ABSTRACT

Title of dissertation: WOMEN'S LABOR SUPPLY AND THE FAMILY

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The past century has seen a tremendous rise in female labor force participation. My dissertation addresses aspects of how the American family has shaped and has been shaped by rising levels of female labor supply. The first chapter provides an introduction and discussion. The second chapter describes the impact of maternal employment on children's health. While most prior research has found little effect, I argue that a woman's choice to work may reflect unobservable characteristics of the mother or child which complicates the measurement of the causal effect. I utilize exogenous variation in each child's youngest sibling's eligibility for kindergarten as an instrument for maternal employment. I find robust evidence that maternal employment increases a child's probability of having had an overnight hospitalization, injury or poisoning, or asthma episode.

The third and fourth chapters analyze two possible sources of increased female labor force participation. In the third chapter, co-authored with Judith Hellerstein, we consider the role that fathers play in their daughters' occupational choices.

We demonstrate that over the past century fathers have increasingly transmitted occupation-specific human capital to their daughters in response to the changing opportunities for women in the labor market.

In the fourth chapter, I investigate work first published by Fernandez et al. (2004) and find evidence that contradicts their central conclusions. Their paper suggests a mechanism by which working mothers endow sons with a preference for having a working wife, which in turn leads women to choose to work more in order to attract these men. The key empirical results in their paper show a strong conditional correlation between a woman's labor supply and that of her mother-in-law when her husband was young and no similar relationship between a woman's labor supply and that of her own mother. While I confirm the former relationship in my own analysis, I find that a woman's choice to work is also highly correlated with her own mother's labor supply. While their model provides an interesting hypothesis for women's motivation to work, I find that the data do not support their conclusions.

WOMEN'S LABOR SUPPLY AND THE FAMILY

by

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Dedication

I would like to dedicate this work to my husband, Thayer Morrill. Without his support and encouragement I never would have began, let alone finish, my dissertation.

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Chapter 1

Introduction

The labor force participation rate of women rose dramatically throughout the latter half of the twentieth century. According to a Bureau of Labor Statistics report,[1] about 43 percent of women ages 16 and older were in the labor force in 1970. This number rose to 60 percent by the late 1990s. Furthermore, 47 percent of mothers with children under age 18 worked in 1975, compared with a labor force participation rate of 73 percent in 2000. Concurrent with these trends, women were increasingly likely to be employed in higher paying occupations, to have higher contribution rates to total family income, and to pursue higher levels of education and professional degrees.

Such dramatic changes in women's labor force participation have transformed women's role in society and in the family. There are many questions that arise from considering women's labor force participation. My dissertation addresses three aspects of how female labor supply relates to the American family and reflects on both causes and effects of the rising rate. The second chapter considers an important consequence of female labor force participation: the effects of maternal employment on children's health. The third and fourth chapters discuss intergenerational aspects of female labor force participation. Chapter 3 investigates the role of fathers in

[1] Chao, Elaine L. and Philip L. Rones, (2006), "Women in the Labor Force: A Databook," *U.S. Department of Labor* and *U.S. Bureau of Labor Statistics*, September 2006, BLS Report 996.

shaping the occupational choices of their daughters, while Chapter 4 considers how mothers' employment shapes the preferences of their sons (and daughters).

In the second chapter I ask whether maternal employment is harmful to children's health. Many policies in the United States and elsewhere have been aimed at bringing women into the workplace. Most studies have found very little effect of maternal employment on children, and some have even found a positive effect. However, it is complicated to directly measure the effects since mothers may make labor supply decisions based upon their own (unobserved) preferences and skills and on the characteristics of their family. This is of particular concern when we consider child health. A child's health could directly affect a mother's labor supply if women respond to having a child in poor health by reducing (or increasing) their labor supply. I implement an instrumental variables empirical strategy, which overcomes the bias introduced by this reverse relationship. I utilize exogenous variation in maternal labor supply from kindergarten eligibility laws. Gelbach (2002) established that a child's kindergarten eligibility increases maternal employment. For school-age children with at least one younger sibling, I use each child's youngest sibling's eligibility for kindergarten as an instrument for maternal employment.

Ideally, I would like to measure the children's underlying health stock and to understand if and how this stock is affected by maternal employment. It would be particularly interesting to investigate whether any change in this health stock has long-term consequences for children. However, the empirical strategy and data source I employ do not allow for any longitudinal analysis or for the measurement of pure "underlying health." Instead, I use four proxies for health: overnight hospitalizations, emergency room visits, injuries/poisonings, and asthma episodes. While none of these measures are perfect individually, together they tell a compelling story that maternal employment raises a child's risk of suffering from a negative health event.

The effect sizes I find are quite large, indicating that having a mother that works increases hospitalizations, asthma episodes, or injuries/poisonings each by around 200 percent. Emergency room visits are only measured for a small subset of my sample, and these estimates are never statistically significant. I perform a long series of specification checks and search for any evidence of a non-representative local average treatment effect. My findings are robust, and I find little evidence of heterogeneous effects across different types of families.

The empirical strategy that I employ relies on the change in labor supply at the time when a woman's youngest child becomes eligible for kindergarten. It is possible, and even likely, that it is the change in labor supply that has such large consequences for children, rather than employment generally. As mentioned above, ideally we would like to measure the consequences of maternal employment on the long-term health of children. It could be the case that when women start working, there is a period of adjustment within the family that creates a temporary spike in adverse health events that subsides over time. It would be interesting to further explore this "transition" hypothesis and to then compare the estimates to the previous literature.

And, finally, health is just one aspect of children's well-being that may be affected by a mother's choice to work. While I do not consider other outcomes for children in this work, the same omitted variable concern plagues studies of the effects of maternal employment in other contexts. Investigating other measures of children's well-being using a similar empirical strategy would be an interesting and important extension of this work.

The third chapter of my dissertation, which is co-authored with Judith Hellerstein, looks at a different familial relationship. We consider whether the rise in female labor force participation has led fathers to increasingly transmit occupation-specific human capital to their daughters. We hypothesize that as women are increasingly likely to work, fathers face an increased incentive to invest in their daughters, since the daughters are more likely to use that investment in the labor force.

We frame this question by asking whether women are increasingly more likely to enter their father's occupation over birth cohorts from 1909 and 1977. However, women are more likely to be entering any man's occupation over this time period. Our approach utilizes a woman's father-in-law as a "counterfactual." This choice is motivated by a theoretical model that combines features of intergenerational human capital transmission, occupational choice, and assortative mating. The basic idea is that a woman's father-in-law works in a set of occupations that are "close" to her father's occupations, and represent a set of occupations that a woman could have or might have chosen given endowed characteristics such as social class or preferences. We compare the trend across birth cohorts in the probability a woman works in her father's occupation with the trend in the probability a woman works in her father-in-law's occupation. We find strong evidence suggesting that fathers have increasingly transmitted occupation-specific human capital to their daughters.

In the fourth chapter, I analyze a different intergenerational relationship: the influence that mothers have on the preference formation of their sons (and daughters). I investigate work first published by Fernandez et al. (2004) and find evidence that contradicts their central conclusions. They present a model whereby a woman's participation in the labor force is partially determined by her mother-in-law's work experience. The men in the model have preferences for the working behavior of their wife. These preferences are formed through the man's mother's work experience by mothers having endowed their sons either with skills that make them better partners for a working woman or with preferences for a working wife. Women in the model acquire market skills in order to attract this growing subset of men that prefer working women. The model has dynamic implications, since the new incentive for wives to work yields more mothers that work, creating more sons who will eventually want working wives. The wives' preferences are not addressed and, in particular, the wife's own mother's work experience is shown empirically to have no independent impact on the labor force participation of the wife.

