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

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Abstract

The effects of maternal employment on children's health are theoretically ambiguous and challenging to identify. There are trade-offs between income and time, and a mother's decision to work reflects, in part, her children's health and her underlying preferences. I utilize exogenous variation in each child's youngest sibling's eligibility for kindergarten as an instrument. Using the restricted-access National Health Interview Survey (1985-2004), I identify the effects on overnight hospitalizations, asthma episodes, and injuries/poisonings for children ages seven to seventeen. Employment increases the probability of each adverse health event by nearly 200 percent. These effects are robust and do not reflect a non-representative local effect.

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, in 1975 54.9 percent of women with children ages six to seventeen were in the civilian labor force (Chao and Rones, 2006). 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 but have more money to purchase services, leading to an ambiguous net effect.

Previous studies on the effects of maternal employment find little measurable impact on child health, as discussed further in Section **??**. 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

[1] For example, Dietz (1997) and Fletcher et al. (2010).

[2] For example, Case and Paxson (2006) and Case, Fertig, and Paxson (2005).

[3] For example, Lleras-Muney (2005).

[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 children are unhealthy, some mothers reduce their labor supply. Gould (2004) shows that mothers reduce their labor supply if their children have a time intensive disability but increase their labor supply if children have a high-cost disability. Corman et al. (2005) find that having an unhealthy child 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.

with 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.[5] 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 three health events: overnight hospitalizations, asthma episodes, and injuries and poisonings. These measures capture both acute and chronic conditions that are plausibly related to contemporaneous labor supply, but do not necessarily imply any long-term health consequences. Still, as argued in Section **??**, these measures do reflect unambiguously negative health events for children. While each measure has its own strengths and weaknesses, when taken

[5]See, e.g., Ruhm, 2008.

together the estimates, presented in Section ??, provide compelling evidence of an increase in the probability a child experiences an adverse health event due to maternal employment. This does not necessarily imply that there are long-term health consequences for children. In fact, it may be the case that children suffer from these short-term health events, but on net gain a larger health stock as a consequence of maternal employment.

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. 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 restrict the estimation sample to be children ages seven through seventeen years old that have at least one younger sibling whose youngest sibling is within a specified age range around five years old. I use each focal 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 focal 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 focal 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. The 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). The effect sizes I find are very large; each represents an approximately 200 percent increase. Although the estimates are sometimes imprecise, the coefficients are consistent across different samples and for all three health events. I explore whether the effects are heterogeneous by socioeconomic status, by labor force attachment, or across major demographic categories. Although the coefficients do vary, there is not enough power to detect any stastitically significant differences. The instrumental variables results suggest that, contrary to the basic OLS relationship, maternal employment increases a child's risk of experiencing an adverse health event.

2 Related Literature

Literature assessing the effects of maternal employment on children has focused primarily on child development as an outcome, perhaps due to the wider availability of objective measures such as academic performance, and on children at early ages (see, e.g., Bernal, 2008; Blau and Grossberg, 1992; Desai, Chase-Lansdale, and Michael, 1989; Kaestner and Corman, 1995; Ruhm, 2004; 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 (2008) 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., Bitler and Hoynes, 2007; Moore and Driscoll, 1997). 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 growing literature that finds maternal employment increases childhood obesity risk, though only for the higher socioeconomic status populations (Anderson, Butcher, and Levine, 2003).[6]

Several recent studies have sought to identify the effects of maternal employment on the health of children. Gennetian et al. (2010) consider the effects of low-income mothers' employment on the health of young children by exploiting a welfare-to-work experiment, the National Evaluation of Welfare-to-Work Strategies (NEWWS). They find that among the low-income children in the sample maternal employment decreases a child's probability of being in good or excellent health by a modest amount. Ruhm (2008) 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 mother and family characteristics. He finds large differences in effects by the child's socioeconomic status (and other proxies for disadvantage), where disadvantaged children see no effect or benefit from maternal employment and advantaged children experience harmful consequences.

Baker, Gruber, and Mulligan (2008) 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-differences 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. Baker et al. consider the impact of the child care subsidy

[6] Fertig, Gloom, and Tchernis (2009) provide a thorough review of the literature and an analysis of the mechanisms by which maternal employment affects childhood obesity.

program on the child who is eligible and therefore cannot separate the direct effect of child care from the effect of maternal employment.

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

Medical and epidemiological literatures have explored how demographic characteristics of children and their families contribute to disease incidence, severity, and management. Poverty has long been established as a leading risk factor for many childhood ailments, as has being a racial or ethnic minority.[7] On the whole, relatively little attention has been paid outside of the social sciences to the potentially harmful - or beneficial - effects of maternal employment.

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

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 an ordinary least squares 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 ($Cov(Z, \epsilon) = 0$). Because it is an exactly identified model, the instrumental variable estimate of β can be thought of as resulting from the division of the "reduced form" estimate, β_{RF} below, by the first-stage coefficient described above, β_{FS}. The reduced form equation is the regression of the child health outcome on the instrument:

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

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 our case maternal employment. This is discussed further in Section 5.[8]

All three child health measures I analyze 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 (2001) 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 (2001) 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.

[8]One can consider specifications using a binary instrument in a two-stage least squares model as a fuzzy regression discontinuity design (Imbens and Lemieux, 2008). Ideally, in a fuzzy regression discontinuity model, one 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 in this context. Extensive covariates are included to minimize potential bias associated with this limitation.

3.2 The Instrument: Youngest Sibling's Kindergarten Eligibility

The 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 cutoff date and many states changed their policies over the 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 moved their cutoff date earlier, so children had to be somewhat older when entering school in the later periods, and that September 1st remains the modal cutoff date.[9] Some states allow the local educational authorities (LEAs) to determine their own cutoff date, as indicated in the bottom row of Appendix Table A. I use state and year specific cutoff dates where available and assume a September 1st cutoff date for states with no standard date; however, results are not sensitive to including only states with a standard cutoff date.[10,11]

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, 2009).[12] To confirm

[9]For a comprehensive list of the state laws used in this study see Table 1 of Evans et al. (2010).

[10]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.

[11]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.

