result in the poorer supervision or care of her children. A child's health is at least partially a function of time-intensive activities such as healthy meal preparation and house cleaning. A working mother may have less time to allocate to these types of activities. Bianchi (2000) shows that working mothers spend less time doing housework, and Crepinsek et al. (2004) document that children of working mothers have lower overall "Healthy Eating Index" scores. In addition, a child whose mother works may be left unsupervised or less-supervised more often than if the mother were at home full-time.

Previous studies on the effects of maternal employment find little measurable impact on child health, as discussed further in Section 2. Empirical identification of the effect is difficult because a mother's choice to participate in the labor market is endogenous. Maternal employment has often been considered as the effect of, not the cause of, the family's characteristics.[4] Mothers with

[1] Dietz, 1997.

[2] Case and Paxson, 2006 and Case, Fertig, and Paxson, 2005.

[3] Lleras-Muney, 2006.

[4] There is a substantial literature estimating the effect of child morbidity and disability on maternal employment. For example, Powers (2001 and 2003) argues that when a child is unhealthy, some mothers reduce their labor supply. Gould (2004) shows that a mother reduces her labor supply if her child has a time intensive disability but increases her labor supply if her child has a high-cost disability. Corman et al. (2004) find that having an unhealthy child

healthy children may find it easier to work, whereas mothers of children with special needs may find it difficult to work outside of the home. Alternatively, having a child with a chronic condition may make it necessary for a mother to work in order to provide health insurance or additional income for her family. Isolating the effect of a mother's labor force participation on the health and well-being of her children is confounded by this reverse relationship: a child's health may directly affect a mother's labor supply decision.

In addition, a mother's choice to work or not may indicate something about the mother's (unobserved) preferences and skills. If a mother's decision to work indicates something about her general ability level, motivation, inclinations, skill at caretaking, etc., then the sample of working mothers may not be a random sample of all mothers. This might lead to a spurious correlation between maternal labor supply and child health. This particular concern has prompted researchers to employ fixed effects strategies that can capture unobserved mother (and sometimes child-specific) characteristics (Ruhm, 2004). However, this methodology can only account for the unobserved characteristics that are constant over time. This may be problematic given the reverse relationship described above if children's health itself changes over time. In this study, I employ an instrumental variables strategy to isolate the causal effect of maternal employment, overcoming this limitation of fixed effects analysis.

In the absence of a perfect measure of underlying child health, I analyze the effects of maternal employment on four health outcomes: overnight hospitalizations, emergency room visits, asthma episodes, and injuries and poisonings. These outcomes capture both acute and chronic conditions. As argued in Section 3.3, while none of these outcomes alone are perfect, when taken together they provide a good proxy for child health. Together the effects of maternal employment on these four health outcomes, presented in Section 5, provide compelling evidence of a true effect on child health.

Consistent with much of the existing literature, I find that the conditional correlations between maternal employment and each of the child health episodes, as estimated using ordinary least squares regressions, are zero or negative. That is, having a working mother is associated with a lower risk of a child having the health incident. Because of the endogeneity of maternal employment, however, these correlations do not necessarily represent a causal relationship. In this paper, I use an instrumental variables strategy where the instrument relies on the fact that the opportunity cost of a woman working is substantially lowered when her youngest child becomes eligible for public school, potentially leading to an increase in maternal labor supply at that time. I measure the health of children ages seven through seventeen years old that have at least one younger sibling. I

reduces a mother's probability of working by around 8 percentage points. Duggan and Kearney (2007) investigate the effects of a child's enrollment in the federal Supplemental Security Income program (SSI) on his/her family and find little direct effect on maternal employment. Norberg (1998) looks at outcomes at birth to determine maternal employment in first year of life. She argues that it is not daycare that affects child health and development but that child health affects a mother's decision to work.

further restrict the estimation sample to children whose youngest sibling is within a specified age range around five years old. I use each child's youngest sibling's eligibility for kindergarten as an instrument for maternal labor supply in assessing the causal impact of maternal labor supply on the health of the older child. As discussed further below, Gelbach (2002) established that a child's eligibility for kindergarten, as measured by quarter of birth, increases maternal employment. I argue that a child's youngest sibling's eligibility for kindergarten provides variation in maternal employment that is plausibly exogenous to the older child's health. Nonetheless, in Sections 3 and 5 I provide discussions of the potential biases associated with this instrument. I also explore whether there is treatment effect heterogeneity across major demographic categories, and I discuss the generalizability of the estimated local average treatment effect measured by the instrumental variables strategy.

My estimates suggest that maternal employment *increases* the probability a child will have a negative health episode. The estimates are large and statistically significant when child health is measured by having had an overnight hospitalization, an injury or poisoning, or an asthma episode. My main results indicate that maternal employment increases overnight hospitalizations by 4 percentage points (baseline 2 percent), injuries/poisonings by 5 percentage points (baseline 3 percent), and asthma episodes by 12 percentage points (baseline 6 percent). Results for ER visits are not statistically significant, but point toward a similar qualitative conclusion. The effect sizes I find are large in percentage terms. Although the estimates are sometimes imprecise, the coefficients are consistent across different samples and for all four health measures. Decomposition by socioeconomic status, labor force attachment, and major demographic categories suggest that this is an effect that is homogeneous across various subpopulations. In total, the instrumental variables results suggest that, contrary to the basic OLS relationship, maternal employment has a large negative effect on the health of children.

The remaining sections of this paper are organized as follows. Section 2 reviews the relevant literature. Section 3 outlines the empirical specifications used and discusses issues related to the validity of the instrument. In Section 4 I describe the data and key variables. Section 5 discusses the empirical results and Section 6 concludes.

2 Related Literature

The literature on the effects of maternal employment on child outcomes has focused primarily on child development, perhaps due to the wider availability of objective measures such as academic performance. In particular, there has been substantial interest in estimating how maternal labor supply at early ages affects child development (e.g., Desai, Chase-Lansdale, and Michael, 1989, Blau and Grossberg, 1992, Ruhm, 2004, Kaestner and Corman, 1995, and Waldfogel, Han, and Brooks-Gunn, 2002). The findings are mixed, but generally the estimated effect of maternal employment is small. In one study specifically addressing health, Baker and Milligan (2007) use variation in maternity leave benefits in Canada to analyze the short-run effects of maternal non-employment on infant's health and development and find no significant effects. There is a related literature on how public assistance and low-wage maternal employment affect child outcomes, again usually focusing on younger populations (see, e.g., Moore and Driscoll, 1997, Cadena and Resch, 2006, and Bitler and Hoynes, 2006). Gordon, Kaestner, and Korenman (2007) use a fixed effects strategy to measure the effects of maternal employment (and child care) on child injuries and infectious disease for children ages 12 to 36 months. There is also a developing literature that finds maternal employment increases childhood obesity risk, though only for certain populations (Anderson, Butcher, and Levine, 2003).[5]

Most closely related to this paper, Ruhm (2004) uses the National Longitudinal Survey of Youth (NLSY) to analyze the effect of maternal employment on a cohort of children ages 10-11. He employs a fixed effects strategy to control for fixed family and mother characteristics. He includes a specification regressing a child's contemporaneous outcomes on maternal employment in the subsequent period, a relationship that cannot be interpreted as causal (although maternal employment is likely highly correlated between time periods). For his measure of obesity, he finds a positive and significant coefficient of maternal employment in the time period *after* height and weight were measured that is similar in magnitude to the main effect. He interprets this as calling into question the causality in portions of this and earlier work (cited above) using fixed effects strategies to measure the effect of maternal employment on childhood obesity.

Also closely related to this paper, Baker, Gruber, and Mulligan (2005) estimate the effect of maternal labor supply on young children's health by examining the impact of a local child care subsidy program in Quebec in the late 1990's. They use a difference-in-difference identification strategy and conclude that the policy led to an increase in maternal labor supply, an increase in formal child care enrollment, and a decline in health for children. This study considers the impact of the child care subsidy program on the child who is eligible and therefore cannot separate the direct effect of child care from the effect of maternal employment per se.

Though they measure the effect of child care quality, rather than maternal employment, Currie and Hotz (2004) suggest an important role for supervision in avoiding childhood accident and injury in young children. They find that the incidence of unintentional injury for children under age 5 is reduced in states with more stringent child care regulation. In related work, Aizer (2004) shows that after school supervision of adolescents (ages 10-14) has a large effect on their well-being as measured by criminal activity and behavior problems. Aizer uses a sample from the National Longitudinal Survey of Youth (NLSY) to estimate several fixed effects models using variation in supervision between and within families. If children whose mothers work spend more time unsupervised, then

[5] Fertig, Gloom, and Tchernis (2006) provide a thorough review of the literature and an analysis of the mechanisms by which maternal employment affects childhood obesity.

those children may have a higher risk of accident or injury (which may also lead to additional ER visits or hospitalizations).

Medical and epidemiological literatures have explored how demographic characteristics of children and their families contribute to disease incidence, severity, and management. Poverty has been established as a leading risk factor for many childhood ailments, as has being a racial or ethnic minority.[6] On the whole, relatively little attention has been paid outside of the social sciences to the potentially harmful - or beneficial - effects of maternal employment.

3 Empirical Specification and Methodology

3.1 Econometric Models

The key equation of interest is the effect of maternal labor supply on child health, which can be written as:

$$CHealth_i = \alpha + \beta MLS_i + \gamma X_i + \epsilon_i \tag{1}$$

Here $CHealth$ is the child health outcome of interest, MLS is maternal labor supply, and X is a vector of demographic characteristics of the child and his/her family. The unit of observation, i, is the child. In this model, β is the effect of maternal labor supply on child health. Because of omitted variables, the covariance of MLS and ϵ is not necessarily equal to zero, so the estimate of β may be inconsistent. One strategy for recovering a consistent estimate of β is to identify an instrumental variable Z, i.e. a variable that partially determines maternal labor supply but is uncorrelated with ϵ. With such an instrument Z, a two stage regression model can be estimated, with the first stage equation:

$$MLS_i = \alpha_{FS} + \beta_{FS} Z_i + \gamma_{FS} X_i + \mu_i \tag{2}$$

The consistency of the estimate of β relies on the validity of the instrument ($Cov(Z, \epsilon) = 0$). If Z is uncorrelated with ϵ, then the instrumental variable estimate of β is consistent. This is fundamentally an untestable assumption. Although I do consider below how violations of this assumption would affect my results, as long as the instrument, Z, is uncorrelated with ϵ, the model can be estimated by taking the predicted (fitted) value of MLS from Equation (2) and substituting it in for MLS in Equation (1) in a two-stage least squares model (2SLS). The instrumental variable estimate of β can also be thought of as resulting from the division of the "reduced form" estimate, β_{RF} below, by the first-stage coefficient derived above, β_{FS}. The reduced form equation is the regression of the child health outcome on the instrument:

$$CHealth_i = \alpha_{RF} + \beta_{RF} Z_i + \gamma_{RF} X_i + \sigma_i \tag{3}$$

[6] For examples on the etiology of asthma see Flores et al. (2005), Akimbami et al. (2003), and references therein.

The reduced form equation is interesting in its own right, as it indicates whether the instrument is correlated with the outcome of interest. The interpretation of the instrumental variable estimate, β_{IV}, as the causal effect is reliant on the assumption that the effect of the instrument on the outcome (β_{RF}) operates solely through the endogenous variable, in my case maternal employment. This is discussed further in Section 5.[7]

All four child health outcomes I present are dichotomous variables, taking a value of one if the child experienced the health episode and zero otherwise. Models with binary dependent variables require special consideration, since the two-stage least squares (2SLS) estimate described above assumes that the dependent variable in the second stage equation is continuous. As is well known, estimates from linear models with binary dependent variables may be a poor approximation when the dependent variable has a very low (or very high) mean (Bhattacharya et al., 2006). Angrist (1999) argues that, in most cases, the 2SLS estimate is a reasonable estimation strategy with limited dependent variables and a dichotomous endogenous variable. With some assumptions about the distribution of the error terms (i.e., that both are distributed bivariate normal), a bivariate probit model can be specified. Because of the strong functional form assumptions, the bivariate probit estimates are more precise. However, as Angrist (1999) argues, these estimates are potentially biased if the functional form assumptions are not correct. Estimating all specifications with probit and bivariate probit models lead to similar results. I include the non-linear version of the main regression results as Appendix Table C; non-linear results for all other tables are available upon request. The marginal effects from the bivariate probit model confirm the conclusions from the two-stage least squares estimates. Future work will explore the sensitivity using other limited dependent variable estimation strategies.