While the empirical results presented in Fernandez et al. support this story, I present empirical evidence that directly contradicts this finding. I demonstrate that the results in Fernandez et al. are highly sensitive to small changes in specification. I utilize an additional data set and confirm that there is a strong and statistically significant relationship between a woman's labor supply and her own mother's work experience, which is not consistent with the Fernandez et al. model.

There are alternative models that could explain the large coefficient on mother-in-law's work experience in the regression of a woman's labor supply on maternal employment and observable characteristics. If there is assortative mating between men and women along characteristics of their families, it seems very likely that a husband and wife were raised by mothers with similar work behavior. What is so surprising about the results in Fernandez et al. is that the coefficient on mother-in-law's work experience is not diminished when a woman's own mother's work experience is included in the regression. The key identifying assumption in Chapter 3 of this work is that assortative mating by fathers' occupation has not decreased over time. Although we are not able to test this directly, Chapter 3 provides evidence consistent with this. Given this evidence, and a large literature on assorative mating (see, e.g., Mare 1991, Rose, 2001, and Lam and Schoeni, 1993), FFO's choice not to model assortative mating directly is surprising. Future work will be aimed at developing an alternative model of female labor force dynamics and assortative mating, similar to that proposed by Lam and Schoeni (1993).

The empirical results presented in this chapter suggest that assortative mating is an important component to understanding the relative correlations between mothers and daughters and mothers-in-law and daughters-in-law work behavior. I find strong evidence that the empirical results presented in Fernandez et al. are not robust, and additional empirical results suggest that there are significant flaws in their model and interpretation. I find evidence consistent with assortative mating along mothers' labor supply, which suggests that an alternative model may better explain the empirical findings.

The third and fourth chapters of my dissertation both consider the role that one generation has in the labor supply decisions of women in the next generation.

The empirical support we find in chapter 3 suggests that fathers played an increasingly important role in the labor supply decisions of their daughters throughout the twentieth century. One limitation of this paper is that we do not address the transmission of human capital from mothers to daughters. We do not consider this transmission because it was rare for the women in our sample to have mothers that worked, and information on mother's occupation was not available from all three of our data sources. However, the evidence presented in chapters three and four together suggest a strong role for both fathers and mothers in the occupational choices and labor supply decisions of their daughters.

There are many important and interesting questions related to the work in this dissertation. We are only beginning to understand how the rise in the labor force participation rate of women has been influenced by and is affecting families. In recent years a debate has erupted in popular press, dubbed "The Mommy Wars," that considers the relative benefits of mothers staying home to care for children and going to work and earning income. While my work is only one piece of a large literature considering the costs of maternal employment, it does highlight an important methodological complication that is not widely recognized. This paper provides evidence that there is a large health consequences of maternal employment once the endogeneity of the labor supply decision is accounted for.

The latter two chapters consider the transmission of preferences and skills from parents to children. Both chapters suggest that parents do play an important role in the formation of their children's preferences for working. As more women are raised by working mothers, it will be interesting to consider their occupational choices and career paths. We are left with many important questions to consider. For example, how will daughters (and sons) be influenced differently by the careers of mothers versus fathers? And, as women become more economically independent and men take on more responsibilities in child-rearing and housework, will higher labor force attachment strengthen or weaken the family in the long-run?

The growth in female labor force participation, from 43 percent in 1970 to 60 percent in 1999, slowed and even saw a modest drop by 2005 to 59.3 percent.[2] It is possible that the negative consequences of maternal employment, as found in Chapter 2, are beginning to be recognized by women. Chapter 4 documents a modest correlation between maternal employment and a daughter's choice to work as an adult. It might be the case, as is often suggested in the "Mommy Wars" debate, that the daughters of working women, after observing the struggles of their mothers, chose to leave the labor force when they themselves have families. If working mothers endow their daughters with skills for working but preferences for not working, then the net effect on daughters' eventual occupational choices is theoretically ambiguous. As more data become available for recent years, it will be interesting to study these and other hypotheses for the recent decline in female labor force participation.

[2]Chao, Elaine L. and Philip L. Rones, (2006), "Women in the Labor Force: A Databook," *U.S. Department of Labor* and *U.S. Bureau of Labor Statistics*, September 2006, BLS Report 996.

Chapter 2

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

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

2.1 Introduction

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

The direction and magnitude of the effect of maternal labor supply on child health is theoretically ambiguous. The clearest mechanism through which maternal employment might positively impact children is through an increase in family income. There is a well established income-health gradient, which has been shown to exist for children as well as adults (see Case, Lubotsky, and Paxson, 2002, and Currie and Lin, 2007). More income allows families to increase investments in health

[1]Dietz, 1997.

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

[3]Lleras-Muney, 2006.

for their children, including better diet and better health care. In addition, some mothers acquire or improve their family's health insurance coverage due to their employment. However, maternal employment imposes a burden on a mother's time and may result in the poorer supervision or care of her children. A child's health is at least partially a function of time-intensive activities such as healthy meal preparation and house cleaning. A working mother may have less time to allocate to these types of activities. Bianchi (2000) shows that working mothers spend less time doing housework, and Crepinsek et al. (2004) document that children of working mothers have lower overall "Healthy Eating Index" scores. In addition, a child whose mother works may be left unsupervised or less-supervised more often than if the mother were at home full-time.

Previous studies on the effects of maternal employment find little measurable impact on child health, as discussed further in Section 2.2. Empirical identification of the effect is difficult because a mother's choice to participate in the labor market is endogenous. Maternal employment has often been considered as the effect of, not the cause of, the family's characteristics.[4] Mothers with healthy children may find it easier to work, whereas mothers of children with special needs may find it

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

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

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

In the absence of a perfect measure of underlying child health, I analyze the effects of maternal employment on four health outcomes: overnight hospitalizations, emergency room visits, asthma episodes, and injuries and poisonings. These outcomes capture both acute and chronic conditions. As argued in Section 2.3.3, while none of these outcomes alone are perfect, when taken together they provide a reasonable proxy for child health. Together the effects of maternal employment on these four health episodes, presented in Section 2.5, provide compelling evidence of an increase in the probability a child experiences an adverse health event.

Consistent with much of the existing literature, I find that the conditional correlations between maternal employment and each of the child health episodes, as estimated using ordinary least squares regressions, are zero or negative. That is, having a working mother is associated with a lower risk of a child having the health incident. Because of the endogeneity of maternal employment, however, these correlations do not necessarily represent a causal relationship. In this paper, I use an instrumental variables strategy where the instrument relies on the fact that the opportunity cost of a woman working is substantially lowered when her youngest child becomes eligible for public school, potentially leading to an increase in maternal labor supply at that time. I measure the health of children ages seven through seventeen years old that have at least one younger sibling. I further restrict the estimation sample to children whose youngest sibling is within a specified age range around five years old. I use each child's youngest sibling's eligibility for kindergarten as an instrument for maternal labor supply in assessing the causal impact of maternal labor supply on the health of the older child. As discussed further below, Gelbach (2002) established that a child's eligibility for kindergarten, as measured by quarter of birth, increases maternal employment. I argue that a child's youngest sibling's eligibility for kindergarten provides variation in maternal employment that is plausibly exogenous to the older child's health. Nonetheless, in Sections 3 and 5 I provide discussions of the potential biases associated with this instrument. I also explore whether there is treatment effect heterogeneity across major demographic categories, and I discuss the generalizability of the estimated local average treatment effect measured by the instrumental variables strategy.

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

The remaining sections of this paper are organized as follows. Section 2.2 reviews the relevant literature. Section 2.3 outlines the empirical specifications used and discusses issues related to the validity of the instrument. In Section 2.4 I describe the data and key variables. Section 2.5 discusses the empirical results and Section 2.6 concludes.