[12]Gelbach (2002) argues that kindergarten provides a cost subsidy for child care, so the eligibility of a child for

this relationship for our sample, Figure 1 illustrates the basic relationship between a mother's youngest child's age and her likelihood of employment. This graph uses the full sample of mothers in the restricted National Health Interview Survey (NHIS) from years 1985-2004,[13] regardless of the number and ages of her children (or child), and each mother is represented only once in this sample. In the graph, I consider only those mothers whose youngest child's exact eligibility could be determined, dropping observations from children living in states with no state-wide kindergarten eligibility law in place during the most recent school year.

Figure 1 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 cutoff date faced by that child. The dots in Figure 1 represent the fraction of mothers that were employed and locally weighted regression 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 cutoff date. This provides suggestive evidence that kindergarten eligibility raises the probability a mother works, which is confirmed in estimates of β_{FS} presented in Section **??**.[14]

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

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 (2009) 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. In related recent work, Fitzpatrick (2010) shows little effect of universal pre-kindergarten programs on women's labor supply using a regression discontinuity design framework.

[13] For more information on the data, see Section **??**.

[14] 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 (NHIS), this link cannot be tested directly because kindergarten enrollment is not observed. Elder and Lubotsky (2009) 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.

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). Identification comes from two sources, variation in the exact timing of fertility and variation across states in eligibility laws. For example, I measure the health difference between two otherwise identical eight year old boys living in the same state, 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 the validity of the estimated effects 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. Similarly, consider two otherwise identical eight year old boys both with youngest siblings that turned five years old on September 15th of the most recent school year, one that lives in Texas and one that lives in Kentucky. In Texas the youngest sibling was not eligible for kindergarten, since she did not reach 60 months of age by the September 1st cutoff. However, the youngest sibling of the child that lives in Kentucky would be eligible, since that child turned 60 months two weeks prior to the October 1st cutoff. Thus, these two sources of variation, and the rich set of controls that are included, should allow for an accurate estimate of the effect.

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 explore threats to instrument validity and other potential sources of bias further, which all confirm the robustness of my main findings.

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.[15] Because true underlying health cannot be measured, I instead analyze three

[15]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

health events which capture both chronic and acute conditions: overnight hospitalizations, asthma episodes, and injuries and poisonings. Each of these measures most likely reflects a health experience 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 episode has its own strengths and weaknesses in its ability to approximate health, my analysis over all three health measures provides consistent evidence that maternal employment does affect children's health, at least in the short-term.

The first health event 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.[16] Many of these conditions may be sensitive to supervision, regular access to care, and access to appropriate medication and preventative treatment.[17] Hospitalizations reflect the most severe health events, so recall of having had a hospitalization is likely measured correctly in the survey, 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 (Goodman et al., 1994), indicating that having had a hospitalization may reflect access to care in addition to true differences in morbidity.[18]

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child's mother.

[16]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.

[17]The AHRQ HCUP Fact Book No. 5, Kruzikas et al. (2004), 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.

[18]Whether the child had an emergency room visit in the past 12 months, a more common event than overnight hospitalizations, is measured for a small subset of the sample. Having had an ER visit may reflect both inadequate

The next health event 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.[19] 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.[20] 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).[21] 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 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,

access to care in a doctor's office and true emergencies. There is not sufficient sample size to produce statistically significant estimates using ER visits as an outcome, but the estimates are qualitatively similar to the three outcomes presented here. Full results for ER visits are available upon request.

[19]The Asthma and Children Fact Sheet can be accessed at: http://www.lungusa.org.

[20]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.

[21]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).

Lanphear et al., 2001). Bianchi et al. (2000) 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. On the other hand, mothers who work might be able to afford to purchase housecleaning services, thereby reducing children's asthma attacks due to household exposures.

The final health event 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 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.

While the health episodes I consider here are all *bad*, there is a large literature that suggests income is good for children's health (see, e.g., Case, Lubotsky, and Paxson, 2002). In this paper I present results that suggest that maternal labor supply increases a child's probability of experiencing an adverse health event. But, this does not necessarily imply any long-term health consequences for children. In fact, it may be the case that children suffer from these short-term health events, but on net gain a larger health stock as a consequence of maternal employment.

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 restricted-access version of the National Health Interview Survey (NHIS), conducted by the Centers for Disease Control and Prevention's National Center for Health Statistics (NCHS), satisfies these extensive data requirements. The NHIS is a repeated cross-sectional 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.[22] I combine data from survey years 1985 - 2004.[23] 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, year fixed effects are included in all relevant regressions to control for any differences in question wording across the survey years. See Appendix Table B for a description of how the key variables are defined across survey periods.[24]

Because of the NHIS survey design, each of the three health events I explore is defined for a different, nested sample of children. Figure 2 provides a diagram illustrating the relationship between these samples. Information on whether a child experienced an overnight hospitalization in the past year is available for all children in the sample. This group, denoted "All Children," consists of all children from survey years 1985-2004, ages seven through seventeen years, who

[22]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 estimation sample.

[23]The NHIS uses a stratified sampling design. Primary sampling units are drawn every ten years, so my data span inclusively two sample design periods: 1985-1994 and 1995-2004.

[24]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.

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 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, the 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 three months.[25] I denote the sample where injuries and poisonings are measured as "Post Children," which is Sample 2 in Figure 2. 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." This is the sample of children for which asthma episodes are measured. 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

[25]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.

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.[26] 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.

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 three 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, ages seven through seventeen years old. Note that mothers with more than one child ages 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. 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

[26]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.

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 the estimates 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 event are very similar. Comparing between the column panels, we see that, when restricting the sample to children where each health event 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. 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 reports the fraction of children that had each health episode. The columns are again divided by the health episodes. The rows specify different samples of children. The first row gives the fraction of all children experiencing each health episode. The second set of rows compares the fraction of children experiencing each health event by the mother's work status. For the sample of children whose mother did not work the fraction of children experiencing an overnight hospitalizations or asthma episode is larger while the fraction of children experiencing an injury or poisoning is smaller relative to the children whose mother worked. This foreshadows the coefficients in the ordinary least squares specifications in Table 3.

The bottom panel of Table 2 restricts the sample children for whom all three health events are measured. 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 were over twice as likely to have had an asthma episode compared with the full sample (14.9 percent versus 6.6 percent). Similarly, children that had an asthma episode were over twice as likely to have had a hospital episode (5 percent). Children that had an injury or poisoning in the past three months were nearly twice as likely to have been hospitalized overnight in the past 12 months than the full sample. 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 and asthma episodes 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 the main IV results.