3.2 The Instrument: Youngest Sibling's Kindergarten Eligibility

My exogenous instrument is motivated by the observation that the opportunity cost of a woman's time is substantially lowered when her youngest child becomes eligible for public school. In the United States, kindergarten is provided free of charge through public schools for all children ages five or older. By 1983 (the first school year in my data) all states provided kindergarten, but individual states determine by what date a child must turn five years in order to be eligible to enroll in the current school year. The school year usually begins some time around the beginning of September. There is a fair amount of variation across states in this eligibility cut-off date and many states changed their policies over my study period. Appendix Table A demonstrates this variation for the first and last school year of my sample, 1983 versus 2004. Notice both that many states

[7] One can consider specifications using a binary instrument in a two-stage least squares model as a fuzzy regression discontinuity design (Imbens and Lemieux, 2007). Ideally, in a fuzzy regression discontinuity model, I would include a flexible polynomial trend in youngest child's age in month and identify only off of the break at 60 months (exactly 5 years). The data do not have sufficient power to identify the effect of the instrument and a polynomial. Extensive covariates are included to minimize potential bias associated with this limitation.

moved their cut-off date earlier during the 20 years of my sample, so children had to be somewhat older when entering school in the later periods, and that September 1st remains the modal cut-off date. Some states allow the local educational authorities (LEAs) to determine their own cut-off date, as indicated in the bottom row of Appendix Table A. I use state and year specific cut-off dates where available and assume a September 1st cut-off date for states with no standard date, however, results are not sensitive to including only states with a standard cut-off date.[8,9]

One key to the success of the instrumental variables strategy is identifying an instrument with sufficient predictive power. A child's eligibility for kindergarten has been found to predict maternal labor supply in several studies to date (Gelbach, 2002, and Cascio, 2006).[10] To confirm this relationship for my sample, in Figures 1A and 1B I illustrate the basic relationship between a mother's youngest child's age and her likelihood of employment. For these graphs, I use the full sample of mothers in the restricted NHIS, regardless of the number and ages of her children (or child), and each mother is represented only once in this sample. In these graphs, I consider only those mothers whose youngest child's exact eligibility could be determined, dropping observations from states where the local education authority determines the cut-off date (N = 89,317).

First, Figure 1A plots the fraction of mothers that were employed for each month of age, where their youngest child's age in months is calculated at the exact cut-off date faced by that child. The dots in Figure 1A represent the fraction of mothers that were employed and fractional polynomial interpolation was used to produce the smoothed curves on either side of 60 months. A clear increase in maternal employment occurs when the youngest child achieved 60 months (exactly 5 years) by the cut-off date. Figure 1B instead plots average maternal employment by the youngest child's age in months on September 1st of the most recent school year. Children in states with cut-off dates at the beginning of September, October, and December are included separately. So, for example, children who live in Kentucky face an October 1st cut-off date, so must have turned 59 months by September 1st to be eligible for kindergarten in the current school year. The curve with the break at 59 months includes only mothers who live in states with an October 1st cut-off date. These figures each provide suggestive evidence that kindergarten eligibility raises the probability a

[8]This research does not address the underlying mechanisms by which kindergarten enrollment affects maternal labor supply. We might expect that a mother faces a reduced opportunity cost of her time. But there may be more intangible reasons as well, such as a perceived reduction in the social stigma of working.

[9]In results not shown, I find that estimates using the youngest child's age in months at the interview as an alternative instrument are qualitatively similar. This alternative instrument relies on the observation that the probability a mother is employed increases approximately linearly in the youngest child's contemporaneous age.

[10]Gelbach (2002) argues that kindergarten provides a cost subsidy for child care, so the eligibility of a child for kindergarten will lower child care costs thereby lowering the cost of maternal work. As part of his analysis, he presents results demonstrating the positive effect of eligibility (as approximated by quarter of birth) on maternal labor supply. Cascio (2006) also measures the maternal labor supply response to publicly provided kindergarten, but instead uses variation in introduction of kindergarten in the 1960's and early 1970's. Cascio demonstrates a larger heterogeneity in the labor supply response to kindergarten eligibility between married and single mothers. She finds no significant labor supply response from married mothers, though Cascio's analysis uses a much earlier period of time than that considered here .

mother works, which is confirmed in estimates of β_{FS} presented in Section 5.[11]

As discussed above, the instrument in Equation (2), Z, must be uncorrelated with the error term in Equation (1), ϵ, in order for the estimate of β to be consistent. If the youngest child's kindergarten eligibility has a direct effect on health, then this assumption would be violated. In order to mitigate potential bias, I restrict the analysis to children with at least one younger sibling and whose own school eligibility status is not changing (ages seven and older). So, for example, I measure the health difference between two otherwise identical eight year old boys, one whose mother works because his youngest sibling is 5.5 years old and eligible for kindergarten and one whose mother does not work because his youngest sibling is 4.5 years old and was ineligible for kindergarten. Therefore validity relies on the assumption that the exact timing of these two births (4.5 years ago versus 5.5 years ago) was random, conditional on observable family characteristics such as race, maternal education, total number of children in the household, and the mother's current marital status.

All preferred specifications include the number of children present in the household and the mother's age as covariates, which should minimize the potential of a spurious correlation between maternal employment and child health through endogenously determined fertility. Furthermore, since all children ages seven and older must be enrolled in school by law, the direct effect on the older child of the youngest child being exposed to illness at school, for example, is likely very small. Another concern is that the instrument is correlated with unobserved maternal effort. If Z were positively correlated with unobserved maternal effort (for example, if the youngest child's eligibility for school reduced the time constraints on the mother, ceteris paribus), and if maternal effort is good for child health, then the instrument will be positively correlated with the error term. This would lead to a positive bias of β_{IV} in the instrumental variable regression, so it would appear that the effect of maternal employment is better for child health than it truly is. The IV specifications in this paper demonstrate a large *negative* effect of maternal employment on child health. To the extent that this latter type of bias is present, these should be considered underestimates.

In Section 5 I present results for a series of samples of children ages seven through seventeen who have at least one younger sibling. I restrict the samples to children whose youngest sibling is within a progressively smaller age band around five years old. Comparing these findings reflects the trade-off between the statistical power gained from expanding the sample and the precision and plausible validity of the instrument. In Section 5 I also explore threats to instrument validity and other potential sources of bias further, which all confirm the robustness of my main findings.

[11] By using kindergarten eligibility as an instrument directly for maternal labor supply, I am implicitly making the assumption that eligibility for kindergarten leads to kindergarten enrollment for (at least part of) the sample. In the data that I use for this analysis, the National Health Interview Survey, this link cannot be tested directly because kindergarten enrollment is not observed. Elder and Lubotsky (2006) utilize state variation in kindergarten eligibility laws to instrument for a child's age at school entry and provide compelling evidence that kindergarten eligibility does lead to kindergarten enrollment.

3.3 Health Measures

To explore the relationship between maternal employment and child health, I use the restricted access version of the National Health Interview Survey (NHIS), pooling observations from survey years 1985-2004. In the NHIS, maternal employment is measured contemporaneously, so I limit my analysis in this paper to health outcomes that can be plausibly influenced by present conditions in the family. There is no perfect measure of child health, especially since the NHIS questionnaire relies on reports of child health from a family respondent rather than a medical professional.[12] Because true underlying health cannot be measured, I instead use four proxies for health which capture both chronic and acute conditions: overnight hospitalizations, emergency room visits, asthma episodes, and injuries and poisonings. Each of these measures is likely reflecting a health event that is unambiguously bad, unlike, for example, having had a doctor's visit. Visiting the doctor could indicate adequate access to care and healthy, preventative behaviors. Although each health outcome has its own strengths and weaknesses, my analysis over all four health outcomes provides strong evidence that maternal employment does affect child health.

The first health outcome I consider is whether the child was hospitalized overnight at least once in the past 12 months. In the year 2000, over 6 million children were hospitalized overnight. Leading causes of hospitalizations include acute health incidents, such as injuries and poisonings, and chronic diseases, such as mental disorders, asthma, and diabetes.[13] Many of these conditions may be sensitive to supervision, regular access to care, and access to appropriate medication and preventative treatment.[14] Hospitalizations reflect the most severe health events, recall of having had a hospitalization is likely unbiased, and admission to a hospital requires the objective judgment of a medical professional. However, there is some evidence that utilization of hospitals is affected by an individual's health insurance status (Currie and Gruber, 1996, and Kemper, 1988) or characteristics of hospital or region (see, e.g., Goodman et al., 1994), indicating that having had a hospitalization may reflect access to care in addition to true differences in morbidity. I provide evidence suggesting that this is not a major source of bias in my estimates. The second health outcome I consider is whether the child had an emergency room visit in the past 12 months, a more common event than overnight hospitalizations. The leading causes of ER visits are similar to those for hospitalizations, but often reflect more acute health events. However, having had an ER visit may reflect both

[12]In the NHIS one family member answers questions for the entire family. Children seventeen and under are not eligible to be family respondents, so all data on children is gathered from a proxy respondent, usually the child's mother.

[13]Estimates are provided by the Agency for Healthcare Research and Quality's HCUPnet and are nationally representative for children 0-17 years. Data is collected from the HCUP Kids' Inpatient Database, 2000 and can be accessed at http://hcupnet.ahrq.gov.

[14]The AHRQ HCUP Fact Book No. 5, Kruzikas et al (2000), presents a Prevention Quality Indicator (PQI) for a number of childhood diseases and finds that 179 (ages 5-9), 113 (ages 10-14), and 70 (ages 15-17) children per 100,000 population in 2000 were admitted for pediatric asthma, a condition classified as preventable. Other preventable diseases that the AHRQ Fact book discusses are short-term diabetes complications, pediatric gastroenteritis, and urinary tract infection.

inadequate access to care in a doctor's office and true emergencies.[15]

The next health outcome I analyze is whether the child had an asthma episode in the past 12 months. Asthma is a leading cause of hospitalizations, emergency department care, and doctor's office visits. According to the American Lung Association, asthma is the most common chronic disorder in childhood, affecting 6.2 million children under age 18.[16] Asthma rates are consistently high across individuals from all levels of socio-economic status, however some researchers have found that children from low income families and racial minorities are at a higher risk (McDaniel et al., 2006, and Smith et al., 2005). Differential underreporting and underdiagnosis are of particular concern when analyzing the effect of maternal employment on asthma, though I am able to control for an extensive set of covariates.[17] There are several mechanisms through which maternal employment may affect a child's risk of having an asthma episode. Asthma is an atopic condition and can usually be controlled through medication. Flores et al. (2005) find that the majority of preventable hospitalizations for asthma were due to parent or patient causes, predominately medication related (non-adherence, ran out, etc).[18] If an employed mother is less able to adequately monitor adherence to medication or is not able to respond promptly to an asthma attack, then maternal employment should increase a child's risk of having an overnight hospitalization, an emergency room visit, or an asthma episode. However, regular access to care and the ability to purchase appropriate medication to control asthma may reduce the incidence of having an asthma episode. In addition, one widely recognized risk factor for asthma is indoor allergens (see, for example, Lanphear et al., 2001). Bianchi et al. (2002) find that mothers who work spend significantly less time on housecleaning. Maternal employment could lead to more residential exposure to allergens for children and hence more asthma episodes.

The final health outcome I consider is whether the child had an injury or poisoning episode in the past three months. This measure is less likely to be confounded by utilization and access to care, since the measure I consider does not require that the child received medical attention. It is less objective, however, since it is up to the respondent to determine what constitutes an injury. An employed mother may be less aware of injuries, so underreporting could lead to a spurious negative correlation between *reporting* an injury or poisoning and maternal employment. However, in my main results I find that maternal employment is associated with a large *increase* in injuries and

[15] Future work will explore incorporating information on the causes of ER visits and of overnight hospitalizations.

[16] The Asthma and Children Fact Sheet can be accessed at:
http://www.lungusa.org/site/pp.asp?c=dvLUK9O0E&b=44352.

[17] Akinbami et al. (2003) provide evidence that measurement of asthma is sensitive to question wording. Yeatts et al. (2003) find high rates of undiagnosed asthma and that underdiagnosis was correlated with characteristics such as gender, socioeconomic status, and race/ethnicity.

[18] Flores et al. provide estimates on the fraction of hospitalizations for asthma which were preventable, based on assessments by primary care physicians (PCP), inpatient attending physicians (IAP), and parents. IAP's responded that 43.3 percent (87 cases) of the 230 children's hospitalizations for asthma were preventable; while PCP's reported 37.7 percent (63 cases) were preventable. Of these, the estimated percent due to parent or patient related causes were 66.7 percent for IAP's and 82.5 percent for PCP's, with the leading cause in both cases to be medication related (non-adherence, ran out, etc).

poisonings, so this particular sort of bias should only cause an underestimate of the negative effect of maternal employment on child health using this measure.

4 Data Description

To conduct this analysis, information on a child's health, the mother's labor supply, and the ages of the child's siblings are all needed. The National Health Interview Survey (NHIS), conducted by the Centers for Disease Control and Prevention's National Center for Health Statistics (NCHS), satisfies the extensive data requirements of this project. The NHIS is a repeated cross-section survey which has been conducted annually in the United States since 1957. The restricted-access version of the NHIS includes state of residence identifiers, which allow for the more precise measurement of whether the youngest child was eligible for kindergarten.[19] I combine data from survey years 1985 - 2004.[20] A major survey instrument redesign occurred in 1997, so some variables are only defined in the "post redesign" sample. I define the analysis variables as consistently as possible across years and especially between survey designs, however, an indicator for whether the observation was drawn from "pre-" versus "post-" survey redesign is included in all relevant regressions. See Appendix Table B for a description of how the key variables are defined across survey periods.[21]

Because of the NHIS survey design, each of the four health outcomes I explore is defined for a different, nested sample of children. Figure 2 provides a diagram illustrating the relationship between these samples. The first sample, "All Children," consists of all children from survey years 1985-2004 ages seven through seventeen years who were part of the primary family and whose mother was between eighteen and sixty-four years old. Children whose mother could not be identified within the household or who had missing values for any key variable are excluded, yielding 274,842 children in the pooled sample, as indicated in Figure 2 Sample 1. For the key results in this paper the sample is further restricted to children that have at least one younger sibling. I restrict attention to children with at least one younger sibling to ensure that a child's own eligibility for schooling will not confound the analysis. I further restrict the sample to those children ages seven through seventeen years old whose youngest sibling is within a certain age range around five years. Within Sample 1 there are 88,887 children whose youngest sibling was between 24 and 107 months (2 - 8 years), 66,160 children whose youngest sibling was between 36 and 95

[19]In addition to birth month and year, which are available in all survey years, the restricted version of the 1997-2004 data includes the *day* of birth, allowing an even more refined measurement of kindergarten eligibility. Also, in survey years 1985-1996 month of birth was imputed to August in approximately two percent of the sample. The restricted version of the data contains an imputation flag (the public use version only contains this flag in 1996), to allow these imputed values to be identified. I eliminate any children whose youngest sibling's birth month was imputed from the main estimation sample.