2.2 Related Literature

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

Most closely related to this paper, Ruhm (2004) uses the National Longitudinal Survey of Youth (NLSY) to analyze the effect of maternal employment on a cohort

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

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

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

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

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

2.3 Empirical Specification and Methodology

2.3.1 Econometric Models

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

$$CHealth_i = \alpha + \beta MLS_i + \gamma X_i + \epsilon_i \tag{2.1}$$

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

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 lease 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:

$$MLS_i = \alpha_{FS} + \beta_{FS} Z_i + \gamma_{FS} X_i + \mu_i \tag{2.2}$$

The consistency of the estimate of β relies on the validity of the instrument ($Cov(Z, \epsilon) = 0$). If Z is uncorrelated with ϵ, then the instrumental variable estimate of β is consistent. This is fundamentally an untestable assumption. Although I do consider below how violations of this assumption would affect my results, as long as the instrument, Z, is uncorrelated with ϵ, the model can be estimated by taking the predicted (fitted) value of MLS from Equation (2.2) and substituting it in for MLS in Equation (2.1) in a two-stage least squares model (2SLS). The instrumental variable estimate of β can also be thought of as resulting from the division of the "reduced form" estimate, β_{RF} below, by the first-stage coefficient derived above, β_{FS}. The reduced form equation is the regression of the child health outcome on the instrument:

$$CHealth_i = \alpha_{RF} + \beta_{RF} Z_i + \gamma_{RF} X_i + \sigma_i \tag{2.3}$$

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 my case maternal employment. This is discussed further in Section 5.[7]

All four child health outcomes I present are dichotomous variables, taking a value of one if the child experienced the health episode and zero otherwise. Models with binary dependent variables require special consideration, since the two-stage least squares (2SLS) estimate described above assumes that the dependent variable in the second stage equation is continuous. As is well known, estimates from linear models with binary dependent variables may be a poor approximation when the dependent variable has a very low (or very high) mean (Bhattacharya et al., 2006). Angrist (1999) argues that, in most cases, the 2SLS estimate is a reasonable estimation strategy with limited dependent variables and a dichotomous endogenous variable. With some assumptions about the distribution of the error terms (i.e., that both are distributed bivariate normal), a bivariate probit model can be specified.

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

Because of the strong functional form assumptions, the bivariate probit estimates are more precise. However, as Angrist (1999) argues, these estimates are potentially biased if the functional form assumptions are not correct. Estimating all specifications with probit and bivariate probit models lead to similar results. I include the non-linear version of the main regression results as Appendix Table 2.11; non-linear results for all other tables are available upon request. The marginal effects from the bivariate probit model confirm the conclusions from the two-stage least squares estimates. Future work will explore the sensitivity using other limited dependent variable estimation strategies.

2.3.2 The Instrument: Youngest Sibling's Kindergarten Eligibility

My exogenous instrument is motivated by the observation that the opportunity cost of a woman's time is substantially lowered when her youngest child becomes eligible for public school. In the United States, kindergarten is provided free of charge through public schools for all children ages five or older. By 1983 (the first school year in my data) all states provided kindergarten, but individual states determine by what date a child must turn five years in order to be eligible to enroll in the current school year. The school year usually begins some time around the beginning of September. There is a fair amount of variation across states in this eligibility cut-off date and many states changed their policies over my study period. Appendix Table 2.9 demonstrates this variation for the first and last school year of my sample, 1983 versus 2004. Notice both that many states moved their cut-off date earlier during the 20 years of my sample, so children had to be somewhat older when entering school in the later periods, and that September 1st remains the modal cut-off date. Some states allow the local educational authorities (LEAs) to determine their own cut-off date, as indicated in the bottom row of Appendix Table 2.9. I use state and year specific cut-off dates where available and assume a September 1st cut-off date for states with no standard date; however, results are not sensitive to including only states with a standard cut-off date.[8,9]

One key to the success of the instrumental variables strategy is identifying an instrument with sufficient predictive power. A child's eligibility for kindergarten has been found to predict maternal labor supply in several studies to date (Gelbach, 2002, and Cascio, 2006).[10] To confirm this relationship for my sample, in Figures 2.1 and 2.2 I illustrate the basic relationship between a mother's youngest child's age and her likelihood of employment. For these graphs, I use the full sample of mothers

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

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

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

in the restricted National Health Interview Survey (NHIS) from years 1985-2004,[11] regardless of the number and ages of her children (or child), and each mother is represented only once in this sample. In these graphs, I consider only those mothers whose youngest child's exact eligibility could be determined, dropping observations from states where the local education authority determines the cut-off date (N = 89,317).

First, Figure 2.1 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 cut-off date faced by that child. The dots in Figure 2.1 represent the fraction of mothers that were employed and fractional polynomial interpolation was used to produce the smoothed curves on either side of 60 months. A clear increase in maternal employment occurs when the youngest child achieved 60 months (exactly 5 years) by the cut-off date. Figure 2.2 instead plots average maternal employment by the youngest child's age in months on September 1st of the most recent school year. Children in states with cut-off dates at the beginning of September, October, and December are included separately. So, for example, children who live in Kentucky face an October 1st cut-off date, so must have turned 59 months by September 1st to be eligible for kindergarten in the current school year. The curve with the break at 59 months includes only mothers who live in states with an October 1st cut-off date. These figures each provide suggestive evidence that kindergarten eligibility raises the probability a mother works, which is confirmed in estimates of β_{FS} presented in

[11] For more information on the data, see Section 2.4.

Section 2.5.[12]

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

All preferred specifications include the number of children present in the household and the mother's age as covariates, which should minimize the potential of a spurious correlation between maternal employment and child health through endogenously determined fertility. Furthermore, since all children ages seven and older

[12]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 (2006) utilize state variation in kindergarten eligibility laws to instrument for a child's age at school entry and provide compelling evidence that kindergarten eligibility does lead to kindergarten enrollment.

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

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

2.3.3 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.[13] Because true underlying health cannot be measured, I instead use four proxies for health which capture both chronic and acute conditions: overnight hospitalizations, emergency room visits, asthma episodes, and injuries and poisonings. Each of these measures is likely reflecting a health event that is unambiguously bad, unlike, for example, having had a doctor's visit. Visiting the doctor could indicate adequate access to care and healthy, preventative behaviors. Although each health event has its own strengths and weaknesses, my analysis over all four health outcomes provides strong evidence that maternal employment does increase a child's risk of having an adverse health event. It should be noted that this does not necessarily imply that a child's long-term health is adversely affected, but rather simply that the child experiences a negative health episode due to maternal employment.

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

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

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.[14] Many of these conditions may be sensitive to supervision, regular access to care, and access to appropriate medication and preventative treatment.[15] 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 (see, e.g., Goodman et al., 1994), indicating that having had a hospitalization may reflect access to care in addition to true differences in morbidity. I provide evidence suggesting that this is not a major source of bias in my estimates.

The second health event I consider is whether the child had an emergency room visit in the past 12 months, a more common event than overnight hospitalizations. The leading causes of ER visits are similar to those for hospitalizations, but often reflect more acute health events. However, having had an ER visit may reflect both inadequate access to care in a doctor's office and true emergencies.[16]

[14]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.

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

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

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.[17] 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.[18] 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).[19] If an employed mother is less able to adequately monitor adherence to medication or is not able to

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

[18]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.

[19]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).

respond promptly to an asthma attack, then maternal employment should increase a child's risk of having an overnight hospitalization, an emergency room visit, or an asthma episode. However, regular access to care and the ability to purchase appropriate medication to control asthma may reduce the incidence of having an asthma episode. In addition, one widely recognized risk factor for asthma is indoor allergens (see, for example, Lanphear et al., 2001). Bianchi et al. (2002) find that mothers who work spend significantly less time on housecleaning. Maternal employment could lead to more residential exposure to allergens for children and hence more asthma episodes.

The final health 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.