The cells of Table 3 report the coefficient on maternal employment from separate ordinary least squares regressions.[27] Note that since each episode is considered a *negative* health outcome, a negative coefficient on maternal employment implies that working benefits child health. Each column in Table 3 represents a different sample, as in Figure 2.

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 (2) and (3) the probability a child had each health event is not statistically significantly related to maternal employment. The covariates in Row (2) are interview quarter,[28] state, and year fixed effects. Row (3) adds dummy variables for the child's age and an indicator for the child's sex.[29] 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.[30] Finally, Row (5) adds dummies for mother's age (18-24, 25-29, 30-34, 35-39, 40-64),[31] mother's education (less than high school, high school, some college, or BA/Professional Degree), and mother's race/ethnicity (black, white, Hispanic,

[27]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.

[28]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. Specifications dropping the third quarter are very similar.

[29]The child's birth weight is only available for children selected as the Sample Child in the 1997-2004 surveys. The results are robust to including this covariate, where available, as an indicator for low birth weight (below 2,500 grams, approximately 5 pounds, 8 ounces).

[30]The age spread variables are categorized in year bins with 0 or 1 year as the omitted category and 9 or more years grouped together. The results are insensitive to insteading specifying the birth spacing as the age of the oldest child minus the age of the second oldest child. Similarly, the estimates are slightly larger (although not statistically significantly so) when these controls are eliminated entirely, as in the portion of Table 8 where the sample is disaggregated by family size.

[31]Including a similar set of categories for father's age yields very similar results.

other). In the preferred (most saturated) model, reported in Row (5) of Table 3, the estimated relationship between maternal employment and each child health episode is negative and significant, implying that maternal employment is associated with a lower probability that a child experiences an adverse health event.

Income is one mechanism through which maternal employment may plausibly impact child health. As such, including controls for income level will not allow for the full effect of maternal employment to be measured. However, it is interesting to consider whether 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.[32] In the bottom panel, Row (6) adds income dummies to the specification in Row (5) and there is little change in the coefficients.

Next I explore whether a mother's own health might be related to child health in a way that is biasing the OLS estimates in Table 3.[33] 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

[32]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.

[33]The mother's 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).

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 three health episodes are negative or zero once child and maternal demographic characteristics are included as covariates. This implies that, if anything, maternal employment is associated with a reduction in a child's probability of experiencing an adverse health event. As discussed above, there are a number of potential 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 36 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).[34] The 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.[35]

Table 4 Column (1) reports the coefficient from the OLS regression of maternal employment on child health, corresponding to Equation (**??**) in Section **??**. These estimates are directly comparable to Row (5) of Table 3. These estimates differ slightly because of sample construction;

[34] The regression coefficients reported in Tables 4-6 do not employ survey weights because the IV assumption is that there is a true underlying effect, β_{IV}. Estimates using survey weights are extremely similar.

[35] See Appendix Table D for the full specification results for the baseline specification (Table 4, Row 1).

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 sample is most comparable to that in Table 4 Column (1) Row (1), which includes only the children with a youngest sibling 2-8. The OLS estimate here is -.0018 (.0011), which is somewhat smaller and no longer statistically significant. 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 an adverse health event.

The coefficient of interest in Equation (**??**), as described in Section **??**, 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 and are almost always statistically significant. For example, in Table 4 Column (3) Row (1) the estimated effect is .0031 (.0011), indicating that the youngest child's eligibility for kindergarten raises the risk of the older child having been hospitalized by .31 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 experiences an adverse health event.

These reduced form 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 (**??**)), 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, we 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 adverse 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 2SLS.[36] 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

[36]For computational and expositional simplicity, I include only the 2SLS estimates in this table. 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. The estimates are smaller in magnitude and much more precise, but the qualitative results are consistent.

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. Overall, the results in Panel A suggest a robust relationship where maternal employment increases a child's probability of having an overnight hospitalization, contrary to the OLS relationship.

Turning now to the second health measure, 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, are presented in Panel B, Row (1). These estimates imply that maternal employment increases injuries and poisonings by 5.6 percentage points, which represents just over 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 effect, but the point estimates are similar.

The probability of having had an asthma episode, Panel C in Table 4, again demonstrates an increase due to maternal employment. In Row (1), Column (4), the coefficient implies that maternal employment causes a 12 percentage point increase in the probability of having an asthma episode. Similar to the estimates above, this corresponds to just under a 200 percent increase. The effect is statistically significant, but the magnitudes become very large and the estimates are imprecise.

Table 4 provides evidence that maternal employment causes an increase in children experiencing several adverse health events. 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 approximately 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). 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 the 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.[37] 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 (.0533), and 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 (.0075 for blacks versus .0038 for whites), although the difference is not statistically significant. Column (4) presents the instrumental variable results, indicating that maternal employment causes a 14 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 percentage points. Both IV estimates are statistically significant and indicate that maternal employment increases child hospitalizations for black and white mothers. The estimates for

[37] Results for asthma and injuries/poisonings are similar, but not statistically significant. Full results are available upon request.

Hispanic mothers are not statistically significant.

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, and Ruhm, 2008). 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 2.3 percentage points for mothers with at least some college education. I therefore find suggestive evidence consistent with larger effects for children whose mothers have low education levels.

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 the child health measures 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) show a larger coefficient for not-married mothers (.0905) as compared to married mothers (.0272), although the estimate for not-married mothers is not statistically significant, so the two effects are statistically indistinguishable. These results suggest that having another adult present in the household mitigates the negative consequences of maternal employment.[38]

Although there was no statistically significant differences across the major demographic groups, the patterns do suggest that the largest effects are seen for mothers that are black, have a high school degree or less, or are not married. Still, it may be the case that the coefficient in the instrumental variable estimate is measuring the effect of maternal employment for a very specific, and potentially non-representative, sample of women within these broad categories. To explore this further, I disaggregate the sample in an alternative way, which may better approximate 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.[39,40] The second to last set of rows of Table 5 include the results from dividing the sample by median labor force attachment. Notice that while the overall probabilities of having had an overnight hospitalization are very similar (around 2 percent), the mean of maternal employment is over 20 percentage points higher for children whose mother is in the top half of the LFA scale (71 versus 51 percent). The first stage estimates in Column (2) indicate that the instrument does affect labor supply in both groups,

[38] Although for some years of data we can observe the direct relationship between the mother's husband and the child, that information is not available for a large enough sample to run the analysis just for families where we observe that the biological father is present.