[20]The National Health Interview Survey uses a stratified sampling design. Primary sampling units are drawn every ten years, so my data span two sample design periods: 1985-1994 and 1995-2004.

[21]Note that survey weights are utilized for all mean calculations. Because of the complicated sample construction, all weights are normalized to sum to one for each survey year.

months (3 - 7 years), and 41,583 children whose youngest sibling was between 48 and 83 months (4 - 6 years) at the scheduled interview date.

As discussed above, my sample is comprised of a “pre” and “post” redesign period. In the post-redesign period, 1997-2004, respondents are asked whether the child had an injury or poisoning in the past 3 months.[22] I denote Sample 2 in Figure 2 as the “Post Children,” indicating that these are all children ages seven to seventeen in survey years 1997 - 2004. Sample 3 in Figure 2 is also a subset of Sample 1, referred to as the “Sample Children.” In the pre-redesign survey years, 1985-1996, families were randomly assigned one out of six condition lists. The respondent was asked whether each family member had the conditions or episodes on their assigned list. For my third outcome measure, asthma, I include children from families that were asked whether each child had asthma in the past 12 months (condition list 6). In 1997 the survey was redesigned and, rather than ask about one list of conditions for every family member, the respondent was asked detailed health information about one randomly selected “sample child” from the family. A Sample Child Supplement is provided for approximately one child in every family and asks whether the sample child had an asthma episode in the past 12 months.[23] Sample 3 in Figure 2 indicates that the “Sample Children” are the subset of the full sample that were asked the asthma question, N = 76,362.

Finally, Sample 4 in Figure 2 denotes the children in the post-redesign period who were given the Sample Child Supplement described above, N = 44,838. Information on emergency room visits in the past 12 months was only formally collected in the Sample Child Supplement in the post-1997 survey redesign period.[24] I will present results for each child health outcome on this sample, so that the effects can be compared for a consistent sample. However, there are obviously many fewer observations in Sample 4, which will limit the power to make inferences.

Since the main results are necessarily specified on a sample of families with two or more children within specified age ranges, one may be concerned that the results are not readily generalizable. Within the four different samples described above, I next consider how similar the children in my main estimation samples are to all children having information about the outcome of interest. In Table 1, I compare the demographic characteristics of these groups. The column numbers of Table 1 correspond to the sample numbers in Figure 2.

Table 1 Column (1a) represents all children in the NHIS from the pooled 1985-2004 surveys,

[22] Data on injuries were collected in the pre-redesign surveys, but only if medical attention was sought. This is qualitatively very different, since this measure would again confound access to care with true morbidity. In addition, the reference time period within which the injury must have occurred was two weeks in length, so there are many fewer incidents reported in the pre-redesign period.

[23] The Sample Child Supplement contains more detailed data on asthma, including whether the child was ever diagnosed with asthma by a doctor. The variable I chose to use is most similar to the pre-redesign data and, I believe, most closely reflects the child’s current health.

[24] In the pre-redesign period a doctor’s visit record does report whether the child saw a doctor in the emergency room. However, these records are only for the past two weeks, so are not directly comparable to the 12 month measure.

ages seven through seventeen years old. Note that mothers with more than one child aged seven to seventeen years will be represented more than once in the sample. In the regression results to follow, all standard errors are clustered by state of residence to account for potential correlations in the error terms introduced by the NHIS sample design and the state-level nature of my instrument, and from including siblings in the regressions. Nearly 70 percent of children had mothers who worked. The average age for mothers is 38 years old, and almost 80 percent of the mothers in the sample are currently married (this includes mothers who are remarried). The average age for children is 12 years and there are slightly more boys than girls in the sample. Table 1 Column (1b) restricts the sample to children with at least one younger sibling whose youngest sibling was between ages 2-8 years (24 - 107 months) at the scheduled interview date. Further restricting the sample around age 5 does not change things qualitatively, so these samples are omitted for presentational clarity. Notice that the full and restricted samples are very similar. Because of the mechanical relationship between having a sibling and family size, the number of children in the family is larger when I restrict to the sample of children with at least one younger sibling. The mothers are over 2 years younger on average and slightly less likely to be white in Column (1b) relative to Column (1a), presumably because fertility rates are lower among whites.

Moving across the columns in Table 1, I show that the characteristics of the samples for each health outcome are very similar. Comparing between the column panels, we see that, when restricting the sample to children where each health outcome is reported, the samples remain representative, as expected from the NHIS sample design. Similar to the findings in Columns (1a) and (1b), we see in the remaining columns that children with at least one younger sibling are more likely, on average, to have mothers who are married, less educated, and younger. They are more likely to be minorities (especially Hispanic). When each sample is restricted to children with at least one younger sibling, the fraction of mothers employed drops. As is explored in more detail below, mothers with more children are less attached to the labor market. Therefore the sample of children with at least one younger sibling is not representative of the full sample along some dimensions. This should be kept in mind when considering the generalizability of the key findings to the full population of children.

Table 2 presents sample means disaggregated by the incidence of each health episode. Column (1a) includes all children in Sample 1 that did *not* have a hospital episode in the past 12 months, approximately 98 percent of the full sample. Column (1b) presents means for the children in Sample 1 that did have a hospital episode, approximately 2.4 percent or 6,576 children. By comparing these means, we see that children who were hospitalized are more likely to have mothers who are not married and who are less educated. The mothers of the children who were hospitalized are also slightly less likely to work. This foreshadows a negative relationship in the OLS estimates between a mother working and her child having a hospitalization.

Columns (2a) and (2b) of Table 2 provide a parallel disaggregation, where Column (2a) includes children that did not have an injury or poisoning in the past 3 months and Column (2b) includes children that did have an injury or poisoning. Most notably, children that had an injury or poisoning were more likely to have a mother that worked than those that did not, which is the opposite of the pattern found with the three other health outcomes. The remaining two sets of columns are analogously constructed. Table 3 Column (3) compares the sample that did and did not have an asthma episode, while Column (4) compares those children that did and did not have an ER visit. While hospitalizations and ER visits are relatively more common for children whose mothers had less than a college degree, injuries and poisonings and asthma episodes are more likely among children with higher educated mothers. Boys are more likely than girls to have had an injury or poisoning, asthma episode, or ER visit, and girls are more likely to have been hospitalized overnight. Notice also that older children are slightly more likely to have a hospital, injury or poisoning, or ER episode, so all preferred specifications will include the child's age as a control.

The bottom panel of Table 2 restricts the sample to the Post Sample Redesign Children, to allow for a more direct comparison between the health outcomes. Near the bottom of Table 2 I include a measure of whether the child has a regular place of care that is not an emergency department. Children that had an asthma episode were *more* likely to have a regular doctor, raising some suspicion of underdiagnosis of asthma among the poor (which is not surprising given the previously discussed evidence). Looking across the columns in the bottom panel of Table 2, there is clear positive correlation between the measures, though they are not perfectly correlated. For example, children who were hospitalized or had an ER visit were nearly three times as likely to have had an asthma episode. Similarly, children that had an asthma episode were over twice as likely to have had a hospital episode (5 percent) or an ER visit (37 percent). Children that had an injury or poisoning in the past 3 months were twice as likely to have been hospitalized in the past 12 months than those that did not; however, only four percent were hospitalized overnight. The means in the bottom panel indicate that these measures are reflecting some underlying morbidity, each with varying levels of severity and incidence.

5 Empirical Results

5.1 OLS Estimates

Comparison of the means in Table 2 suggested that, unconditionally, maternal employment is associated with a slight decrease in the incidence of hospitalization, asthma, and ER visits, and a slight increase in injuries or poisonings. In Table 3 I explore how this relationship changes once demographic characteristics and other controls are included, before presenting my main IV results.

The cells of Table 3 report the coefficient on maternal employment from separate ordinary least squares regressions.[25] Note that since each episode is considered a *negative* health outcome, a negative coefficient on maternal employment implies working benefits child health. Each column in Table 3 represents a different sample. The sample in Table 3 Column (1) contains all children ages seven to seventeen (Figure 2, Sample 1) and Column (2) includes sample children seven through seventeen in the post-redesign survey years (Figure 2, Sample 4). Columns (3)-(7) contain analogous samples for the other three health outcomes, as shown in Figure 2. The specifications on the post-redesign sample (Figure 2, Sample 4) allow for the inclusion of more extensive control variables and allow for a better comparison across outcome measures. All regressions include year fixed effects to control for differences in question wording.

The rows of Table 3 successively add covariates to explore the sensitivity of estimates of the relationship between maternal employment and child health. Row (1) of Table 3 presents the basic relationship between maternal employment and each health episode, with an indicator for "pre-" or "post-" redesign year. In Column (1) Row (1), the coefficient implies that maternal employment lowers the probability that a child had an overnight hospitalization by .2 percentage points, a statistically significant effect. In Columns (3) through (7), the probability a child had each health event is not statistically significantly related to maternal employment. Row (2) adds interview quarter,[26] state, and year fixed effects. Row (3a) adds dummy variables for the child's age and an indicator for the child's sex. Row (3b) adds an indicator for child having had low birth weight, which is only available in the post 1997 redesign Sample Child surveys.[27] In Row (4) family characteristics are added to the specification: the mother's marital status (married or not married), the number of children (1, 2, 3, 4, and 5 or more), and dummy variables for the age spread between the oldest and youngest child present in the family. Finally, Row (5) adds dummies for mother's age (18-24, 25-29, 30-34, 35-39, 40-64), mother's education (less than high school, high school, some college, or BA/Professional Degree), mother's race/ethnicity (black, white, Hispanic, other). In the preferred (most saturated) model, reported in Row (5) of Table 3, the estimated relationship between maternal employment and the child health episode are negative and significant for hospitalizations, asthma, and ER visits (injuries/poisonings is only sometimes significant), implying that maternal employment *reduces* the probability that a child has a negative health outcome.

Income and health insurance are two mechanisms through which maternal employment may plausibly impact child health. As such, including these variables as controls will not allow for the full effect of maternal employment to be measured. However, it is interesting to consider whether

[25] Because the outcome variables are dichotomous, this can also be referred to as the linear probability model. Marginal effects estimated from probit models are very similar and are available upon request.

[26] I include interview quarter dummies to address the concern that the effect of employment varies by interview quarter, since many third quarter interviews (July - September) are conducted when school is not in session. In results not shown, when the interaction between maternal employment and quarter three is included in this regression, the coefficient is small and not significant.

[27] The child's birth weight is classified as "low" if it is below 2,500 grams (approximately 5 pounds, 8 ounces).

the positive effect of maternal employment disappears when these covariates are included in the regressions. Unfortunately, family income is measured poorly in the NHIS, so the fact that the estimates of the positive effect are only slightly diminished when family income is included in Row (6) may simply be because true income is not being properly measured.[28] In the bottom panel, Row (6) adds income dummies to the specification in Row (5) and their is little change in the coefficients.

Maternal health is another source of potential bias, as is illustrated in Table 2. The health measure is a decreasing scale from 1 to 5, where 1 indicates a self-report of excellent health and 5 indicates poor health (i.e., a higher number for average health implies worse health). Notice that, looking across the columns of Table 2, the sample of children that had each health episode had mothers with worse health on average. A mother in poor health may not work due to her health problems and may have children in poorer health (or may be more inclined to report her children being in poorer health), inducing a spurious positive correlation between maternal employment and good child health. However, it may also be the case that being employed affects a mother's health and through this channel also affects the child's health. If this were the case, including maternal health as a covariate would "over control" and would not allow for the full measurement of the effects of employment on health. Adding to the specification in Row (5), Table 3 Row (7) includes dummy variables for each maternal health level. Including maternal health controls substantially reduces the size of the coefficients in absolute value and renders the relationship statistically insignificant in each column except Column (1).

Table 3 demonstrates that the conditional correlations between maternal employment and the four health episodes are negative or zero once child and maternal demographic characteristics are included as covariates. This implies that, if anything, maternal employment is good for child health. As discussed above, there are a number of non-causal explanations for this apparent effect.