2.4 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 the extensive data requirements of this project. The NHIS is a repeated cross-section survey which has been conducted annually in the United States since 1957. The restricted-access version of the NHIS includes state of residence identifiers, which allow for the more precise measurement of whether the youngest child was eligible for kindergarten.[20] I combine data from survey years 1985 - 2004.[21] A major survey instrument redesign occurred in 1997, so some variables are only defined in the "post redesign" sample. I define the analysis variables as consistently as possible across years and especially between survey designs, however, an indicator for whether the observation was drawn from "pre-" versus "post-" survey redesign is included in all relevant regressions. See Appendix Table 2.10 for a description of how the key variables are defined across survey periods.[22]

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

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

[22]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.

Because of the NHIS survey design, each of the four health events I explore is defined for a different, nested sample of children. Figure 2.3 provides a diagram illustrating the relationship between these samples. The first sample, "All Children," consists of all children from survey years 1985-2004 ages seven through seventeen years who were part of the primary family and whose mother was between eighteen and sixty-four years old. Children whose mother could not be identified within the household or who had missing values for any key variable are excluded, yielding 274,842 children in the pooled sample, as indicated in Figure 2.3 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, my sample is comprised of a "pre" and "post" redesign period. In the post-redesign period, 1997-2004, respondents are asked whether the child had an injury or poisoning in the past three months.[23] I denote Sample 2 in

[23]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.

Figure 2.3 as the "Post Children," indicating that these are all children ages seven to seventeen in survey years 1997 - 2004. Sample 3 in Figure 2.3 is also a subset of Sample 1, referred to as the "Sample Children." In the pre-redesign survey years, 1985-1996, families were randomly assigned one out of six condition lists. The respondent was asked whether each family member had the conditions or episodes on their assigned list. For my third outcome measure, asthma, I include children from families that were asked whether each child had asthma in the past 12 months (condition list 6). In 1997 the survey was redesigned and, rather than ask about one list of conditions for every family member, the respondent was asked detailed health information about one randomly selected "sample child" from the family. A Sample Child Supplement is provided for approximately one child in every family and asks whether the sample child had an asthma episode in the past 12 months.[24] Sample 3 in Figure 2.3 indicates that the "Sample Children" are the subset of the full sample that were asked the asthma question, N = 76,362.

Finally, Sample 4 in Figure 2.3 denotes the children in the post-redesign period who were given the Sample Child Supplement described above, N = 44,838. Information on emergency room visits in the past 12 months was only formally collected in the Sample Child Supplement in the post-1997 survey redesign period.[25] I will present results for each child health outcome on this sample, so that the effects

[24]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.

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

can be compared for a consistent sample. However, there are obviously many fewer observations in Sample 4, which will limit the power to make inferences.

Since the main results are necessarily specified on a sample of families with two or more children within specified age ranges, one may be concerned that the results are not readily generalizable. Within the four different samples described above, I next consider how similar the children in my main estimation samples are to all children having information about the outcome of interest. In Table 2.1, I compare the demographic characteristics of these groups. The column numbers of Table 2.1 correspond to the sample numbers in Figure 2.3.

Table 2.1 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 aged seven to seventeen years will be represented more than once in the sample. In the regression results to follow, all standard errors are clustered by state of residence to account for potential correlations in the error terms introduced by the NHIS sample design and the state-level nature of my instrument, and from including siblings in the regressions. Nearly 70 percent of children had mothers who worked. The average age for mothers is 38 years old, and almost 80 percent of the mothers in the sample are currently married (this includes mothers who are remarried). The average age for children is 12 years and there are slightly more boys than girls in the sample. Table 2.1 Column (1b) restricts the sample to children with at least one younger sibling whose youngest sibling was between ages 2-8 years (24 - 107 months) at the scheduled interview date. Further restricting the sample around age 5 does not change things qualitatively, so these samples are omitted for presentational clarity. Notice that the full and restricted samples are very similar. Because of the mechanical relationship between having a sibling and family size, the number of children in the family is larger when I restrict to the sample of children with at least one younger sibling. The mothers are over 2 years younger on average and slightly less likely to be white in Column (1b) relative to Column (1a), presumably because fertility rates are lower among whites.

Moving across the columns in Table 2.1, I show that the characteristics of the samples for each health outcome are very similar. Comparing between the column panels, we see that, when restricting the sample to children where each health outcome is reported, the samples remain representative, as expected from the NHIS sample design. Similar to the findings in Columns (1a) and (1b), we see in the remaining columns that children with at least one younger sibling are more likely, on average, to have mothers who are married, less educated, and younger. They are more likely to be minorities (especially Hispanic). When each sample is restricted to children with at least one younger sibling, the fraction of mothers employed drops. As is explored in more detail below, mothers with more children are less attached to the labor market. Therefore the sample of children with at least one younger sibling is not representative of the full sample along some dimensions. This should be kept in mind when considering the generalizability of the key findings to the full population of children.

Table 2.2 reports the fraction of children that had each health episode. The columns indicate which health episode. 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. The fraction of children experiencing each health episode is larger for the not-working sample for all health outcomes except for injuries and poisonings. This foreshadows the negative coefficient in the ordinary least squares specifications in Table 2.3, indicating that children are less likely to have had a health episode if their mother worked.

The next set of rows divides the sample by the marital status of the mother. Notice that children living with not-married mothers are more likely to experience all health events except injuries and poisonings. Similarly, the next set of rows divides the sample by the mother's education level. While hospitalizations and ER visits are relatively more common for children whose mothers had less than a college degree, injuries/poisonings and asthma episodes are more likely among children with higher educated mothers. Finally, dividing the sample by race we see that blacks are more likely to have had an asthma episode or an emergency room visit, while whites are more likely to report having had an injury or poisoning.

The bottom panel of Table 2.2 restricts the sample to the Post Sample Redesign Children, to allow for a more direct comparison between the health outcomes. Looking across the columns in the bottom panel of Table 2.2, 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) or an ER visit (37 percent). Children that had an injury or poisoning in the past three months were nearly twice as likely to have been hospitalized in the past 12 months than the full sample; however, only four percent were hospitalized overnight. The means in the bottom panel indicate that these measures are reflecting some underlying morbidity, each with varying levels of severity and incidence.

2.5 Empirical Results

2.5.1 OLS Estimates

Comparison of the means in Table 2.2 suggested that, unconditionally, maternal employment is associated with a slight decrease in the incidence of hospitalization, asthma, and ER visits, and a slight increase in injuries or poisonings. In Table 2.3 I explore how this relationship changes once demographic characteristics and other controls are included, before presenting my main IV results.

The cells of Table 2.3 report the coefficient on maternal employment from separate ordinary least squares regressions.[26] Note that since each episode is considered a *negative* health outcome, a negative coefficient on maternal employment implies working benefits child health. Each column in Table 2.3 represents a different sample. The sample in Table 2.3 Column (1) contains all children ages seven to seventeen (Figure 2.3, Sample 1) and Column (2) includes sample children seven through seventeen in the post-redesign survey years (Figure 2.3, Sample 4). Columns

[26]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.

(3)-(7) contain analogous samples for the other three health outcomes, as shown in Figure 2.3. The specifications on the post-redesign sample (Figure 2.3, Sample 4) allow for the inclusion of more extensive control variables and allow for a better comparison across outcome measures. All regressions include year fixed effects to control for differences in question wording.

The rows of Table 2.3 successively add covariates to explore the sensitivity of estimates of the relationship between maternal employment and child health. Row (1) of Table 2.3 presents the basic relationship between maternal employment and each health episode, with an indicator for "pre-" or "post-" redesign year. In Column (1) Row (1), the coefficient implies that maternal employment lowers the probability that a child had an overnight hospitalization by .2 percentage points, a statistically significant effect. In Columns (3) through (7), the probability a child had each health event is not statistically significantly related to maternal employment. Row (2) adds interview quarter,[27] state, and year fixed effects. Row (3a) adds dummy variables for the child's age and an indicator for the child's sex. Row (3b) adds an indicator for child having had low birth weight, which is only available in the post 1997 redesign Sample Child surveys.[28] 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

[27]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.