[39] Full results similar to Kling (2001), including estimated weights, are included in Morrill (2008). No large differences by quartiles in weights are detected, unlike in Kling (2001).

[40] 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, race/ethnicity, education level, marital status, number of children, and age-spread of children (the difference between the oldest and youngest child's ages), as defined above. I use the coefficients from this regression to predict the labor force attachment for all 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 two groups, at or below versus above median LFA. Note that further dividing into quartiles produces qualitatively similar results, but the sample sizes are insufficient to produce statistically significant effects. Results available upon request.

though the effect is larger (in magnitude and in percent terms) for the less attached mothers. The reduced form in Column (3) is only significant for less attached sample (.4 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.

In all, potentially due to insufficient sample size and power, no strong heterogeneity in treatment effects can be found across major demographic categories or levels of labor force attachment. Although the point estimates do vary somewhat by the race/ethnicity and marital status of the mother, the results are consistently large and positive, suggesting that maternal employment does increase the probability a child has an adverse health event across these different groups. Furthermore, the estimates at the bottom of Table 5 somewhat alleviate 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. We might expect that the mothers most attached to the labor market are the most likely to be changing work intensity rather than work participation (since they are more likely to have been working already). Breaking down the sample by labor force attachment demonstrates that the results are not driven by women most attached to the labor force. On the contrary, the effects are largest for the less attached women for whom this source of bias is less of a concern.

The NHIS does not include data on child care or supervision, but there is some limited information on hours of work for survey years 1997-2004. Although the hours of work question is not ideal for this type of analysis, I include two specifications at the bottom of Table 5 that use work hours, rather than work participation, as the key explanatory variable.[41] Although this will not help to detect any treatment effect heterogeneity, it will provide an estimate of the effect along the intensive margin. In these two rows work hours is modeled linearly, first including observations that have zero hours of work and then restricting to only mothers with positive work hours.[42] On average women are working 22.67 hours, and women who work are working 35.35 hours. The first stage estimates in Column (2) indicate that when a mother's youngest child is eligible for kindergarten, this increases her hours of work by 2.81 hours. For mothers that are already working, kindergarten eligibility increases work hours by only .45 hours, which is not statistically significantly different than zero. This provides some evidence that a change in work intensity for women that already were working is not driving the results. Similarly, among children with mothers that worked, youngest sibling's kindergarten eligibility does not increase a child's probability of having had an overnight hospitalization, as seen in Column (3), the bottom row.

The estimated marginal effect of an hour work, show in Column (4), is .1 percentage points. If women who work tend to work 35 hours on average, then we would expect that having a

[41]For survey years 2001-2004 the question asks how many hours the person worked last week if the person worked, whether for pay or not. If the person did not work last week, the question asks how many hours the person usually works at all jobs or businesses. For survey years 1997 - 2000, the question just asks for work hours if the person was working at a job or business last week.

I define hours of work as equal to the value of the hours of work variable if the mother was "working" according to the definition used throughout this paper (see Appendix Table B) and equal to zero if the mother was not working. I also limit hours of work to be less-than or equal to 80 hours.

[42]To properly estimate the marginal effect of work hours, one would need a model that allows for the large mass of zero's. Tobit models are not ideal in this instance, since it is not truncation that is occurring.

mother change from not working to working would increase a child's probability of having had an overnight hospitalization by 3.5 percentage points. This is naturally very similar to the average effect that we see in the full sample (repeated in the first row of Table 5 for reference).

5.4 Robustness Checks

The final table, Table 6, explores the robustness of the main findings to different sample selection criterion. The first row of Table 6 repeats the main results (Table 4, Row 1) for reference. The second and third rows of Table 6 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 (.0281, standard error .0119). 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 (.0259, standard error .0114). These results suggest that endogenous 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).[43] 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 larger, though less precisely measured, for children with exactly one sibling.

Row 6 in Table 6 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 (.0482).

Next I look at the difference in estimates for boys versus girls. We might expect that working has differential effects by gender if, for example, boys require more supervision than girls. In Table 6 we see that the estimated effect for boys is about twice that for girls, and that the effect is not statistically significant for girls. Understanding the mechanism by which maternal employment more strongly affects boys than girls is an interesting avenue for future research.

[43] For these two rows I do not include the "age spread" dummy variables because in the two-child families it is too highly correlated with the instrument.

The next set of rows in Table 6 presents results for children broken into three age categories (7-9, 10-12, and 13-17). Because of the restriction that the youngest sibling must be between two and eight years old, there are relatively few children in the oldest age group.[44] Interestingly, the largest effect is seen for those children in the middle age-range, 10-12 years old, with smaller and statistically insignificant effects for the youngest and oldest children. This pattern is consistent with the theory that it is *supervision* that matters. If mothers purchase child care for children in elementary school but not for children in middle or high school, then we might expect that children in middle school would be the most vulnerable to the negative health shocks. This is not directly testable in these data, since we do not observe child care arrangements or other forms of supervision and time-use.

Finally, because the data span two decades, over which the nature of maternal employment has undoubtedly changed, I disaggregate the sample by survey year. Recall that 1997 is the year in which a major survey redesign was conducted, so I divided the sample to before and after that year. Note that the mean number of women working increased from 60.5 percent in the 1985-1996 time period to 64.6 percent in the 1997-2004 period. Interestingly, the ordinary least squares estimate of the coefficient on maternal employment, reported in Column (1), is statistically significant for the 1997-2004 group. This suggests that maternal employment is associated with a .4 percentage point lower probability of having had an overnight hospitalization. The IV results are similar in both time periods, although the estimate for the 1997-2004 period is slightly larger (3.47 and 4.11 percentage points, respectively).

In all, I find strong evidence that maternal employment leads to an increase in child hospitalizations, injuries and poisonings, and asthma. I do not find statistical evidence of treatment

[44] Disaggregating the sample by the child's grade-level in school leads to very similar results. The NHIS question on grade-level asks for the child's highest grade completed, which appears to have caused some confusion for parents in how to report a child's current grade. For a complete discussion of the issues concerning how grade is measured in the NHIS, see Evans et al. (2010).

effect heterogeneity by the mother's race, education level, labor force attachment, or marital status, although this may be due to insufficient sample size.