5.2 Causal Estimates

Table 4 presents the main results of this paper. Each coefficient represents the results from separate regressions, so a total of 60 regressions are summarized in this Table. Each regression includes maternal, family, and child demographic characteristics and state, year, and quarter fixed effects that parallel the specification in Table 3 Row (5) (with standard errors clustered by state).[29] The

[28] Family income is defined differently before and after the 1997-survey redesign. The early survey years 1985-1996, income is summarized in nine categories with a tenth category for "unknown." In the post-redesign surveys, income is grouped into 10 salary categories, 2 overlapping salary subgroups, and 3 missing data categories. These income values are not adjusted over time for inflation and do not reflect any differences in family size (as does the poverty ratio categorizations, for example). Rather than interpolate income directly from these or more disaggregated income measures, I include dummy variables for each salary category for each survey year. Many studies using these data choose to impute family income. For example, Case, Lubotsky, and Paxson, 2002, use the Current Population Survey to impute family income.

[29] The estimates in Tables 4-8 do not employ survey weights. Estimates using survey weights are very similar.

sample is restricted to children ages seven through seventeen years old who have at least one younger sibling and whose youngest sibling was within the age range specified by row. Panel A, the first set of rows, presents the effect of maternal employment on the probability of the child having had an overnight hospitalization. The first three rows report estimates for all children (Figure 2, Sample 1), while the fourth row provides estimates for the post-redesign sample children (Figure 2, Sample 4). I include results for the post-redesign sample children to allow for comparisons between health measures within a consistent sample.

Table 4 Column (1) reports the coefficient from the OLS regression of maternal employment on child health, corresponding to Equation (1) in Section 3. These estimates are directly comparable to Row (5) of Table 3. These estimates differ slightly because of sample construction; in Table 4 I restrict the sample to children that have at least one younger sibling whose youngest sibling is within a specified age range around 5 years old. Because of the smaller sample size in this table relative to Table 3, the estimates are less precise. For example, in Table 3 Column (1) Row (5), the all children sample, the OLS estimate is -.0032 (.0007). This estimate can be compared to Table 4 Column (1) Row (1), the all children "with youngest sibling 2-8" sample, OLS estimate of -.0018 (.0011). The validity of the instrument relies on restricting the sample in this way, though at the cost of a substantial reduction in sample size and a resulting loss of power. Note again that a negative coefficient on maternal employment implies that a child whose mother worked has a *lower* risk of having had a bad health episode.

The coefficient of interest in Equation (2), as described in Section 3, is β_{FS}, the effect of the instrument on maternal employment. These "first stage" estimates are presented in Column (2) of Table 4. The effect of kindergarten eligibility is large and significant for all regressions, suggesting that the instrument has predictive power. Column (3) of Table 4 reports the coefficient from the "reduced form" regression, where the coefficient of interest is the effect of the instrument on the health outcome. The reduced form coefficients are consistently positive, but are only statistically significant for hospitalizations, asthma, and injuries or poisonings and only in the largest samples. For example, in Table 4 Column (3) Row (1) the estimated effect is .0033 (.0011), indicating that the youngest child's eligibility for kindergarten raises the risk of the older child having been hospitalized by .33 percentage points. In all rows the estimates point toward a similar finding: the kindergarten eligibility of the child's youngest sibling increases the risk the child has a bad health episode. These results are particularly important when interpreting the overall findings.

If the instrument does not completely satisfy the validity assumption (i.e., the instrument may be correlated with the error term in Equation (1)), the reduced form results still give a direct measure of the correlation between the youngest child's eligibility for kindergarten and negative health consequences for the older child. I argue that the predominant mechanism through which kindergarten eligibility should affect elder sibling health is through the mother's labor supply, but this interpretation is not testable, at least not in the current data. As an alternative, it is possible that maternal effort toward the older child increases with the youngest child's kindergarten eligibility, thus leading to better health for the older child. If this effect dominated, I would find a *negative* coefficient on the instrument in the reduced form, indicating that the instrument was good for child health. On the other hand, it might be the case that eligibility affects the level of supervision of the child. For example, a mother that works could cease to purchase formal child care for her children when her youngest child ages into kindergarten and instead rely on her older children to supervise her younger children after school. In this example, we might expect to see an increased probability of bad health events for the older child when the younger child ages into kindergarten eligibility. The change in health in this example is still theoretically an effect of maternal employment, but it does confound the interpretation of the instrumental variable estimate. I explore this possibility further in Sections 5.3 and 5.4.

Table 4, Column (4) presents the instrumental variable estimates using a two-stage least squares model (2SLS). For computational and expositional simplicity, I include only the 2SLS estimates in this table.[30] As expected from the positive and significant coefficients in the reduced form and first stage models, the instrumental variable coefficients are positive in all specifications. Panel A of Table 4 presents the effects of maternal employment on children's overnight hospitalizations. In Column (4) the IV effects are large and statistically significant in all rows. The estimate in Row (1) indicates that a mother working increases the probability of overnight hospitalization by approximately 4 percentage points, or just under 200 percent. When the sample is further restricted in Rows (2) and (3) the estimate is much less precise, but is still statistically significant. Using the Post Sample children in Panel A, Row (4) indicates a similar effect size, although the coefficient is no longer statistically significant. Overall, the results in Panel A suggest a reasonably robust relationship where maternal employment increases a child's probability of having an overnight hospitalization, contrary to the OLS relationship. The remaining tables explore the robustness of this effect, breaking down the sample in Row (1).

Turning now to the second health outcome, Panel B of Table 4 presents analogous specifications for the effects of maternal employment on injuries and poisonings. The estimate on the largest sample: the Post Sample children whose youngest sibling was between 2 - 8 years, in Panel B, Row (1) implies that maternal employment increases injuries and poisonings by 5.1 percentage points. This represents just under a 200 percent increase from the baseline 2.6 percent probability. The remaining rows in Panel B do not have sufficient sample size to estimate a statistically significant

[30] Appendix Table C replicates this table using entirely non-linear models. The first three columns report marginal effects from probit models, which all very closely match the linear estimates in Table 4. The fourth column presents estimated marginal effects from a bivariate probit model. As described in Section 3, this model relies on strong functional form assumptions. Notice that the estimates are smaller in magnitude and much more precise, but the qualitative results are consistent. Future work will further explore the robustness of the results to estimation using these and other limited dependent variable models.

effect, but the point estimates are similar.

The probability of having had an asthma episode, Panel C in Table 4, again demonstrates a positive effect. In Row (1), Column (4), the coefficient implies that maternal employment causes an 12 percentage point increase in the probability of having an asthma episode. Again, this corresponds to just under a 200 percent increase. The effect is statistically significant in the three largest sample, but the magnitudes become very large and the estimates are imprecise. Finally, Panel D in Table 4 explores the effect of maternal employment on ER visits. The IV estimates are not statistically significant for ER visits, but the results are qualitatively similar to those from the other health outcomes. The "Post Sample," that used in Row (4) in Panels A, B, and C, and in all of Panel D, does not have sufficient sample size to produce statistically significant estimates. However, the results are similar in magnitude and always large and positive.

Table 4 provides evidence that maternal employment negatively affects children's health. The point estimates are large in magnitude, indicating that, in the largest estimation sample, maternal employment raises the probability of overnight hospitalization, injury or poisoning, and asthma by just under 200 percent each. These are large effects. For example, having had an asthma episode raises the probability of having had a hospitalization by roughly 3.3 percentage points, compared with the estimated effect of a 3.9 percentage points increase due to maternal employment. I explore the robustness of these estimates in the subsequent sections.

5.3 Heterogeneous Effects and the Local Average Treatment Effect

Up to this point, I have assumed that the effect of maternal employment on child health is identical for all children. However, the effects of maternal employment on child health may vary with characteristics of the mother and her family. In this section, I estimate the effect for subsets of the population, to determine whether it is qualitatively different for different groups. This is of particular relevance in an instrumental variables context, since the IV strategy measures the effect only for the population of women whose labor supply is influenced by the instrument. This is generally referred to as the local average treatment effect (LATE) (see Angrist and Imbens, 1994 and Angrist, Imbens, and Rubin, 1996). For example, Angrist and Imbens (1994) document how instrumental variables estimates measure the effect of "treatment" on the population whose treatment status is affected by the instrument. They refer to this group as the "compliers." In my context, the instrumental variable estimate is the effect of maternal employment on child health for the population of mothers whose labor supply is affected by their youngest child's eligibility for kindergarten. The population of compliers is never actually observed, so one might be concerned that this population may be different from the full population of mothers in important ways. In particular, the OLS and IV estimates could differ solely because OLS is estimating an average effect of maternal employment on child health while IV estimates the effect for the compliers. In other words, it could be that maternal employment is good on average for child health but particularly bad for a very specific population. To address this concern as much as possible, I first estimate the extent of treatment effect heterogeneity by estimating 2SLS equations on subsets of the population.

Because hospitalizations are defined for the largest sample, and therefore have sufficient observations to break down the sample along various dimensions, in all subsequent tables I focus on the effect of maternal employment on overnight hospitalizations for children ages seven through seventeen whose youngest sibling was between 24 and 107 months (2-8 years) at the scheduled interview date. In the first row of Table 5, I reproduce the results from the first row of Panel A in Table 4, for reference. In the subsequent rows, I disaggregate this sample based on demographic characteristics of the mother. Note that I provide the means of both child hospitalization and maternal employment for each sample.

The second set of rows in Table 5 shows the results for non-Hispanic black, non-Hispanic white, and Hispanic mothers. Column (1) reports the OLS estimates of the relationship between maternal employment and child hospitalizations. The OLS estimate for blacks is much larger in magnitude than for whites (-.0045 versus -.0015), but the coefficients on both are statistically insignificant. The first stage estimates, Column (2) of Table 5, suggest that white mothers are more likely to begin working after their youngest child ages into kindergarten eligibility (.0951) than black mothers (.0507), although the baseline probability of working is 68 percent for blacks compared to 63 percent for whites. Next we notice that the reduced form estimates for blacks are larger than for whites (.0073 for blacks versus .0039 for whites), although the difference is not statistically significant. Column (4) presents the instrumental variable results, indicating that maternal employment causes a 14.5 percentage point increase in the risk of overnight hospitalizations for the children of black women. This estimate is very large in magnitude, but is imprecise. For white mothers, it is estimated that employment increases hospitalizations by 4.1 percentage points. Both IV estimates are statistically significant and indicate that maternal employment increases child hospitalizations for black and white mothers. The estimates for Hispanic mothers are not statistically significant, although employment may be more poorly measured for these women.

Another dimension along which there might be heterogeneous effects is maternal education level. A woman's education level can be thought of as a reasonable proxy for socioeconomic status of the family. As stated earlier, some literature suggests that the consequences of maternal employment are more severe for more affluent mothers (e.g., Anderson, Butcher, and Levine, 2003). The next set of rows disaggregates the sample by two levels of maternal education: 12 years of schooling or less (high school degree or less) and more than 12 years of schooling (some college and BA or Professional Degree). The difference in the IV effect for these two groups of women is small and not statistically significant. The effect of maternal employment for mothers with a high school degree or less is 4.3 percentage points compared with an effect size of 2.9 percentage points for mothers with at least some college education. I therefore find no evidence consistent with heterogeneous effects by maternal education level.

The third panel of Table 5 presents the effects decomposed by marital status. The "not married" sample consists of any woman not currently married, whether widowed, divorced, or never married, and also includes women who are separated from their husbands. The probability of having an overnight hospitalization is over 25 percent higher for the not-married mothers sample (.026 versus .019) and that sample shows a much stronger relationship between maternal employment and child health in the OLS specification (-.0094 versus -.0002) in Column (1). Note that the probability of working is very similar in these two samples. The instrumental variable estimate in Column (4) suggests that maternal employment has a larger effect on overnight hospitalizations for not-married mothers (.0952) as compared to married mothers (.0292), although the coefficient for not-married mothers is not statistically significant.

The next set of rows explores the treatment effect heterogeneity by the age of the mother. In all the empirical results presented in this paper, I exclude from the sample mothers younger than 18 or older than 64. I do this for two reasons. First, matching children to mothers is complicated in the data and occasionally children are miscoded as spouses (and vice versa) in the raw data. Restricting the sample to women 18 to 64 eliminates many instances of siblings or grandmothers being miscoded as mothers. In addition, women outside of the 18 to 64 age range are more likely to be in school or to be retired, complicating the interpretation of employment. In the next set of rows in Table 5 I further disaggregate the sample by maternal age. In doing this, I am also able to consider whether endogenous fertility is spuriously affecting the instrument, since women above 40 are less likely to have any more children. Women younger than 40 may remain not-employed because they are pregnant or are trying to become pregnant, so these women are not an appropriate comparison group for employed women of the same age and number of children. As mentioned above, the number of children is a strong predictor of employment and may be correlated with child health, so I include controls for mother's age and number of children in all specifications to mitigate any potential biases. Table 5 shows that the effect of maternal employment is only somewhat heterogeneous across categories of maternal age. The only age groups with statistically significant effects are mothers who are 25 - 29 and 30 - 34, where the 2SLS coefficients are .1284 (.0692) and .0476 (.0228), respectively.[31] The estimates for the other age groups are smaller and less precise. Therefore the different estimated effects and the lack of statistical significance for some maternal age groups appears to be an artifact of a limited sample size and not of heterogeneous effects along this dimension.