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

spread between the oldest and youngest child present in the family.[29] Finally, Row (5) adds dummies for mother's age (18-24, 25-29, 30-34, 35-39, 40-64),[30] mother's education (less than high school, high school, some college, or BA/Professional Degree), mother's race/ethnicity (black, white, Hispanic, other). In the preferred (most saturated) model, reported in Row (5) of Table 2.3, the estimated relationship between maternal employment and the child health episode are negative and significant for hospitalizations, asthma, and ER visits (injuries/poisonings is only sometimes significant), implying that maternal employment *reduces* the probability that a child has a negative health outcome.

Income and health insurance are two mechanisms through which maternal employment may plausibly impact child health. As such, including these variables as controls will not allow for the full effect of maternal employment to be measured. However, it is interesting to consider whether 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.[31] In the bottom panel, Row (6) adds income dummies to the specification in

[29]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 Table 2.8 Section 3 where the sample is disaggregated by family size.

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

[31]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

Row (5) and their is little change in the coefficients.

Maternal health is another source of potential bias, as is illustrated in Table 2.2. The health measure is a decreasing scale from 1 to 5, where 1 indicates a self-report of excellent health and 5 indicates poor health (i.e., a higher number for average health implies worse health). Notice that, looking across the columns of Table 2.2, the sample of children that had each health episode had mothers with worse health on average. A mother in poor health may not work due to her health problems and may have children in poorer health (or may be more inclined to report her children being in poorer health), inducing a spurious positive correlation between maternal employment and good child health. However, it may also be the case that being employed affects a mother's health and through this channel also affects the child's health. If this were the case, including maternal health as a covariate would "over control" and would not allow for the full measurement of the effects of employment on health. Adding to the specification in Row (5), Table 2.3 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 2.3 demonstrates that the conditional correlations between maternal

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.

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

2.5.2 Causal Estimates

Table 2.4 presents the main results of this paper. Each coefficient represents the results from separate regressions, so a total of 60 regressions are summarized in this Table. Each regression includes maternal, family, and child demographic characteristics and state, year, and quarter fixed effects that parallel the specification in Table 2.3 Row (5) (with standard errors clustered by state).[32] 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. The first three rows report estimates for all children (Figure 2.3, Sample 1), while the fourth row provides estimates for the post-redesign sample children (Figure 2.3, Sample 4). I include results for the post-redesign sample children to allow for comparisons between health measures within a consistent sample.

Table 2.4 Column (1) reports the coefficient from the OLS regression of maternal employment on child health, corresponding to Equation (2.1) in Section 2.3.

[32]The estimates in Tables 2.4-2.8 do not employ survey weights. Estimates using survey weights are very similar.

These estimates are directly comparable to Row (5) of Table 2.3. These estimates differ slightly because of sample construction; in Table 2.4 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 2.3, the estimates are less precise. For example, in Table 2.3 Column (1) Row (5), the all children sample, the OLS estimate is -.0032 (.0007). This estimate can be compared to Table 2.4 Column (1) Row (1), the all children "with youngest sibling 2-8" sample, OLS estimate of -.0018 (.0011). The validity of the instrument relies on restricting the sample in this way, though at the cost of a substantial reduction in sample size and a resulting loss of power. Note again that a negative coefficient on maternal employment implies that a child whose mother worked has a *lower* risk of having had a bad health episode.

The coefficient of interest in Equation (2.2), as described in Section 2.3, is β_{FS}, the effect of the instrument on maternal employment. These "first stage" estimates are presented in Column (2) of Table 2.4. The effect of kindergarten eligibility is large and significant for all regressions, suggesting that the instrument has predictive power. Column (3) of Table 2.4 reports the coefficient from the "reduced form" regression, where the coefficient of interest is the effect of the instrument on the health outcome. The reduced form coefficients are consistently positive, but are only statistically significant for hospitalizations, asthma, and injuries or poisonings and only in the largest samples. For example, in Table 2.4 Column (3) Row (1) the estimated effect is .0033 (.0011), indicating that the youngest child's eligibility for kindergarten raises the risk of the older child having been hospitalized by .33

percentage points. In all rows the estimates point toward a similar finding: the kindergarten eligibility of the child's youngest sibling increases the risk the child has a bad health episode. These results are particularly important when interpreting the overall findings.

If the instrument does not completely satisfy the validity assumption (i.e., the instrument may be correlated with the error term in Equation (2.1)), the reduced form results still give a direct measure of the correlation between the youngest child's eligibility for kindergarten and negative health consequences for the older child. I argue that the predominant mechanism through which kindergarten eligibility should affect elder sibling health is through the mother's labor supply, but this interpretation is not testable, at least not in the current data. As an alternative, it is possible that maternal effort toward the older child increases with the youngest child's kindergarten eligibility, thus leading to better health for the older child. If this effect dominated, I would find a *negative* coefficient on the instrument in the reduced form, indicating that the instrument was good for child health. On the other hand, it might be the case that eligibility affects the level of supervision of the child. For example, a mother that works could cease to purchase formal child care for her children when her youngest child ages into kindergarten and instead rely on her older children to supervise her younger children after school. In this example, we might expect to see an increased probability of bad health events for the older child when the younger child ages into kindergarten eligibility. The change in health in this example is still theoretically an effect of maternal employment, but it does confound the interpretation of the instrumental variable estimate. I explore this possibility further in Sections 5.3 and 5.4.

Table 2.4, Column (4) presents the instrumental variable estimates using 2SLS. For computational and expositional simplicity, I include only the 2SLS estimates in this table.[33] 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 2.4 presents the effects of maternal employment on children's overnight hospitalizations. In Column (4) the IV effects are large and statistically significant in all rows. The estimate in Row (1) indicates that a mother working increases the probability of overnight hospitalization by approximately 4 percentage points, or just under 200 percent. When the sample is further restricted in Rows (2) and (3) the estimate is much less precise, but is still statistically significant. Using the Post Sample children in Panel A, Row (4) indicates a similar effect size, although the coefficient is no longer statistically significant. Overall, the results in Panel A suggest a reasonably robust relationship where maternal employment increases a child's probability of having an overnight hospitalization, contrary to the OLS relationship. The remaining tables explore the robustness of this effect, breaking down the sample in Row (1).

Turning now to the second health outcome, Panel B of Table 2.4 presents anal-

[33]Appendix Table 2.11 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 2.4. The fourth column presents estimated marginal effects from a bivariate probit model. As described in Section 3, this model relies on strong functional form assumptions. Notice that the estimates are smaller in magnitude and much more precise, but the qualitative results are consistent. Future work will further explore the robustness of the results to estimation using these and other limited dependent variable models.

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

The probability of having had an asthma episode, Panel C in Table 2.4, again demonstrates a positive effect. In Row (1), Column (4), the coefficient implies that maternal employment causes an 12 percentage point increase in the probability of having an asthma episode. Again, this corresponds to just under a 200 percent increase. The effect is statistically significant in the three largest sample, but the magnitudes become very large and the estimates are imprecise. Finally, Panel D in Table 2.4 explores the effect of maternal employment on ER visits. The IV estimates are not statistically significant for ER visits, but the results are qualitatively similar to those from the other health outcomes. The "Post Sample," that used in Row (4) in Panels A, B, and C, and in all of Panel D, does not have sufficient sample size to produce statistically significant estimates. However, the results are similar in magnitude and always large and positive.

Table 2.4 provides evidence that maternal employment negatively affects children's health. The point estimates are large in magnitude, indicating that, in the largest estimation sample, maternal employment raises the probability of overnight hospitalization, injury or poisoning, and asthma by just under 200 percent each.

These are large effects. For example, having had an asthma episode raises the probability of having had a hospitalization by roughly 3.3 percentage points, compared with the estimated effect of a 3.9 percentage points increase due to maternal employment. I explore the robustness of these estimates in the subsequent sections.