The question of *timing* is necessarily somewhat ambiguous in this study. The instrumental variables strategy allows us to measure the effect of a mother working in the last week or last two weeks on the occurrence of a child health event in the past three or twelve months. In addition, the instrument is calculated as the youngest child's eligibility as of the beginning of the most recent school year. One additional test is to consider children whose youngest sibling was eligible for the entire past twelve months instead of at least part of the previous twelve months (the current instrument). Surprisingly, in results not show, the reduced form estimates indicate that the effect of having a youngest sibling eligible for at least part of the school year are *larger* than that of having a youngest sibling eligible for the entire past twelve months. When including the two covariates, eligible for at least part of the year and eligible for the entire past year, the former has a coefficient of .004 (.001) and the latter -.002 (.001). This implies that the strongest effects are seen for children whose youngest siblings are changing their eligibility status within the past twelve months. Therefore, the large effects of maternal employment may be at least partially due to shifting dynamics within the family. This hypothesis could be more appropriately tested using high-frequency longitudinal data.

6 Conclusion

Maternal employment might 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 a child's risk of experiencing an adverse health event, I use an instrumental variables estimation strategy. I consider 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 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. My results suggest that studying only the conditional correlation between maternal employment and child health could lead to incorrect conclusions. I find that maternal employment is an important determinant of a child's risk of experiencing an adverse health event. This result is an important contribution to our understanding of how a mother's participation in the labor force may affect her children's health in the short-run.

The results presented here do not indicate whether children experience any long-term health consequences due to maternal employment. Indeed, it may be the case that maternal employment causes an increase in these short-term health events, but on net children gain a larger health stock or develop higher cognitive abilities that lead to better health in adulthood.

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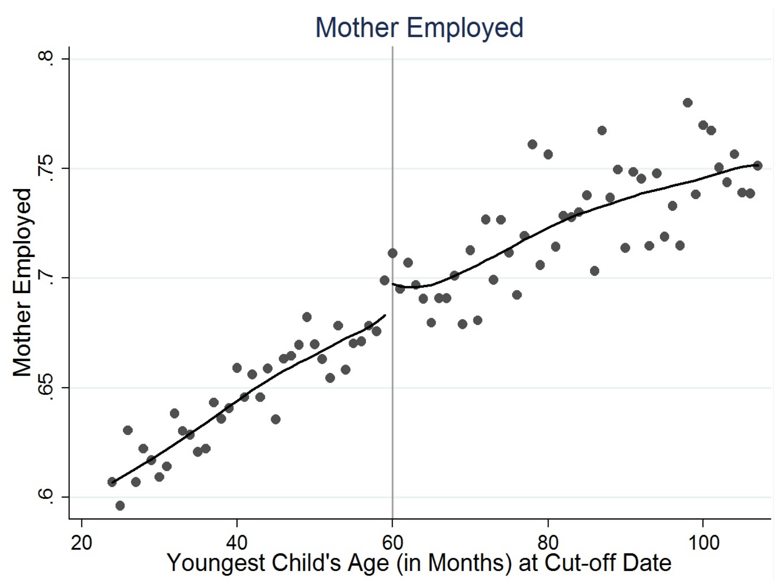
Figure 1: The Fraction of Mothers Working by Youngest Child's Age in Months



Notes: The figure shows the fraction of mothers working before and after the youngest child is eligible for kindergarten. The dots represent average maternal employment for each youngest child's age by months. Lines are from a locally weighted regression 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 = 74,120.