To explore how access to care may be confounding the results, I compare the effects of maternal employment for children with public versus private health insurance. Health insurance is defined

[31] Note that there were too few mothers ages 18-24 (N = 716) to estimate the effects separately on this sample.

most consistently for survey years 1998-2004, therefore in the bottom set of rows I focus on children from these survey years. The estimated effect of maternal employment on hospitalizations for this sample is slightly larger and less precise than that for the full sample, indicating that maternal employment increases hospitalizations by 5 percentage points in these survey years. The subsequent three rows estimate the effects for populations of children with differing health insurance types. First I present results for children with any health insurance. Then I break this sample into two groups: those with private health insurance and those with public health insurance.[32] As discussed in the Introduction, children with public health insurance are more likely than those without health insurance and with private health insurance to receive treatment in a hospital setting (see, e.g., Currie and Gruber, 1996). In my data, the mean rate of overnight hospitalizations is almost twice as large for children with public health insurance versus private health insurance (2.8 versus 1.5 percent). This could be due to a higher disease burden in this population or due to characteristics of reimbursement that lead families to seek care in hospitals. Notice also that maternal employment rates are lowest for children with public health insurance and highest for children with private health insurance. To measure the full effect of maternal employment, we would like to allow health insurance to be a mechanism through which maternal employment affects child health. Estimating the different effects of maternal employment by the child's health insurance types allows the exploration of the effects of maternal employment on child health without confounding the positive aspects of an increase in access to care, although there may be selection into health insurance types based upon child health status.

Although the IV estimates for children with public health insurance are imprecise (.0465, SE .0646), the effects of maternal employment among children with any health insurance and with private health insurance are large and statistically significant (.0587 and .0667, respectively). While health insurance may play some mitigating role, the effects of maternal employment on hospitalization are consistently positive and do not simply reflect an increase in access to care. The sample of children with no health insurance is too small to enable reliable estimates of the effect of maternal employment on child health.

In all, the estimates in Table 5 indicate that there may be some heterogeneity in the treatment effects along major demographic categories. However, the effects of maternal employment are consistently measured as being bad for children's health. This exercise in exploring treatment effect heterogeneity does not eliminate the possibility that the women whose labor supply is affected by the instrument (the compliers) are actually subsets of women within each category presented in Table 5. Though the consistency of the estimates across different portions of the population suggest that there is not a great deal of treatment effect heterogeneity, it may still be the case that the

[32] I approximate having public health insurance by taking the sample of children that report having some kind of health insurance and removing those that do not have private health insurance.

coefficient in the instrumental variable estimate is measuring the effect of maternal employment for a very specific, and potentially non-representative, sample of women. To explore this further, I disaggregate the sample in an alternative way, which may be a better approximation to subsets more or less affected by the instrument.

I construct an index of labor force attachment (LFA) and break down the sample by this index. This analysis is similar to that of Kling (2001), who uses the family background index in Card (1995) in order to determine how the instrumental variables estimate of the return to schooling differs across quartiles of family background. Because LFA varies in important ways by race (for example, black women have much higher employment rates and Hispanic women have much lower employment rates, as compared to whites), I restrict the sample to white mothers for this exercise. I calculate a labor force attachment index from the full sample of mothers with at least one child between the ages of zero through seventeen from the NHIS pooled survey (years 1985-2004). I calculate the probability the mother works from a linear probability model on year, state, and quarter dummies, and maternal age, education level, marital status, and number of children, as defined above. I use the coefficients from this regression to predict the labor force attachment for all white mothers. I then divide the regression sample (i.e., children ages seven through seventeen years old with at least one younger sibling, whose youngest sibling was between ages 2 - 8 at the scheduled interview date) into four quartiles based on their LFA score. This procedure yields over 13,000 children in each quartile.

Table 6 presents sample means for each Labor Force Attachment (LFA) quartile. Notice that the women most highly attached to the labor market have fewer children on average, have more education, are less likely to be married, and are older on average. ER visits occur most frequently for the lowest quartile while injury or poisoning episodes are reported most frequently for those in the highest quartile. To confirm that the LFA scale is a good approximation for actual probability of employment, I compare the probability of maternal employment across the four quartiles. Less than half of the mothers in the lowest quartile worked (48.5 percent), as compared to 76 percent of mothers in the highest quartile, indicating that LFA is reflecting a true difference in likelihood of employment. The probability of working is one way to measure labor force attachment, another way is through the intensity of work or hours worked. However, hours of work are only measured in the post-redesign survey years, 1997-2004. Table 6 demonstrates the relationship between work intensity and my LFA index. The first work-hours measure includes hours of work for women that reported working last week. We see only a slightly higher average number of hours worked for the most attached women. When I impute zero hours of work for women who reported not working last week, there is a strong gradient in hours worked for the four LFA quartiles.

In Table 7 I present results disaggregated by LFA. The first row of Table 7 repeats the results from the third row of Table 5, the estimated effect on hospitalizations for the sample of children with white mothers. The second set of rows in Table 7 splits the sample by mothers above and below the median LFA. Notice that while the overall probabilities of having a hospitalization are very similar (around 2 percent), the mean of maternal employment is over 16 percentage points higher for children whose mother is in the top half of the LFA scale (71 versus 55 percent). The first stage estimates in Column (2) indicate that the instrument does affect labor supply in both groups, though the effect is larger for the less attached mothers. The reduced form in Column (3) is only significant for less attached sample (.5 percentage points), indicating a large increase in hospitalizations due to the instrument. Likewise, the instrumental variable estimates of the effect of maternal employment on child hospitalizations (Column 4) are positive, large, and statistically significant for the less attached sample only. Although the effect for the most attached women is not statistically significant, the standard error is large and the difference in the coefficients for the more and less attached women are not statistically significant. Indeed, when the sample is broken down further into the four quartiles, the effects of maternal employment are statistically insignificant for all four quartiles, but are always positive in sign.

The estimates in Table 7 inform one concern about the magnitudes of the coefficients. The youngest child's eligibility for kindergarten might affect the older child's health for women that do not change their employment status. For example, women may respond to the instrument by increasing their work intensity. Or, women may change the child care arrangements for all of their children once their youngest child becomes eligible for school. The first stage does not take into account the population of women whose work intensity changes. In the second scenario, where the child care arrangements change for all children, the instrument is correlated with the error term in equation (1), where supervision or child care is an omitted variable correlated with kindergarten eligibility of the youngest child. Though the instrumental variable estimate is not measuring the correct effect, in both scenarios the change in hospitalizations is still a consequence of maternal employment. Future work will explore the sensitivity of the results to using measures of work intensity and to outliers. Unfortunately the NHIS does not include data on child care or supervision. However, failure to find differences along the labor force attachment scale provides some evidence that this is not a major source of bias in these results.

5.4 Robustness Checks

The final table, Table 8, explores the robustness of the main findings to different sample selection criterion. The first row of Table 8 repeats the main results (Table 4, Row 1) for reference. Again, these estimates are for all children ages seven to seventeen in survey years 1985-2004 that have at least one younger sibling whose youngest sibling was between 24 and 107 months at the scheduled interview date. The second and third rows of Table 8 explore the possibility that the youngest child's exact age is correlated with the older child's health in a way that is biasing the IV estimates. For example, one might be concerned that the spacing of births is influenced by the health of the older child. Park et al. (2003) look at the extreme case of severe child disability and mothers' tubal sterilization and find that having a severely disabled child only increases the probability of seeking sterilization for mothers who already have one non-disabled child. This evidence suggests that the change in timing of births due to having a disabled child may vary with birth order, but it is likely not a strong effect. I first eliminate all children from the sample that are reported as having an activity limiting disability. This restriction removes children that were recently injured or are still recovering from a debilitating disease, for example, so it should understate the negative consequences of maternal employment. With this restriction, the estimated effect of maternal employment declines, but still implies a large and statistically significant effect (.0304, SE .0123). Further restricting the sample to children whose *siblings* are also not limited has only a minor additional effect on the coefficients and is still statistically significant (.0288, SE .0117). These results suggest that birth spacing is not driving the results.

The next set of rows divides the sample by total family size. I present results for children with exactly one sibling and for children with two or more siblings (i.e., mothers with 2 children versus mothers with 3 or more children). First notice that the hospitalization rate is similar between these samples but mothers are much less likely to work if they have three or more children. The first stage coefficient for mothers with exactly two children is statistically significant but small, implying that fewer mothers are changing their employment status when their youngest child becomes eligible for kindergarten. Because the average employment rate of these mothers is higher, it seems likely that mothers with exactly two children return to work before their youngest child becomes eligible for kindergarten. The IV estimates are large, though less precise and not statistically significant (.1437, SE .0938) for children with exactly one sibling. When I examine families with three or more children, I find a positive and statistically significant effect of maternal employment of 3.1 percentage points.

Row 6 in Table 8 explores the sensitivity of the results to the mother's own health. Employment may harm a mother's health (due to physical strain, stress, etc), which could be a mechanism through which maternal employment affects child health. Therefore, I have so far not controlled for maternal health. However, it may also be the case that the effect of maternal employment is different for mothers with different health statuses and that mothers respond to the instrument differentially by health status. For example, mothers with severe health conditions may remain out of the labor force, even when their youngest child is in school. Restricting the sample to children with mothers who are reported as having very good or excellent health does not change the qualitative results, but the estimated effect is now somewhat larger (.0525).[33]

[33] Future work will explore the effect of a woman's employment on her own (and her husband's) health using a similar estimation strategy. These results, while interesting in their own right, will also inform this potential mechanism through which maternal employment could be affecting children's health.

Finally, because I look at the health of school age children ages seven to seventeen but allow the sample of youngest siblings to be between two and eight years, I test specifications restricting the sample to children ages nine to seventeen. Though the sample size is reduced, the results on this sample are qualitatively similar and statistically significant. These results indicate that maternal employment increases child hospitalizations by 4.6 percentage points. The final row of Table 8 further restricts to children between nine and twelve. It may be that maternal employment is more (or less) harmful for children nine to twelve as compared to teenagers. Also, in the data girls above age thirteen have an increased risk of hospitalizations, presumably due to childbearing. Although I only include in my sample children that report being the child of the household head or the spouse of the household head, the pathways from maternal employment to child health may be different for girls at risk of pregnancy. Note that I include the child's age and sex in all specifications, so a higher risk of hospitalizations should not bias the results. In the bottom row of Table 8 we find that effects of maternal employment are even stronger for the sample of children ages nine to twelve (.0500).

In all, I find evidence that maternal employment leads to an increase in child hospitalizations, injuries and poisonings, and asthma. The results are qualitatively similar for ER visits, but always statistically insignificant. I find little evidence of treatment effect heterogeneity by the mother's race, marital status, and age. Specifications on all subsets of the population indicate a similar effect: maternal employment increases child hospitalizations. To further explore whether treatment effect heterogeneity is present, I construct an index of labor force attachment. I find little heterogeneity in the effects of working along this dimension, although the results are larger for the less attached women. The main results are robust to estimating the effects for families with no activity-limited children, for families of different sizes, for healthy mothers only, and for older children within smaller age ranges.

6 Conclusion

Maternal employment could affect children's health through a variety of mechanisms. Positive channels include income, health insurance, and the mother's self-esteem. Alternatively, employment may hinder a mother from supervising or otherwise contributing to time-intensive, health promoting activities. The basic correlations between maternal employment and the measures of acute child health events are small, (almost always) negative, and generally insignificant, even after controlling for many other determinants of child health. These results might be interpreted to reflect that maternal employment has no effect on, or even benefits, children's health.

However, there are theoretical reasons to believe estimates of the basic relationship between maternal employment and child health are not causal. A mother's decision to work could reflect underlying (and unobserved) ability, skills, or preferences, so that a mother that works may be different in important ways from a mother that does not work. Or, a mother whose child is chronically ill may choose to remain home to care for her child, inducing a positive correlation between working and good health through a reverse relationship.

To estimate the causal effect of maternal employment on child health, I use an instrumental variables estimation strategy. I analyze the health of children ages seven to seventeen with at least one younger sibling, and I use the child's youngest sibling's eligibility for kindergarten as an exogenous instrument for maternal employment. The instrumental variable estimates suggest that, once the endogeneity of labor supply is accounted for, maternal employment raises the probability of having an adverse health event. The main results indicate that maternal employment increases overnight hospitalizations by 4 percentage points, injuries and poisonings by 5 percentage points, and asthma episodes by 12 percentage points, each by around 200 percent. The estimates for ER visits are smaller in percent terms and are always statistically insignificant, perhaps due to the smaller estimation sample.

The main results are robust to a host of specification checks. Although the estimates are not statistically significant in all cases, the signs and magnitudes are consistent. I find that maternal employment is an important determinant of child health. My results suggest that studying only the conditional correlation between maternal employment and child health will lead to incorrect conclusions. I estimate that maternal employment raises a child's risk of hospitalizations, injuries and poisonings, and asthma, and may also increase the risk of having an ER visit.