2.5.3 Heterogeneous Effects and the Local Average Treatment Effect

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

Because hospitalizations are defined for the largest sample, and therefore have sufficient observations to break down the sample along various dimensions, in all subsequent tables I focus on the effect of maternal employment on overnight hospitalizations for children ages seven through seventeen whose youngest sibling was between 24 and 107 months (2-8 years) at the scheduled interview date. In the first row of Table 2.5, I reproduce the results from the first row of Panel A in Table 2.4, 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 2.5 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 2.5, suggest that white mothers are more likely to begin working after their youngest child ages into kindergarten eligibility (.0951) than black mothers (.0507), although the baseline probability of working is 68 percent for blacks compared to 63 percent for whites. Next we notice that the reduced form estimates for blacks are larger than for whites (.0073 for blacks versus .0039 for whites), although the difference is not statistically significant. Column (4) presents the instrumental variable results, indicating that maternal employment causes a 14.5 percentage point increase in the risk of overnight hospitalizations for the children of black women. This estimate is very large in magnitude, but is imprecise. For white mothers, it is estimated that employment increases hospitalizations by 4.1 percentage points. Both IV estimates are statistically significant and indicate that maternal employment increases child hospitalizations for black and white mothers. The estimates for Hispanic mothers are not statistically significant.

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

The third panel of Table 2.5 presents the effects decomposed by marital status. The "not married" sample consists of any woman not currently married, whether widowed, divorced, or never married, and also includes women who are separated from their husbands. The probability of having an overnight hospitalization is over 25 percent higher for the not-married mothers sample (.026 versus .019) and that sample shows a much stronger relationship between maternal employment and child health in the OLS specification (-.0094 versus -.0002) in Column (1). Note that the probability of working is very similar in these two samples. The instrumental variable estimate in Column (4) suggests that maternal employment has a larger effect on overnight hospitalizations for not-married mothers (.0952) as compared to married mothers (.0292), although the coefficient for not-married mothers is not statistically significant.

The next set of rows explores the treatment effect heterogeneity by the age of the mother. In all the empirical results presented in this paper, I exclude from the sample mothers younger than 18 or older than 64. I do this for two reasons. First, matching children to mothers is complicated in the data and occasionally children are miscoded as spouses (and vice versa) in the raw data. Restricting the sample to women 18 to 64 eliminates many instances of siblings or grandmothers being miscoded as mothers. In addition, women outside of the 18 to 64 age range are more likely to be in school or to be retired, complicating the interpretation of employment. In the next set of rows in Table 2.5 I further disaggregate the sample by maternal age. In doing this, I am also able to consider whether endogenous fertility is spuriously affecting the instrument, since women above 40 are less likely to have any more children. Women younger than 40 may remain not-employed because they are pregnant or are trying to become pregnant, so these women are not an appropriate comparison group for employed women of the same age and number of children. As mentioned above, the number of children is a strong predictor of employment and may be correlated with child health, so I include controls for mother's age and number of children in all specifications to mitigate any potential biases. Table 2.5 shows that the effect of maternal employment is only somewhat heterogeneous across categories of maternal age. The only age groups with statistically significant effects are mothers who are 25 - 29 and 30 - 34, where the 2SLS coefficients are .1284 (.0692) and .0476 (.0228), respectively.[34] The estimates for the other age groups are smaller and less precise. Therefore the different estimated effects and the lack of statistical significance for some maternal age groups appears to be an artifact of a limited sample size and not of heterogeneous effects along this dimension.

To explore how access to care may be confounding the results, I compare the effects of maternal employment for children with public versus private health insurance. Health insurance is defined most consistently for survey years 1998-2004, therefore in the bottom set of rows I focus on children from these survey years. The estimated effect of maternal employment on hospitalizations for this sample is slightly larger and less precise than that for the full sample, indicating that maternal employment increases hospitalizations by 5 percentage points in these

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

survey years. The subsequent three rows estimate the effects for populations of children with differing health insurance types. First I present results for children with any health insurance. Then I break this sample into two groups: those with private health insurance and those with public health insurance.[35] As discussed in the Introduction, children with public health insurance are more likely than those without health insurance and with private health insurance to receive treatment in a hospital setting (see, e.g., Currie and Gruber, 1996). In my data, the mean rate of overnight hospitalizations is almost twice as large for children with public health insurance versus private health insurance (2.8 versus 1.5 percent). This could be due to a higher disease burden in this population or due to characteristics of reimbursement that lead families to seek care in hospitals. Notice also that maternal employment rates are lowest for children with public health insurance and highest for children with private health insurance. To measure the full effect of maternal employment, we would like to allow health insurance to be a mechanism through which maternal employment affects child health. Estimating the different effects of maternal employment by the child's health insurance types allows the exploration of the effects of maternal employment on child health without confounding the positive aspects of an increase in access to care, although there may be selection into health insurance types based upon child health status.

Although the IV estimates for children with public health insurance are imprecise (.0465, standard error .0646), the effects of maternal employment among

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

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

In all, the estimates in Table 2.5 indicate that there may be some heterogeneity in the treatment effects along major demographic categories. However, the effects of maternal employment are consistently measured as being bad for children's health. This exercise in exploring treatment effect heterogeneity does not eliminate the possibility that the women whose labor supply is affected by the instrument (the compliers) are actually subsets of women within each category presented in Table 2.5. Though the consistency of the estimates across different portions of the population suggest that there is not a great deal of treatment effect heterogeneity, it may still be the case that the coefficient in the instrumental variable estimate is measuring the effect of maternal employment for a very specific, and potentially non-representative, sample of women. To explore this further, I disaggregate the sample in an alternative way, which may be a better approximation to subsets more or less affected by the instrument.

I construct an index of labor force attachment (LFA) and break down the sample by this index. This analysis is similar to that of Kling (2001), who uses the family background index in Card (1995) in order to determine how the instrumental variables estimate of the return to schooling differs across quartiles of family background. Because LFA varies in important ways by race (for example, black women have much higher employment rates and Hispanic women have much lower employment rates, as compared to whites), I restrict the sample to white mothers for this exercise.[36]. I calculate a labor force attachment index from the full sample of mothers with at least one child between the ages of zero through seventeen from the NHIS pooled survey (years 1985-2004). I calculate the probability the mother works from a linear probability model on year, state, and quarter dummies, and maternal age, education level, marital status, and number of children, as defined above. I use the coefficients from this regression to predict the labor force attachment for all white mothers. I then divide the regression sample (i.e., children ages seven through seventeen years old with at least one younger sibling, whose youngest sibling was between ages 2 - 8 at the scheduled interview date) into four quartiles based on their LFA score. This procedure yields over 13,000 children in each quartile.

Table 2.6 presents sample means for each Labor Force Attachment (LFA) quartile. Notice that the women most highly attached to the labor market have fewer children on average, have more education, are less likely to be married, and are older on average. ER visits occur most frequently for the lowest quartile while injury or poisoning episodes are reported most frequently for those in the highest quartile. To confirm that the LFA scale is a good approximation for actual probability of employment, I compare the probability of maternal employment across the four quartiles. Less than half of the mothers in the lowest quartile worked (48.5 percent),

[36]Repeating this exercise with the full sample and including controls for race leads to very similar results. The comparative estimates for Table 2.7 Column 5 are included in footnote 37.

as compared to 76 percent of mothers in the highest quartile, indicating that LFA is reflecting a true difference in likelihood of employment. The probability of working is one way to measure labor force attachment; another way is through the intensity of work or hours worked. However, hours of work are only measured in the post-redesign survey years, 1997-2004. Table 2.6 demonstrates the relationship between work intensity and my LFA index. The first work-hours measure includes hours of work for women that reported working last week. We see only a slightly higher average number of hours worked for the most attached women. When I impute zero hours of work for women who reported not working last week, there is a strong gradient in hours worked for the four LFA quartiles.