Figure 2: Estimation Samples for Each Health Event

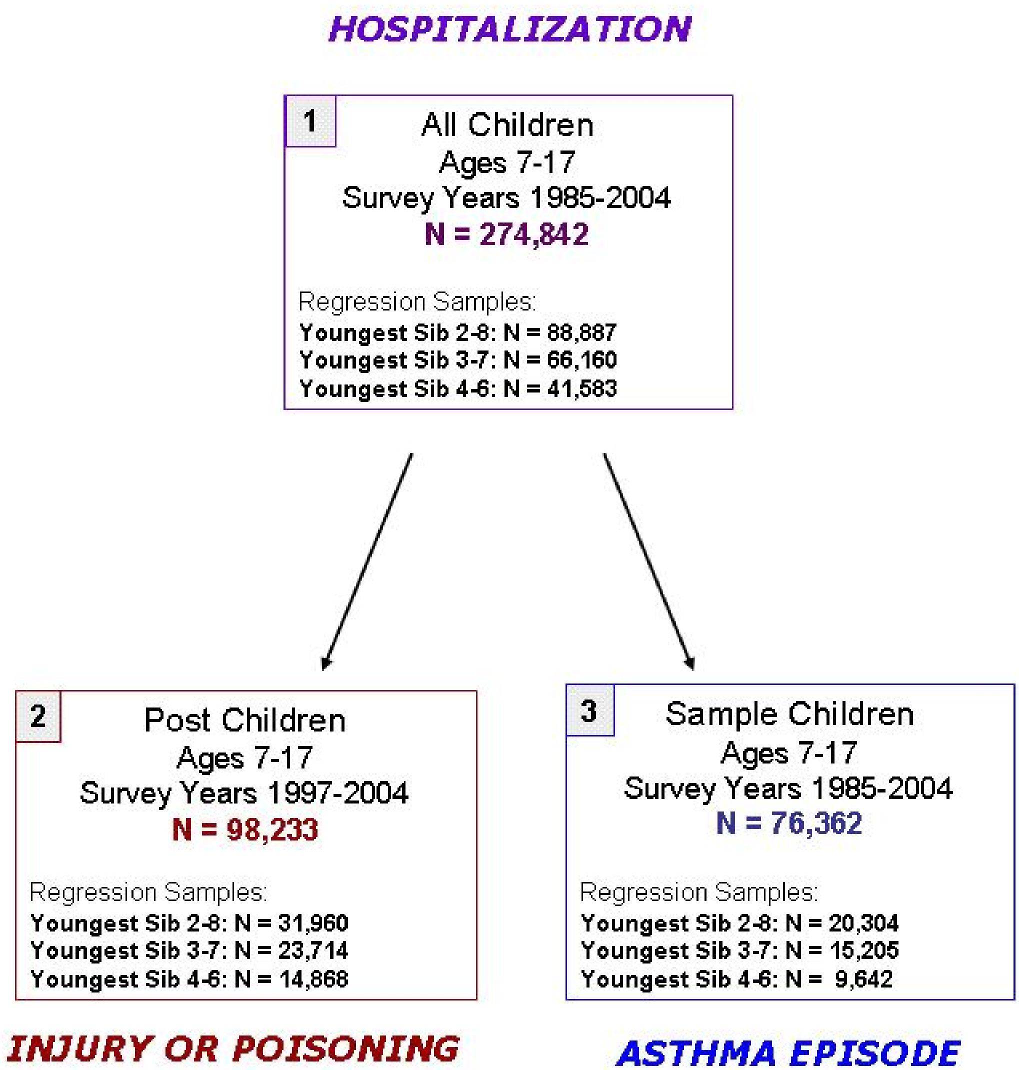


Table 1: Means by Sample

	Hospitalization		Injury or Poisoning		Asthma Episode	
	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)
Number of Obs	274,842	88,887	98,233	31,960	76,362	20,304
Had Episode	.023	.020	.031	.026	.066	.062
	(.001)	(.001)	(.001)	(.002)	(.002)	(.003)
Mother						
Mom Employed	.694	.621	.716	.644	.690	.616
	(.010)	(.013)	(.012)	(.013)	(.011)	(.013)
Num Kids	2.42	3.08	2.43	3.08	2.42	3.09
	(.026)	(.027)	(.028)	(.028)	(.027)	(.027)
Married	.798	.825	.775	.803	.798	.828
	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)
Less Than HS	.171	.189	.153	.176	.172	.190
	(.015)	(.022)	(.019)	(.027)	(.016)	(.023)
High School	.369	.359	.294	.280	.369	.356
	(.014)	(.015)	(.012)	(.013)	(.014)	(.016)
Some College	.265	.263	.324	.323	.261	.263
	(.006)	(.007)	(.006)	(.008)	(.007)	(.008)
BA/Prof Deg.	.195	.188	.229	.221	.198	.191
	(.005)	(.006)	(.008)	(.011)	(.006)	(.008)
Black	.130	.135	.127	.129	.131	.136
(Non-Hispanic)	(.014)	(.016)	(.014)	(.014)	(.014)	(.016)
White	.705	.674	.680	.643	.704	.678
(Non-Hispanic)	(.036)	(.043)	(.040)	(.048)	(.035)	(.040)
Hispanic	.120	.146	.144	.179	.120	.143
	(.034)	(.043)	(.039)	(.050)	(.033)	(.041)
Mom's Age	38.38	35.46	39.08	36.07	38.41	35.45
	(.112)	(.114)	(.130)	(.133)	(.112)	(.127)
Mom's Health	2.10	2.05	2.06	1.99	2.11	2.05
(Decr. Scale 1-5)	(.017)	(.021)	(.017)	(.025)	(.017)	(.020)
Child						
Child's Age	11.93	10.85	11.92	10.86	11.94	10.85
	(.022)	(.021)	(.026)	(.025)	(.023)	(.033)
Child Male	.511	.512	.511	.515	.511	.515
	(.001)	(.002)	(.002)	(.003)	(.002)	(.004)

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: Fraction of Children Having Each Health Event Across Groups

	Had Overnight Hospitalization (1)	Had Injury or Poisoning (2)	Had Asthma Episode (3)
Number of Obs	274,842	98,233	76,362
Full Sample	.023	.031	.066
Worked	.023 (.001)	.032 (.001)	.065 (.002)
Did Not Work	.025 (.001)	.030 (.002)	.067 (.003)
Married	.022 (.001)	.031 (.001)	.061 (.002)
Not Married	.030 (.002)	.031 (.002)	.083 (.003)
Less Than HS	.028 (.002)	.019 (.002)	.056 (.004)
High School	.025 (.001)	.028 (.001)	.063 (.002)
Some College	.023 (.001)	.036 (.002)	.070 (.003)
BA/Prof Deg.	.018 (.001)	.037 (.002)	.072 (.003)
Black (Non-Hispanic)	.025 (.001)	.018 (.001)	.078 (.004)
White (Non-Hispanic)	.024 (.001)	.038 (.001)	.065 (.002)
Hispanic	.021 (.002)	.015 (.001)	.056 (.005)
Had Hospitalization	1	.068 (.008)	.149 (.011)
Had Injury/Poisoning	.043 (.006)	1	.117 (.011)
Had Asthma Episode	.051 (.004)	.064 (.006)	1

Notes: In the top portion, each row represents a sample restricted by the child's mother's demographic characteristics as indicated. The bottom rows include children that experienced each episode as indicated. Each coefficient is the weighted mean with standard errors, clustered by state of residence, in parentheses.

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

	Health Event: Hospitalization All Children (1)	Health Event: Injury/Poisoning Post Children (2)	Health Event: Asthma Episode Sample Children (3)
Number of Obs	274842	98233	76362
Frac Had Episode	.0234 (.0010)	.0312 (.0013)	.0655 (.0016)
(1) Baseline	-.0021 (.0007)	.0016 (.0016)	-.0021 (.0029)
(2) + Survey FE	-.0024 (.0006)	.0009 (.0014)	-.0024 (.0027)
(3) + Child Demographics	-.0033 (.0006)	-.0003 (.0014)	-.0025 (.0028)
(4) + Family Structure	-.0037 (.0007)	-.0018 (.0014)	-.0059 (.0028)
(5) + Mother Demographics	-.0032 (.0007)	-.0033 (.0015)	-.0089 (.0032)
(6) Income	-.0026 (.0007)	-.0035 (.0017)	-.0089 (.0033)
(7) Mom Health	-.0015 (.0007)	-.0025 (.0016)	-.0042 (.0031)

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) includes quarter, state, and year fixed effects, Row (3) adds the child's age (dummy variables) and sex, 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 divided by health event, with the sample for which each event is defined.