A Appendix

Rather than simply studying the treatment effect heterogeneity across LFA quartiles, here I present estimates of the weights that each quartile receives in the local average treatment effect estimate. Because the instrumental variable estimate reflects the effect for women whose labor supply is influenced by the instrument, it may be the case that women in one portion of the LFA scale receive disproportional weight. This analysis modifies methods discussed in Kling (2001) for more precisely identifying the "local" population that dominates the local average treatment effect. Kling decomposes the key results in Card (1995), a classic paper estimating the returns to schooling using proximity to a college as an instrument. Kling uses the family background index constructed in Card's paper and calculates precise weights to estimate how much of the IV estimate is due to each quartile of the family background distribution. The weight given to each quartile is a function of the sample size, the variance of the instrument conditional on the quartile and covariates, and the change in the probability of the endogenous variable due to the instrument, as described below. Kling finds that 53 percent of the IV estimate is due to the bottom quartile of the family background distribution. Though this population is of particular policy interest, the fact that the IV estimate provides a local average treatment effect which consists primarily of the effect of schooling for a subset of the population that may see the most benefit from schooling implies that the local average treatment effect is not generalizable to the full population.

In Appendix Table D I provide a calculation of weights in my data, similar to those in Kling (2001). For this entire exercise I restrict my attention to the regression sample for overnight hospitalizations (Sample 1 in Figure 2) where the child has at least one younger sibling and whose youngest sibling was between 2 - 8 years at the scheduled interview date. I further restrict the sample to white mothers, since the characteristics associated with labor force attachment vary by race/ethnicity. The labor force attachment index (LFA) is discussed in the main text. Column (1) of Appendix Table D gives the fraction of the sample in each quartile, $w_q = P(Q)$, where Q is the quartile. Column (2) gives the conditional variance of the instrument $\lambda_{1|x} = E[P(Z|X,Q)(1-P(Z|X,Q))|Q]$, where Z is the instrument, X are the covariates, and Q is the quartile. The impact of the instrument on the probability the mothers work is given by $\Delta MW_{q|x} = E[E(MW|Z=1,X,Q) - E(MW|Z=0,X,Q)|Q]$. Column (4) shows the overall weight of each quartile in the two-stage least squares regression, $\omega_{q|x} = \frac{(w_q \lambda_{q|x} \Delta MW_{q|x})}{\sum_q (w_q \lambda_{q|x} \Delta MW_{q|x})}$.

In Column (4) of Appendix Table D, we see that the most weight is given to women in the lowest quartile of LFA, though the weights are very similar. Most importantly, the highest quartile receives the least amount of weight, 18 percent versus 30 percent. This table provides evidence that the group of "compliers" is not disproportionally from one particular quartile.

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Figure 1: The Fraction of Mothers Working by Youngest Child's Age in Months

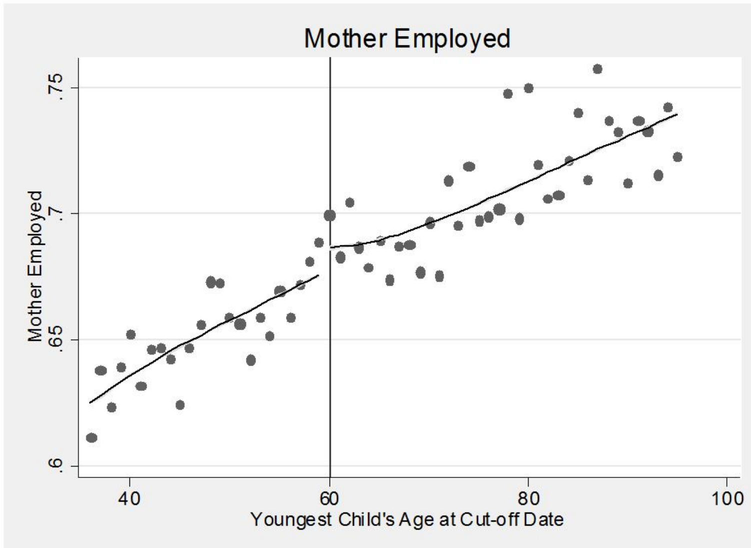

Figure 1A: Maternal employment before and after the youngest child is eligible for kindergarten at the exact eligibility cut-off date. Dots represent average maternal employment for each age by months. Lines are from a fractional polynomial smoother. Each mother/youngest-child observation is only included once and observations are weighted by the youngest-child's sample weight. No restrictions are placed on the number or ages of siblings, N = 89,317.

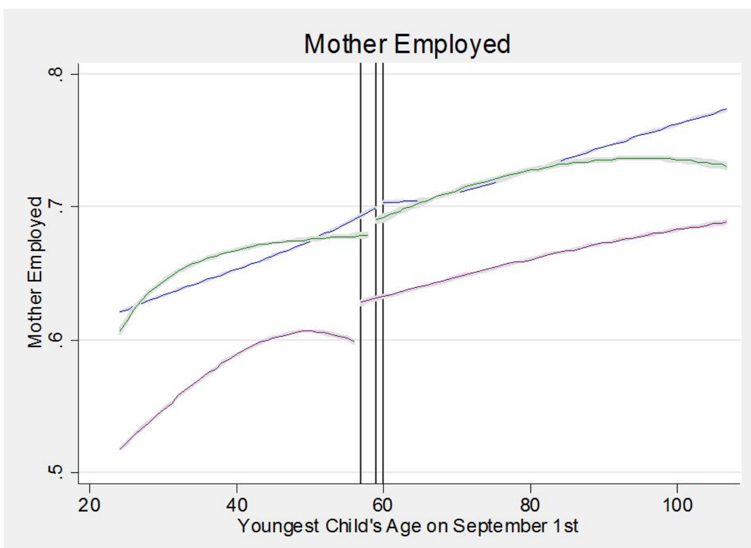

Figure 1B: Maternal employment before and after the youngest child is eligible for kindergarten at 60 months, with the youngest childs age measured on September 1st of the most recent school year. Three cut-off displayed here: September Cut-off = 60 months (N = 36,485), October Cut-off = 59 months (N = 12,818), and December Cut-off = 57 months (N = 23,647). Each mother/youngest-child observation is only included once and observations are weighted by the youngest-child's sample weight. No restrictions are placed on the number or ages of siblings.

Figure 2: Estimation Samples for Each Health Outcome

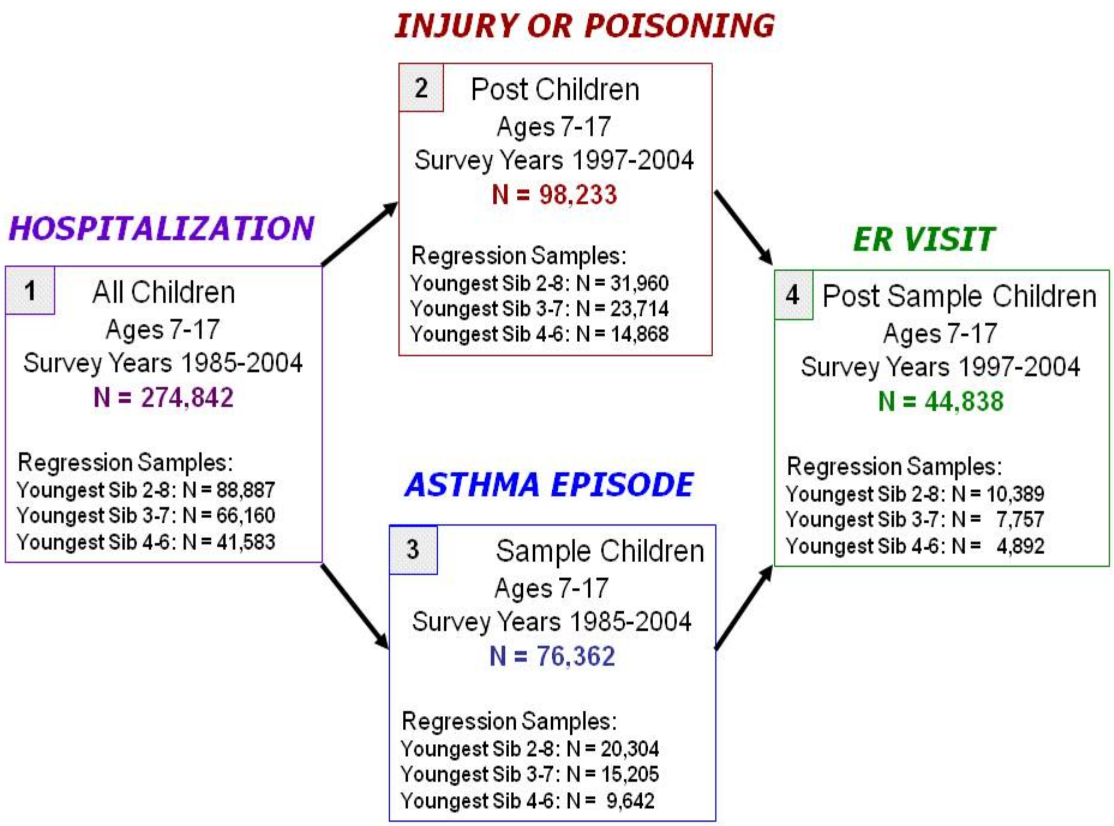

Table 1: Means by Sample

	Health Outcome: Hospital Episode		Health Outcome: Injury or Poisoning		Health Outcome: Asthma Episode		Health Outcome: ER Visit	
	All Child (1a)	Has Sib 2-8 Yrs (1b)	Post Child (2a)	Has Sib 2-8 Yrs (2b)	Sample Child (3a)	Has Sib 2-8 Yrs (3b)	Post Sample (4a)	Has Sib 2-8 Yrs (4b)
Number of Obs	274,842	88,887	98,233	31,960	76,362	20,304	44,838	10,389
Had Episode	.023	.020	.031	.026	.066	.062	.179	.167
	(.001)	(.001)	(.001)	(.002)	(.002)	(.003)	(.004)	(.005)
Mother								
Mom Employed	.694	.621	.716	.644	.690	.616	.714	.638
	(.010)	(.013)	(.012)	(.013)	(.011)	(.013)	(.012)	(.014)
Num Kids	2.42	3.08	2.43	3.08	2.42	3.09	2.41	3.06
	(.026)	(.027)	(.028)	(.028)	(.027)	(.027)	(.026)	(.028)
Married	.798	.825	.775	.803	.798	.828	.772	.803
	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)	(.007)
Less Than HS	.171	.189	.153	.176	.172	.190	.150	.173
	(.015)	(.022)	(.019)	(.027)	(.016)	(.023)	(.018)	(.027)
High School	.369	.359	.294	.280	.369	.356	.290	.276
	(.014)	(.015)	(.012)	(.013)	(.014)	(.016)	(.011)	(.013)
Some College	.265	.263	.324	.323	.261	.263	.328	.329
	(.006)	(.007)	(.006)	(.008)	(.007)	(.008)	(.006)	(.008)
BA/Prof Deg.	.195	.188	.229	.221	.198	.191	.231	.222
	(.005)	(.006)	(.008)	(.011)	(.006)	(.008)	(.008)	(.012)
Black	.130	.135	.127	.129	.131	.136	.127	.129
(Non-Hispanic)	(.014)	(.016)	(.014)	(.014)	(.014)	(.016)	(.014)	(.015)
White	.705	.674	.680	.643	.704	.678	.687	.652
(Non-Hispanic)	(.036)	(.043)	(.040)	(.048)	(.035)	(.040)	(.040)	(.047)
Hispanic	.120	.146	.144	.179	.120	.143	.139	.172
	(.034)	(.043)	(.039)	(.050)	(.033)	(.041)	(.038)	(.050)
Mom's Age	38.38	35.46	39.08	36.07	38.41	35.45	39.05	36.02
	(.112)	(.114)	(.130)	(.133)	(.112)	(.127)	(.135)	(.146)
Mom's Health	2.10	2.05	2.06	1.99	2.11	2.05	2.05	1.98
(Decr. Scale 1-5)	(.017)	(.021)	(.017)	(.025)	(.017)	(.020)	(.017)	(.027)
Child								
Child's Age	11.93	10.85	11.92	10.86	11.94	10.85	11.89	10.81
	(.022)	(.021)	(.026)	(.025)	(.023)	(.033)	(.026)	(.033)
Child Male	.511	.512	.511	.515	.511	.515	.508	.516
	(.001)	(.002)	(.002)	(.003)	(.002)	(.004)	(.003)	(.006)

Notes: Coefficients are weighted sample means. Standard errors are clustered by state of residence and are included in parentheses. Each column reflects the samples displayed in Figure 2, as described in the text.