In Table 2.7 I present results disaggregated by LFA. The first row of Table 2.7 repeats the results from the third row of Table 2.5, the estimated effect on hospitalizations for the sample of children with white mothers. The second set of rows in Table 2.7 splits the sample by mothers above and below the median LFA. Notice that while the overall probabilities of having a hospitalization are very similar (around 2 percent), the mean of maternal employment is over 16 percentage points higher for children whose mother is in the top half of the LFA scale (71 versus 55 percent). The first stage estimates in Column (2) indicate that the instrument does affect labor supply in both groups, though the effect is larger for the less attached mothers. The reduced form in Column (3) is only significant for less attached sample (.5 percentage points), indicating a large increase in hospitalizations due to the instrument. Likewise, the instrumental variable estimates of the effect of maternal employment on child hospitalizations (Column 4) are positive, large, and statistically significant for the less attached sample only. Although the effect for the most attached women is not statistically significant, the standard error is large and the difference in the coefficients for the more and less attached women are not statistically significant. Indeed, when the sample is broken down further into the four quartiles, the effects of maternal employment are statistically insignificant for all four quartiles, but are always positive in sign.[37]

The estimates in Table 2.7 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. Future work will explore the sensitivity of the results to using measures of work intensity and to outliers. Unfortunately the NHIS does not include data on

[37] In results not shown, the estimates for the full sample of children (not restricting on race) are very similar. The low LFA group has a point estimate of 0.0313 (0.0184) and the high LFA group has a point estimate of 0.0351 (0.0266). Full results available upon request.

child care or supervision. However, failure to find differences along the labor force attachment scale provides some evidence that this is not a major source of bias in these results.

2.5.4 Robustness Checks

The final table, Table 2.8, explores the robustness of the main findings to different sample selection criterion. The first row of Table 2.8 repeats the main results (Table 2.4, Row 1) for reference. Again, these estimates are for all children ages seven to seventeen in survey years 1985-2004 that have at least one younger sibling whose youngest sibling was between 24 and 107 months at the scheduled interview date. The second and third rows of Table 2.8 explore the possibility that the youngest child's exact age is correlated with the older child's health in a way that is biasing the IV estimates. For example, one might be concerned that the spacing of births is influenced by the health of the older child. Park et al. (2003) look at the extreme case of severe child disability and mothers' tubal sterilization and find that having a severely disabled child only increases the probability of seeking sterilization for mothers who already have one non-disabled child. This evidence suggests that the change in timing of births due to having a disabled child may vary with birth order, but it is likely not a strong effect. I first eliminate all children from the sample that are reported as having an activity limiting disability. This restriction removes children that were recently injured or are still recovering from a debilitating disease, for example, so it should understate the negative consequences of maternal employment. With this restriction, the estimated effect of maternal employment declines, but still implies a large and statistically significant effect (.0304, standard error .0123). Further restricting the sample to children whose *siblings* are also not limited has only a minor additional effect on the coefficients and is still statistically significant (.0288, standard error .0117). These results suggest that birth spacing is not driving the results.

The next set of rows divides the sample by total family size. I present results for children with exactly one sibling and for children with two or more siblings (i.e., mothers with 2 children versus mothers with 3 or more children).[38] 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 2.8 explores the sensitivity of the results to the mother's own health. Employment may harm a mother's health (due to physical strain, stress, etc), which could be a mechanism through which maternal employment affects child

[38]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. When the age-spread dummies are included, the estimates in Table 2.8 Column 4 are 0.1437 (0.0938) for 2 child families and 0.0306 (0.0135) for 3 or more child families.

health. Therefore, I have so far not controlled for maternal health. However, it may also be the case that the effect of maternal employment is different for mothers with different health statuses and that mothers respond to the instrument differentially by health status. For example, mothers with severe health conditions may remain out of the labor force, even when their youngest child is in school. Restricting the sample to children with mothers who are reported as having very good or excellent health does not change the qualitative results, but the estimated effect is now somewhat larger (.0525).[39]

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 2.8 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.

Finally, because I look at the health of school age children ages seven to seventeen but allow the sample of youngest siblings to be between two and eight years, I test specifications restricting the sample to children ages nine to seventeen. Though the sample size is reduced, the results on this sample are qualitatively similar and statistically significant. These results indicate that maternal employment increases child hospitalizations by 4.6 percentage points. The final row of Table 2.8 further

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

restricts to children between nine and twelve. It may be that maternal employment is more (or less) harmful for children nine to twelve as compared to teenagers. Also, in the data girls above age thirteen have an increased risk of hospitalizations. This could be due to an increase in mental health issues, such as eating disorders, or due to childbearing. Although I only include in my sample children that report being the child of the household head or the spouse of the household head, the pathways from maternal employment to child health may be different for girls at risk of pregnancy. Note that I include the child's age and sex in all specifications, so a higher risk of hospitalizations should not bias the results. In the bottom row of Table 2.8 we find that effects of maternal employment are even stronger for the sample of children ages nine to twelve (.0500).

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

2.6 Conclusion

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

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

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

The main results are robust to a host of specification checks. Although the estimates are not statistically significant in all cases, the signs and magnitudes are consistent. 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 return to the labor force may affect her children.

2.7 Appendix

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

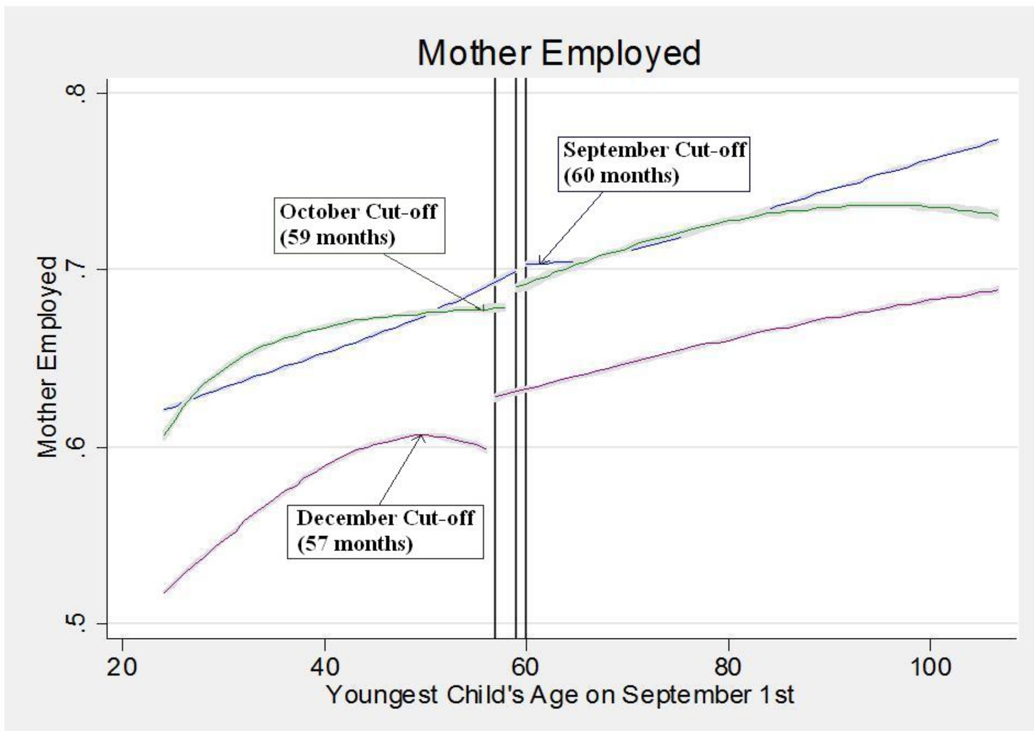
In Appendix Table 2.12 I provide a calculation of weights in my data, similar to those in Kling (2001). For this entire exercise I restrict my attention to the regression sample for overnight hospitalizations (Sample 1 in Figure 2.3) where the child has at least one younger sibling and whose youngest sibling was between 2 - 8 years at the scheduled interview date. I further restrict the sample to white mothers, since the characteristics associated with labor force attachment vary by race/ethnicity. The labor force attachment index (LFA) is discussed in the main text. Column (1) of Appendix Table 2.12 gives the fraction of the sample in each quartile, $w_q = P(Q)$, where Q is the quartile. Column (2) gives the conditional variance of the instrument $\lambda_{1|x} = E[P(Z|X,Q)(1-P(Z|X,Q))|Q]$, where Z is the instrument, X are the covariates, and Q is the quartile. The impact of the instrument on the probability the mothers work is given by $\Delta MW_{q|x} = E[E(MW|Z=1,X,Q) - E(MW|Z=0,X,Q)|Q]$. Column (4) shows the overall weight of each quartile in the two-stage least squares regression, $\omega_{q|x} = \frac{(w_q \lambda_{q|x} \Delta MW_{q|x})}{\sum_q (w_q \lambda_{q|x} \Delta MW_{q|x})}$.