Table 4: The Effects of Maternal Employment on the Probability of Having an Adverse Health Event

Health Event: Overnight 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	.0848	.0031	.0370
(Youngest Sibling 2-8)		(.0010)	(.0127)	(.0011)	(.0051)	(.0011)	(.0131)
(2) All Children	66160	.0205	.6235	-.0021	.0713	.0032	.0447
(Youngest Sibling 3-7)		(.0011)	(.0126)	(.0014)	(.0055)	(.0012)	(.0171)
(3) All Children	41583	.0211	.6243	-.0035	.0439	.0040	.0917
(Youngest Sibling 4-6)		(.0011)	(.0123)	(.0016)	(.0068)	(.0014)	(.0376)
Health Event: 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	.0723	.0041	.0562
(Youngest Sibling 2-8)		(.0015)	(.0133)	(.0020)	(.0080)	(.0020)	(.0283)
(2) Post Children	23714	.0259	.6466	-.0010	.0670	.0033	.0500
(Youngest Sibling 3-7)		(.0016)	(.0130)	(.0022)	(.0075)	(.0020)	(.0303)
(3) Post Children	14868	.0243	.6477	-.0008	.0406	.0030	.0739
(Youngest Sibling 4-6)		(.0017)	(.0127)	(.0024)	(.0088)	(.0024)	(.0602)
Health Event: 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	.0876	.0102	.1163
(Youngest Sibling 2-8)		(.0028)	(.0132)	(.0045)	(.0085)	(.0050)	(.0603)
(2) Sample Children	15205	.0636	.6161	-.0073	.0753	.0108	.1428
(Youngest Sibling 3-7)		(.0031)	(.0130)	(.0051)	(.0089)	(.0042)	(.0591)
(3) Sample Children	9642	.0643	.6134	-.0065	.0536	.0103	.1915
(Youngest Sibling 4-6)		(.0036)	(.0144)	(.0065)	(.0109)	(.0058)	(.1099)

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 Mother's Demographic Characteristics

Health Event: Overnight Hospitalization							
	N	Mean Hospital	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
All Children	88887	.020	.621	-.0018	.0848	.0031	.0370
		(.001)	(.013)	(.0011)	(.0051)	(.0011)	(.0131)
Black	13732	.022	.675	-.0045	.0533	.0075	.1404
		(.002)	(.016)	(.0034)	(.0149)	(.0034)	(.0676)
White	53386	.021	.632	-.0015	.0951	.0038	.0403
		(.001)	(.009)	(.0012)	(.0074)	(.0015)	(.0167)
Hispanic	18182	.019	.515	-.0013	.0786	.0001	.0010
		(.001)	(.019)	(.0019)	(.0092)	(.0019)	(.0248)
Mother HS or Less	52155	.022	.568	-.0030	.0834	.0036	.0434
		(.001)	(.018)	(.0018)	(.0066)	(.0019)	(.0226)
Mother Some College or More	36732	.018	.685	.00003	.0840	.0019	.0231
		(.001)	(.007)	(.0014)	(.0086)	(.0017)	(.0202)
Mother Married	71833	.019	.617	-.0002	.0864	.0024	.0272
		(.001)	(.012)	(.0009)	(.0061)	(.0011)	(.0129)
Mother Not Married	17054	.026	.639	-.0094	.0695	.0063	.0905
		(.002)	(.020)	(.0045)	(.0107)	(.0035)	(.0541)
Labor Force Attachment:							
At or Below Median LFA	44444	.021	.513	-.0012	.0909	.0039	.0425
		(.001)	(.009)	(.0016)	(.0064)	(.0015)	(.0181)
Above Median LFA	44443	.020	.713	-.0025	.0766	.0012	.0162
		(.001)	(.006)	(.0016)	(.0075)	(.0018)	(.0239)
Work Hours							
Work Hours (Linear with Zero's)	31738	.018	22.67	-.0001	2.8132	.0028	.0010
		(.001)	(.510)	(.0000)	(.3175)	(.0015)	(.0006)
Work Hours (if worked only)	19815	.017	35.351	.00001	.4468	.0003	.0006
		(.001)	(.314)	(.0001)	(.2771)	(.0023)	(.0052)

Notes: All coefficients are from linear models and represent 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 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: Robustness Checks

Health Event: Overnight Hospitalization							
	N	Mean Hospital	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
All Children	88887	.020	.621	-.0018	.0848	.0031	.0370
		(.001)	(.013)	(.0011)	(.0051)	(.0011)	(.0131)
Child Not Limited	82372	.016	.624	-.0006	.0864	.0024	.0281
		(.001)	(.013)	(.0008)	(.0053)	(.0010)	(.0119)
No Kids Limited	74661	.016	.631	-.0002	.0872	.0023	.0259
		(.001)	(.013)	(.0008)	(.0059)	(.0010)	(.0114)
2 Child Family	30006	.021	.703	-.0024	.0455	.0039	.0852
		(.001)	(.010)	(.0019)	(.0052)	(.0018)	(.0397)
3+ Child Family	58881	.020	.576	-.0014	.0773	.0024	.0316
		(.001)	(.014)	(.0013)	(.0069)	(.0012)	(.0159)
Mom in Very Good or Excellent Health	58530	.018	.652	.0005	.0876	.0042	.0482
		(.001)	(.011)	(.0012)	(.0078)	(.0013)	(.0149)
Boys	45475	.021	.620	-.0004	.0927	.0042	.0451
		(.001)	(.013)	(.0017)	(.0064)	(.0016)	(.0178)
Girls	43412	.019	.621	-.0031	.0767	.0019	.0251
		(.001)	(.013)	(.0015)	(.0073)	(.0017)	(.0222)
Ages 7-9	33632	.019	.597	-.002	.0889	.0012	.0133
		(.001)	(.012)	(.0017)	(.0067)	(.0016)	(.0188)
Ages 10-12	30184	.017	.632	-.0035	.0962	.0056	.0580
		(.001)	(.014)	(.0017)	(.0075)	(.0028)	(.0296)
Ages 13-17	25071	.026	.639	.0003	.058	.0017	.0285
		(.001)	(.014)	(.0022)	(.0078)	(.0027)	(.0467)
1985-1996	56917	.022	.605	-.0003	.0918	.0032	.0347
		(.001)	(.014)	(.0012)	(.0066)	(.0015)	(.0161)
1997-2004	31970	.018	.644	-.0043	.0723	.003	.0411
		(.001)	(.013)	(.0016)	(.0079)	(.0014)	(.0212)

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. Note that in the third and fourth row the "age spread" dummies are not included, as explained in footnote **??**.