Table 2: Means by Morbidity

	Hospitalization		**Injury/Poisoning**		**Asthma**		**ER Visit**	
	No Episode	Had Episode	No Episode	Had Episode	No Episode	Had Episode	No Episode	Had Episode
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Number of Obs	268266	6576	95456	2777	71435	4927	36669	8169
Mother Employed	.694	.672	.715	.726	.690	.681	.716	.704
	(.011)	(.010)	(.012)	(.014)	(.011)	(.014)	(.012)	(.014)
Num Kids	2.43	2.33	2.44	2.25	2.43	2.31	2.43	2.36
	(.026)	(.030)	(.028)	(.026)	(.028)	(.026)	(.027)	(.029)
Married	.799	.737	.775	.775	.802	.745	.787	.704
	(.006)	(.010)	(.006)	(.008)	(.006)	(.010)	(.006)	(.008)
Less Than HS	.170	.203	.155	.093	.174	.148	.145	.174
	(.016)	(.013)	(.019)	(.008)	(.016)	(.010)	(.019)	(.015)
High School	.369	.389	.294	.267	.370	.355	.288	.301
	(.014)	(.015)	(.012)	(.011)	(.014)	(.013)	(.011)	(.014)
Some College	.265	.255	.323	.372	.260	.280	.326	.337
	(.006)	(.008)	(.007)	(.015)	(.007)	(.011)	(.007)	(.008)
BA/Prof Deg.	.196	.152	.228	.269	.197	.217	.241	.188
	(.005)	(.007)	(.008)	(.013)	(.006)	(.009)	(.009)	(.009)
Black	.130	.141	.129	.073	.129	.155	.123	.148
(Non-Hispanic)	(.014)	(.015)	(.014)	(.009)	(.014)	(.015)	(.014)	(.016)
White	.705	.720	.676	.828	.704	.704	.687	.686
(Non-Hispanic)	(.036)	(.031)	(.041)	(.023)	(.036)	(.026)	(.041)	(.036)
Hispanic	.120	.107	.146	.070	.121	.102	.142	.125
	(.034)	(.029)	(.040)	(.018)	(.034)	(.023)	(.039)	(.034)
Mother's Age	38.37	38.57	39.06	39.76	38.40	38.45	39.12	38.69
	(.113)	(.123)	(.131)	(.190)	(.114)	(.144)	(.138)	(.146)
Mother's Health	2.09	2.35	2.06	2.12	2.09	2.29	2.02	2.23
(Decr. Scale 1-5)	(.017)	(.024)	(.017)	(.023)	(.018)	(.019)	(.019)	(.019)
Child's Age	11.91	12.70	11.90	12.59	11.94	11.93	11.84	12.13
	(.022)	(.048)	(.026)	(.059)	(.024)	(.043)	(.025)	(.048)
Child Male	.511	.500	.508	.592	.506	.579	.500	.545
	(.001)	(.006)	(.002)	(.009)	(.003)	(.008)	(.003)	(.007)
Post Sample (1997+)	46264	992	45769	1487	44405	2851	36669	8169
School Loss Days	3.57	13.52	3.70	5.55	3.53	7.49	3.28	6.23
	(.076)	(.675)	(.082)	(.182)	(.084)	(.234)	(.069)	(.159)
Low Birthweight	.071	.096	.071	.075	.070	.090	.069	.081
	(.002)	(.011)	(.002)	(.006)	(.002)	(.009)	(.002)	(.004)
Usual Place of Care	.918	.912	.917	.939	.916	.947	.923	.908
is NOT an ER	(.008)	(.010)	(.008)	(.007)	(.008)	(.006)	(.008)	(.007)
Dr. Ever Diagnosed	.138	.259	.138	.212	.087	1	.124	.226
with Asthma	(.003)	(.018)	(.003)	(.012)	(.002)	(.000)	(.002)	(.007)
Hospitalization	0	1	.019	.043	.018	.051	.007	.081
			(.001)	(.006)	(.001)	(.004)	(.000)	(.004)
Injury/Poisoning	.031	.068	0	1	.030	.064	.013	.123
	(.001)	(.008)			(.002)	(.006)	(.001)	(.004)
Asthma Episode	.057	.149	.057	.117	0	1	.046	.122
	(.001)	(.011)	(.001)	(.011)			(.002)	(.005)
ER Visit	.168	.719	.162	.674	.167	.365	0	1
	(.004)	(.015)	(.004)	(.016)	(.004)	(.015)		

Notes: See Table 1 and text.

Table 3: Ordinary Least Squares Estimates of the Effect of Maternal Employment on Child Health

	Health Outcome: Hospitalization		**Health Outcome: Injury/Poisoning**		**Health Outcome: Asthma Episode**		**Outcome: ER Visit**
	All Children (1)	Post Sample (2)	Post Children (3)	Post Sample (4)	Sample Children (5)	Post Sample (6)	Post Sample (7)
Number of Obs	274842	44838	98233	44838	76362	44838	44838
Frac Had Episode	.0234 (.0010)	.0210 (.0009)	.0312 (.0013)	.0349 (.0014)	.0655 (.0016)	.0597 (.0015)	.1787 (.0044)
(1) Baseline	-.0021 (.0007)	-.0036 (.0017)	.0016 (.0016)	.0017 (.0029)	-.0021 (.0029)	-.0015 (.0034)	-.0090 (.0054)
(2) + Survey FE	-.0024 (.0006)	-.0040 (.0017)	.0009 (.0014)	.0013 (.0027)	-.0024 (.0027)	-.0018 (.0034)	-.0097 (.0051)
(3a) + Child Demographics	-.0033 (.0006)	-.0047 (.0017)	-.0003 (.0014)	.0002 (.0027)	-.0025 (.0028)	-.0020 (.0034)	-.0115 (.0052)
(3b) + Child Demo + Extra	X (X)	-.0046 (.0017)	X (X)	.0002 (.0027)	X (X)	-.0018 (.0034)	-.0112 (.0052)
(4) + Family Structure	-.0037 (.0007)	-.0059 (.0019)	-.0018 (.0014)	-.0016 (.0027)	-.0059 (.0028)	-.0061 (.0035)	-.0187 (.0050)
(5) + Mother Demographics	-.0032 (.0007)	-.0051 (.0019)	-.0033 (.0015)	-.0024 (.0029)	-.0089 (.0032)	-.0088 (.0036)	-.0149 (.0049)
(6) Income	-.0026 (.0007)	-.0043 (.0020)	-.0035 (.0017)	-.0019 (.0031)	-.0089 (.0033)	-.0077 (.0034)	-.0093 (.0051)
(7) Mom Health	-.0015 (.0007)	-.0035 (.0021)	-.0025 (.0016)	-.0012 (.0030)	-.0042 (.0031)	-.0033 (.0036)	-.0059 (.0049)

Notes: Each coefficient is from a separate regression of the child health outcome on maternal employment, standard errors (clustered by state) are in parentheses. The rows add covariates successively: Row (1) includes only an indicator for post-redesign, Row (2) adds quarter, state, and year fixed effects, Row (3) adds the child's age (dummy variables) and sex, Row (3b) adds an indicator for whether the child had low birth weight, Row (4) adds the mother's marital status (married or not married), the number of children (1, 2, 3, 4, and 5 or more), and dummy variables for the age spread between the oldest and youngest child present in the family, and Row (5) adds dummies for mother's age (18-24, 25-29, 30-34, 35-39, 40-64), mother's education (less than high school, high school, some college, or BA/Professional Degree), mother's race/ethnicity (black, white, Hispanic, other). In the bottom panel, the following covariates are added to specification (5): Row (6) adds income dummies and Row (7) adds mother's health indicators. The columns are estimated on different populations, as indicated.

Table 4: The Effects of Maternal Employment on Child Health

Health Outcome: Hospitalization

Panel A:	N	Mean Hospital	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
(1) All Children	88887	.0203	.6205	-.0018	.0842	.0033	.0388
(Youngest Sib 2-8)		(.0010)	(.0127)	(.0011)	(.005)	(.0011)	(.0133)
(2) All Children	66160	.0205	.6235	-.0021	.0706	.0033	.0474
(Youngest Sib 3-7)		(.0011)	(.0126)	(.0014)	(.0054)	(.0012)	(.0173)
(3) All Children	41583	.0211	.6243	-.0035	.0433	.0042	.0974
(Youngest Sib 4-6)		(.0011)	(.0123)	(.0016)	(.0068)	(.0014)	(.0388)
(4) Post Sample	10389	.0163	.6382	-.0071	.0721	.0030	.0417
(Youngest Sib 2-8)		(.0013)	(.0136)	(.0028)	(.0107)	(.0032)	(.0455)

Health Outcome: Injury/Poisoning

Panel B:	N	Mean Injury	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
(1) Post Children	31960	.0262	.6438	-.0008	.0736	.0038	.0510
(Youngest Sib 2-8)		(.0015)	(.0133)	(.0020)	(.0081)	(.0020)	(.0268)
(2) Post Children	23714	.0259	.6466	-.0010	.0684	.0030	.0443
(Youngest Sib 3-7)		(.0016)	(.0130)	(.0022)	(.0079)	(.0020)	(.0292)
(3) Post Children	14868	.0243	.6477	-.0008	.0428	.0026	.0596
(Youngest Sib 4-6)		(.0017)	(.0127)	(.0024)	(.0094)	(.0024)	(.0545)
(4) Post Sample	10389	.0278	.6382	.0015	.0721	.0022	.0306
(Youngest Sib 2-8)		(.0021)	(.0136)	(.0038)	(.0107)	(.0049)	(.0676)

Health Outcome: Asthma Episode

Panel C:	N	Mean Asthma	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
(1) Sample Children	20304	.0624	.6164	-.0072	.0856	.0103	.1203
(Youngest Sib 2-8)		(.0028)	(.0132)	(.0045)	(.0085)	(.005)	(.0611)
(2) Sample Children	15205	.0636	.6161	-.0073	.0733	.0109	.1489
(Youngest Sib 3-7)		(.0031)	(.0130)	(.0051)	(.0088)	(.0042)	(.0599)
(3) Sample Children	9642	.0643	.6134	-.0065	.0519	.0106	.2034
(Youngest Sib 4-6)		(.0036)	(.0144)	(.0065)	(.0108)	(.0056)	(.1105)
(4) Post Sample	10389	.0561	.6382	-.0010	.0721	.0107	.1484
(Youngest Sib 2-8)		(.0026)	(.0136)	(.0040)	(.0107)	(.0062)	(.0918)

Health Outcome: ER Visit

Panel D:	N	Mean ER	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
(1) Post Sample	10389	.1674	.6382	-.0001	.0721	.0055	.0767
(Youngest Sib 2-8)		(.0055)	(.0136)	(.0079)	(.0107)	(.0127)	(.1778)
(2) Post Sample	7757	.1682	.6377	-.0033	.0701	.0059	.0848
(Youngest Sib 3-7)		(.0062)	(.0135)	(.0099)	(.0106)	(.0119)	(.1716)
(3) Post Sample	4892	.1693	.6366	-.0063	.0523	.0078	.1491
(Youngest Sib 4-6)		(.0063)	(.0149)	(.0134)	(.0126)	(.0136)	(.2697)

Notes: Each coefficient is from a separate regression and includes the covariates listed in Table 3 Row 5, with standard errors (clustered by state) in parentheses. Observations are children ages 7 to 17 whose youngest sibling is within the age range specified by row. Coefficients presented are: Column (1) regression of child health on maternal employment, Column (2) regression of maternal employment on the youngest child's eligibility for kindergarten (First Stage), Column (3) regression of child health on the youngest child's eligibility for kindergarten (Reduced Form), and Column (4) instrumental variables.

Table 5: Heterogeneous Effects by Demographic Characteristics of Maternal Employment on Overnight Hospitalizations

Health Outcome: Overnight Hospitalization							
	N	Mean Hospital	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
All Children	88887	.020	.621	-.0018	.0842	.0033	.0388
		(.001)	(.013)	(.0011)	(.0050)	(.0011)	(.0133)
Black (Non-Hispanic)	13732	.022	.675	-.0045	.0507	.0073	.1450
		(.002)	(.016)	(.0034)	(.0162)	(.0036)	(.0740)
White (Non-Hispanic)	53386	.021	.632	-.0015	.0951	.0039	.0408
		(.001)	(.009)	(.0012)	(.0075)	(.0015)	(.0165)
Hispanic	18182	.019	.515	-.0013	.0800	.0007	.0087
		(.001)	(.019)	(.0019)	(.0089)	(.0019)	(.0236)
Mom HS or Less	52155	.022	.568	-.0030	.0831	.0035	.0425
		(.001)	(.018)	(.0018)	(.0066)	(.0019)	(.0227)
Mom Some College or More	36732	.018	.685	.0000	.0831	.0024	.0287
		(.001)	(.007)	(.0014)	(.0085)	(.0017)	(.0207)
Married	71833	.019	.617	-.0002	.0865	.0025	.0292
		(.001)	(.012)	(.0009)	(.0060)	(.0011)	(.0125)
Not Married	17054	.026	.639	-.0094	.0656	.0062	.0952
		(.002)	(.020)	(.0045)	(.0124)	(.0035)	(.0589)
Mother Age 25-29	11404	.022	.588	-.0021	.0706	.0091	.1284
		(.002)	(.018)	(.0028)	(.016)	(.0037)	(.0692)
Mother Age 30-34	28501	.022	.632	-.0025	.0894	.0043	.0476
		(.002)	(.014)	(.0022)	(.0078)	(.0021)	(.0228)
Mother Age 35-39	30318	.019	.636	-.0010	.1026	.0023	.0227
		(.001)	(.013)	(.0017)	(.0072)	(.0019)	(.0187)
Mother Age 40-64	17948	.018	.602	-.0028	.0560	.0007	.0128
		(.001)	(.012)	(.0025)	(.0108)	(.0023)	(.0408)
Children 1998-2004	27242	.017	.643	-.0046	.0741	.0038	.0507
		(.001)	(.013)	(.0018)	(.0087)	(.0017)	(.0238)
Any Health Insurance	23305	.018	.654	-.0060	.0798	.0047	.0587
		(.001)	(.011)	(.0018)	(.0103)	(.0020)	(.0249)
Private Health Insurance	16327	.015	.690	-.0017	.0803	.0054	.0667
		(.001)	(.009)	(.0018)	(.0128)	(.0021)	(.0271)
Public Health Insurance	6978	.028	.541	-.0105	.0709	.0033	.0465
		(.003)	(.020)	(.0053)	(.0134)	(.0044)	(.0646)

Notes: All coefficients are from linear models. Each coefficient is from a separate regression including covariates listed in Table 3 Row 5, with standard errors (clustered by state) in parentheses. All samples include children ages 7 to 17 whose youngest sibling was between 24 and 107 months at the scheduled interview date. The rows are represented subsamples as specified. The coefficients presented are: Column (1) regression of child health on maternal employment, Column (2) regression of maternal employment on the youngest child's eligibility for kindergarten (First Stage), Column (3) regression of child health on the youngest child's eligibility for kindergarten (Reduced Form), and Column (4) instrumental variables estimates.