Appendix Table A: Kindergarten Eligiblity Cutoff Dates for 1983 and 2004

Approximate Cut-off Date	States 1983-1984 School Year	States 2004-2005 School Year
July 1		IN
August 1		MO
September 1	AZ, FL, KS, MN, MO, MS ND, NM, OK, SD, WA, WV WI	AL, AK, AZ, DE, FL, GA ID, IL, KS, MN, MS, NM ND, OK, OR, RI, SC, SD TX, UT, WA, WV, WI
September 15	IA, MT, WY	AR, IA, MT, WY
October 1	AL, AR, KY, NV, OH, VA	KY, LA, MD, NV, OH, TN VA
October 15	ID, ME, NE, NC	ME, NE, NC
November 1	AK, SC, TN	
November 15	OR	
December 1	CA, IL, MI	CA, MI
January 1	CT, DE, DC, HI, LA, MD RI, VT	CT, DC, HI
No State-Wide Law	CO, IN, GA, MA, NH, NJ NY, PA, TX, UT	CO, MA, NH, NJ, NY, PA VT

Notes: This table shows the general trend towards earlier cutoff dates. Cutoff dates are rounded for ease of presentation. For a full list of state-by-year cutoffs used see Evans et al. (2010). Data were acquired from individual state statues.

Appendix Table B: Key Variable Definitions

<table>
<tr><td>Variable</td><td>1985-1996 Surveys</td><td>1997-2004 Surveys</td></tr>
<tr><td rowspan="2">Mother Worked</td><td>Employment Status in Past TWO WEEKS</td><td>Doing LAST WEEK</td></tr>
<tr><td colspan="2">Equals 1 if worked, 0 if did not work, dropped otherwise</td></tr>
<tr><td>Youngest Child's Age</td><td colspan="2">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).</td></tr>
<tr><td>Kindergarten Eligibility</td><td colspan="2">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: The health outcomes span the past 3 or 12 months, but contemporaneous school eligibility is used.</td></tr>
<tr><td>Overnight Hospitalizations</td><td>Derived from the number of short stay hospital episodes in the past year.

Defined for all children</td><td>Derived from the number of hospital stays.

Defined for all children</td></tr>
<tr><td>Injury/Poisoning</td><td>Not available</td><td>The child had an injury or poisoning episode in the past 3 months

Defined for all children</td></tr>
<tr><td>Asthma Episode</td><td>During the past 12 months, did ___ have... *Asthma?

Children in familes assigned to condition list 6</td><td>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</td></tr>
</table>

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

Health Event: Overnight Hospitalization							
Panel A:	N	Mean Hospital	Mean Work	Probit (1)	First Stage (2)	Reduced Form (3)	IV BiProb (4)
(1) All Children (Youngest Sibling 2-8)	88887	.0203 (.0010)	.6205 (.0127)	-.0017 (.0010)	.0906 (.0055)	.0028 (.0010)	.0246 (.0084)
(2) All Children (Youngest Sibling 3-7)	66160	.0205 (.0011)	.6235 (.0126)	-.0018 (.0013)	.0762 (.0059)	.0028 (.0011)	.0211 (.0073)
(3) All Children (Youngest Sibling 4-6)	41583	.0211 (.0011)	.6243 (.0123)	-.0032 (.0014)	.0472 (.0074)	.0037 (.0013)	.0263 (.0105)
Health Event: Injury/Poisoning							
Panel B:	N	Mean Injury	Mean Work	Probit (1)	First Stage (2)	Reduced Form (3)	IV BiProb (4)
(1) Post Children (Youngest Sibling 2-8)	31960	.0262 (.0015)	.6438 (.0133)	-.0008 (.0019)	.0770 (.0081)	.0034 (.0019)	.0256 (.0134)
(2) Post Children (Youngest Sibling 3-7)	23714	.0259 (.0016)	.6466 (.0130)	-.0010 (.0020)	.0714 (.0078)	.0029 (.0018)	.0424 (.0185)
(3) Post Children (Youngest Sibling 4-6)	14868	.0243 (.0017)	.6477 (.0127)	-.0009 (.0021)	.0436 (.0092)	.0027 (.0021)	X (X)
Health Event: Asthma Episode							
Panel C:	N	Mean Asthma	Mean Work	Probit (1)	First Stage (2)	Reduced Form (3)	IV BiProb (4)
(1) Sample Children (Youngest Sibling 2-8)	20304	.0624 (.0028)	.6164 (.0132)	-.0066 (.0042)	.0941 (.0089)	.0092 (.0047)	.0491 (.0150)
(2) Sample Children (Youngest Sibling 3-7)	15205	.0636 (.0031)	.6161 (.013)	-.0063 (.0047)	.0814 (.0095)	.0097 (.0038)	.0556 (.0180)
(3) Sample Children (Youngest Sibling 4-6)	9642	.0643 (.0036)	.6134 (.0144)	-.0058 (.0059)	.0590 (.0119)	.0095 (.0052)	.0580 (.0283)

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. An "X" indicates that the model did not converge.

Appendix Table D: Full Specifications for Main Results, Compare to Table 4, Row 1

	OLS	First Stage	Reduced Form	IV 2SLS
Dependent Variable	Hospital Episode	Mother Worked	Hospital Episode	Hospital Episode
	(1)	(2)	(3)	(4)
Mother Worked	-.0018			.0370
	(.0011)			(.0131)
Youngest Eligible		.0848	.0031	
		(.0051)	(.0011)	
Child Male	.0021	-.0046	.0021	.0023
	(.0009)	(.0042)	(.0009)	(.0009)
Mother Married	-.0072	-.0151	-.0071	-.0065
	(.0013)	(.0136)	(.0013)	(.0014)
Mother Less Than High School	.0033	-.2558	.0035	.0130
	(.0017)	(.0142)	(.0017)	(.0039)
Mother High School Degree	.0040	-.0756	.0039	.0067
	(.0013)	(.0085)	(.0013)	(.0017)
Mother Some College	.0029	-.0284	.0028	.0038
	(.0015)	(.0142)	(.0015)	(.0016)
Mother Black	-.0034	.0639	-.0036	-.0059
	(.0018)	(.0146)	(.0018)	(.0022)
Mother Hispanic	-.0012	-.0028	-.0012	-.0011
	(.0018)	(.0161)	(.0018)	(.0020)
Mother Other Race/Ethnicity	-.0029	.0316	-.0029	-.0041
	(.0021)	(.0121)	(.0021)	(.0022)
Number of Observations:	88887			

Notes: All specifications also include the following controls: Child's Age (11 categories), Number of Children (4 categories), Mother's Age Category (5 categories), State Fixed Effects (51 categories), Year Fixed Effects (20 categories), Interview Quarter (4 categories), and the Age Spread equal to the oldest child's Age minus the youngest child's age (9 categories)