Table 6: Means by Labor Force Attachment Quartile

	Lowest Quartile	3rd Quartile	2nd Quartile	Highest Quartile
	(1)	(2)	(3)	(4)
Number of Obs	13347	13346	13347	13346
Hospitalization	.023	.019	.02	.02
	(.002)	(.002)	(.002)	(.001)
Injury/Poisoning	.032	.032	.031	.034
	(.004)	(.003)	(.002)	(.003)
Asthma Episode	.054	.072	.068	.065
	(.009)	(.007)	(.006)	(.006)
ER Visit	.196	.162	.158	.16
	(.014)	(.012)	(.01)	(.009)
Mother Worked	.485	.609	.668	.758
	(.006)	(.006)	(.006)	(.006)
Work Hours if Worked	34.052	32.661	33.982	35.571
	(.384)	(.384)	(.384)	(.384)
Work Hours with Zero's	7.632	11.598	14.88	21.025
	(.403)	(.403)	(.403)	(.403)
Num Kids	4.013	3.14	2.615	2.218
	(.036)	(.036)	(.036)	(.036)
Married	.943	.954	.924	.76
	(.016)	(.016)	(.016)	(.016)
Mother's Age	34.183	35.553	36.061	37.028
	(.133)	(.133)	(.133)	(.133)
Mom HS or Less	.754	.516	.401	.28
	(.014)	(.014)	(.014)	(.014)
Mom Some College or More	.246	.484	.599	.72
	(.014)	(.014)	(.014)	(.014)

Notes: Coefficients are weighted means with standard errors (clustered by state) in parentheses. The sample is restricted to children ages 7 to 17, with white mothers, who have at least one younger sibling and whose youngest sibling was between 24 and 107 months at the scheduled interview date. The Labor Force Attachment index is calculated as described in the text.

Table 7: Heterogeneous Effects of Maternal Employment on Overnight Hospitalizations by Labor Force Attachment Quartiles

Health Outcome: Overnight Hospitalization							
	N	Mean Hospital	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
All White Children	53386	.021	.632	-.0015	.0951	.0039	.0408
		(.001)	(.009)	(.0012)	(.0075)	(.0015)	(.0165)
Bottom Half LFA	26693	.021	.548	-.0005	.1077	.0048	.0446
		(.001)	(.005)	(.0020)	(.0106)	(.0021)	(.0218)
Top Half LFA	26693	.020	.713	-.0033	.0746	.0014	.0190
		(.001)	(.007)	(.0021)	(.0090)	(.0026)	(.0359)
Lowest Quartile	13347	.023	.485	-.0010	.0979	.0069	.0706
		(.002)	(.006)	(.0024)	(.0152)	(.0049)	(.0530)
3rd Quartile	13346	.019	.609	.0003	.1161	.0026	.0228
		(.002)	(.007)	(.0030)	(.0154)	(.0025)	(.0215)
2nd Quartile	13347	.020	.668	-.0026	.0875	.0001	.0014
		(.002)	(.008)	(.0024)	(.0149)	(.0036)	(.0407)
Highest Quartile	13346	.020	.758	-.0038	.0577	.0026	.0445
		(.001)	(.006)	(.0033)	(.0107)	(.0041)	(.0712)

Notes: See notes to Table 4 for column descriptions. The sample is restricted to children ages 7 to 17, with white mothers, who have at least one younger sibling and whose youngest sibling was between 24 and 107 months at the scheduled interview date. All coefficients are from separate regressions, standard errors (clustered by state of residence) are in parentheses. The sample is broken down by the labor force attachment scale, as described in the text.

Table 8: Robustness Checks

Health Outcome: Overnight Hospitalization							
	N	Mean Hospital	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
All Children	88887	.020	.621	-.0018	.0842	.0033	.0388
		(.001)	(.013)	(.0011)	(.0050)	(.0011)	(.0133)
Child Not Limited	82372	.016	.624	-.0006	.0857	.0026	.0304
		(.001)	(.013)	(.0008)	(.0052)	(.0010)	(.0123)
No Kids Limited	74661	.016	.631	-.0002	.0866	.0025	.0288
		(.001)	(.013)	(.0008)	(.0058)	(.0010)	(.0117)
2 Children	30006	.021	.703	-.0025	.0314	.0045	.1437
		(.001)	(.010)	(.0019)	(.0076)	(.0029)	(.0938)
3+ Children	58881	.020	.576	-.0014	.0965	.0029	.0306
		(.001)	(.014)	(.0013)	(.0065)	(.0013)	(.0135)
Mom in Very Good or Excellent Health	58530	.018	.652	.0005	.0873	.0046	.0525
		(.001)	(.011)	(.0012)	(.0076)	(.0013)	(.0154)
Child 9-17	67342	.020	.632	-.0015	.0843	.0038	.0456
		(.001)	(.013)	(.0013)	(.0056)	(.0014)	(.0170)
Child 9-12	42271	.017	.628	-.0026	.1028	.0051	.0500
		(.001)	(.013)	(.0015)	(.0073)	(.0021)	(.0211)

Notes: See notes to Table 5.

Appendix Table A: Kindergarten Eligiblity Cut-off Dates for 1983 and 2004

Approximate Cut-off Date	States 1983-1984 School Year	States 2004-2005 School Year
July 1	IN	IN
August 1		MO
August 15		AK
September 1	AZ, FL, GA, KS, MN, ND, NM, OK, PA, SD, TX, UT, WA, WV, WI	AL, AZ, DE, FL, GA, ID, IL, KS, MN, MS, NM, ND, OK, OR, PA, RI, SC, SD, TX, UT, WA, WV, WI
September 15	IA, MT, WY	AR, IA, MT, WY
October 1	AL, AR, KY, MO, NV, OH, VA	KY, LA, NV, OH, TN, VA
October 15	ID, ME, NE, NC	ME, NE, NC
November 1	AK, SC, TN	MD
November 15	OR	
December 1	CA, IL, MI, NY	CA, MI, NY*
January 1	CT, DE, DC, HI, LA, MD, RI, VT	CT, DC, HI, VT
LEA	CO, MA, MS, NH, NJ	CO, MA, NH, NJ

Note: Cut-off dates are rounded for ease of presentation and some cut-off dates are interpolated for years where exact cut-off dates could not be obtained. Data acquired from individual state statues. * NY legally removed the State-level recommendation, but it appears that all major school districts retained a December 1st cut-off.

Appendix Table B: Key Variable Definitions

Variable	1985-1996 Surveys	1997-2004 Surveys
Mother Worked	Employment Status in Past TWO WEEKS Equals 1 if worked, 0 if did not work, dropped otherwise	Doing LAST WEEK
Youngest Child's Age	Determined from date of birth and interview date. Birth month and year available all years, birth *day* 1997-2004 only. Youngest child's age is calculated both at the kindergarten eligibility cut-off month (for the instrument) and at the interview date (for sample selection).	
Kindergarten Eligibility	Kindergarten eligibility is determined by whether the youngest child achieved 60 months of age by the cut-off date. When state-specific cut-offs are not available, I use September 1st. Eligibility is measured for the most recent school year. Note: Though three health outcomes span the past 12 months, contemporaneous school eligibility is used throughout the analysis	
Overnight Hospitalizations	Derived from the number of short stay hospital episodes in the past year. *Defined for all children*	Derived from the number of hospital stays. *Defined for all children*
Injury/Poisoning	*Not available*	The child had an injury or poisoning episode in the past 3 months *Defined for all children*
Asthma Episode	During the past 12 months, did ___ have... *Asthma? *Children in familes assigned to condition list 6*	During the past 12 months, has ___ had an episode of asthma or an asthma attack? (Question only asked if child has ever been diagnosed with asthma by a doctor.) *Children selected as Sample Child*
Emergency Room Visit	*Not available*	Derived from number of ER visits in the past 12 months *Children selected as Sample Child*

Appendix Table C: Non-linear Models, Compare to Table 4

Health Outcome: Hospitalization

Panel A:	N	Mean Hospital	Mean Work	Probit (1)	First Stage (2)	Reduced Form (3)	IV BiProb (4)
(1) All Children (Youngest Sib 2-8)	88887	.0203 (.0010)	.6205 (.0127)	-.0017 (.0010)	.0899 (.0054)	.0030 (.0010)	.0255 (.0086)
(2) All Children (Youngest Sib 3-7)	66160	.0205 (.0011)	.6235 (.0126)	-.0018 (.0013)	.0754 (.0058)	.0030 (.0011)	.0218 (.0076)
(3) All Children (Youngest Sib 4-6)	41583	.0211 (.0011)	.6243 (.0123)	-.0032 (.0014)	.0465 (.0073)	.0039 (.0013)	.0266 (.0107)
(4) Post Sample (Youngest Sib 2-8)	10389	.0163 (.0013)	.6382 (.0136)	-.0065 (.0022)	.0770 (.0110)	.0030 (.0027)	.0117 (.0081)

Health Outcome: Injury/Poisoning

Panel B:	N	Mean Injury	Mean Work	Probit (1)	First Stage (2)	Reduced Form (3)	IV BiProb (4)
(1) Post Children (Youngest Sib 2-8)	31960	.0262 (.0015)	.6438 (.0133)	-.0008 (.0019)	.0785 (.0083)	.0031 (.0019)	.0236 (.0135)
(2) Post Children (Youngest Sib 3-7)	23714	.0259 (.0016)	.6466 (.0130)	-.0010 (.0020)	.0729 (.0082)	.0026 (.0017)	.0395 (.0191)
(3) Post Children (Youngest Sib 4-6)	14868	.0243 (.0017)	.6477 (.0127)	-.0009 (.0021)	.0461 (.0073)	.0023 (.0020)	X (X)
(4) Post Sample (Youngest Sib 2-8)	10389	.0278 (.0021)	.6382 (.0136)	.0009 (.0034)	.0770 (.0110)	.0019 (.0041)	.0325 (.0512)

Health Outcome: Asthma Episode

Panel C:	N	Mean Asthma	Mean Work	Probit (1)	First Stage (2)	Reduced Form (3)	IV BiProb (4)
(1) Sample Children (Youngest Sib 2-8)	20304	.0624 (.0028)	.6164 (.0132)	-.0066 (.0042)	.0919 (.0089)	.0093 (.0046)	.0486 (.0151)
(2) Sample Children (Youngest Sib 3-7)	15205	.0636 (.0031)	.6161 (.0130)	-.0063 (.0047)	.0792 (.0094)	.0099 (.0038)	.0549 (.0182)
(3) Sample Children (Youngest Sib 4-6)	9642	.0643 (.0036)	.6134 (.0144)	-.0058 (.0059)	.0572 (.0117)	.0098 (.0050)	.0567 (.0283)
(4) Post Sample (Youngest Sib 2-8)	10389	.0561 (.0026)	.6382 (.0136)	-.0013 (.0038)	.0770 (.0110)	.0097 (.0059)	.0498 (.0297)

Health Outcome: ER Visit

Panel D:	N	Mean ER	Mean Work	Probit (1)	First Stage (2)	Reduced Form (3)	IV BiProb (4)
(1) Post Sample (Youngest Sib 2-8)	10389	.1674 (.0055)	.6382 (.0136)	.0008 (.0077)	.0770 (.0110)	.0065 (.0127)	.0816 (.086)
(2) Post Sample (Youngest Sib 3-7)	7757	.1682 (.0062)	.6377 (.0135)	-.0023 (.0097)	.0750 (.0109)	.0071 (.0120)	.0825 (.1259)
(3) Post Sample (Youngest Sib 4-6)	4892	.1693 (.0063)	.6366 (.0149)	-.0058 (.0133)	.0572 (.0133)	.0095 (.0138)	.1447 (.1162)

Notes: See Table 4 notes. Columns (1) - (3) are marginal effects from a probit model specification. Column (4) are marginal effects from a bivariate probit estimation.

Appendix Table D: Estimated Weights for Local Average Treatment Effect

Q	w_q (1)	$\lambda_{q\|x}$ (2)	$\Delta S_{q\|x}$ (3)	$\omega_{q\|x}$ (4)
Lowest Quartile	.25	.207	.089	.295
			(.012)	
3rd Quartile	.25	.213	.085	.290
			(.014)	
2nd Quartile	.25	.196	.074	.232
			(.011)	
Highest Quartile	.25	.178	.064	.183
			(.009)	