In Column (4) of Appendix Table 2.12, we see that the most weight is given to women in the lowest quartile of LFA, though the weights are very similar. Most importantly, the highest quartile receives the least amount of weight, 18 percent versus 30 percent. This table provides evidence that the group of "compliers" is not disproportionally from one particular quartile.

Figure 2.1: A: The Fraction of Mothers Working by Youngest Child's Age in Months

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

Figure 2.2: B: The Fraction of Mothers Working by Youngest Child's Age in Months



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

Figure 2.3: Estimation Samples for Each Health Outcome

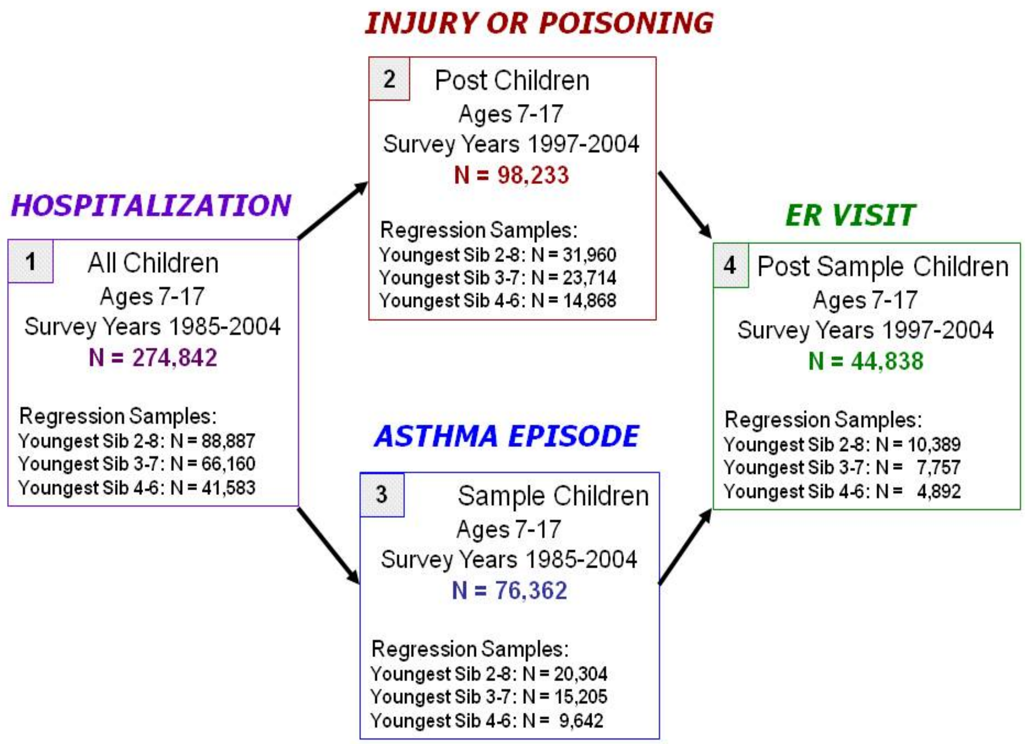


Table 2.1: Means by Sample

	Health Outcome: Hospital Episode		Health Outcome: Injury or Poisoning		Health Outcome: Asthma Episode		Health Outcome: ER Visit	
	All Child (1a)	Has Sib 2-8 Yrs (1b)	Post Child (2a)	Has Sib 2-8 Yrs (2b)	Sample Child (3a)	Has Sib 2-8 Yrs (3b)	Post Sample (4a)	Has Sib 2-8 Yrs (4b)
Number of Obs	274,842	88,887	98,233	31,960	76,362	20,304	44,838	10,389
Had Episode	.023	.020	.031	.026	.066	.062	.179	.167
	(.001)	(.001)	(.001)	(.002)	(.002)	(.003)	(.004)	(.005)
Mother								
Mom Employed	.694	.621	.716	.644	.690	.616	.714	.638
	(.010)	(.013)	(.012)	(.013)	(.011)	(.013)	(.012)	(.014)
Num Kids	2.42	3.08	2.43	3.08	2.42	3.09	2.41	3.06
	(.026)	(.027)	(.028)	(.028)	(.027)	(.027)	(.026)	(.028)
Married	.798	.825	.775	.803	.798	.828	.772	.803
	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)	(.007)
Less Than HS	.171	.189	.153	.176	.172	.190	.150	.173
	(.015)	(.022)	(.019)	(.027)	(.016)	(.023)	(.018)	(.027)
High School	.369	.359	.294	.280	.369	.356	.290	.276
	(.014)	(.015)	(.012)	(.013)	(.014)	(.016)	(.011)	(.013)
Some College	.265	.263	.324	.323	.261	.263	.328	.329
	(.006)	(.007)	(.006)	(.008)	(.007)	(.008)	(.006)	(.008)
BA/Prof Deg.	.195	.188	.229	.221	.198	.191	.231	.222
	(.005)	(.006)	(.008)	(.011)	(.006)	(.008)	(.008)	(.012)
Black	.130	.135	.127	.129	.131	.136	.127	.129
(Non-Hispanic)	(.014)	(.016)	(.014)	(.014)	(.014)	(.016)	(.014)	(.015)

continued on next page

Table 2.1: *continued*

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
White	.705	.674	.680	.643	.704	.678	.687	.652
(Non-Hispanic)	(.036)	(.043)	(.040)	(.048)	(.035)	(.040)	(.040)	(.047)
Hispanic	.120	.146	.144	.179	.120	.143	.139	.172
	(.034)	(.043)	(.039)	(.050)	(.033)	(.041)	(.038)	(.050)
Mom's Age	38.38	35.46	39.08	36.07	38.41	35.45	39.05	36.02
	(.112)	(.114)	(.130)	(.133)	(.112)	(.127)	(.135)	(.146)
Mom's Health	2.10	2.05	2.06	1.99	2.11	2.05	2.05	1.98
(Decr. Scale 1-5)	(.017)	(.021)	(.017)	(.025)	(.017)	(.020)	(.017)	(.027)
Child								
Child's Age	11.93	10.85	11.92	10.86	11.94	10.85	11.89	10.81
	(.022)	(.021)	(.026)	(.025)	(.023)	(.033)	(.026)	(.033)
Child Male	.511	.512	.511	.515	.511	.515	.508	.516
	(.001)	(.002)	(.002)	(.003)	(.002)	(.004)	(.003)	(.006)

Notes: Coefficients are weighted sample means. Standard errors are clustered by state of residence and are included in parentheses. Each column reflects the samples displayed in Figure 2.3, as described in the text.

Table 2.2: Fraction with Health Event by Population Characteristics

	Had Hospital	Had Injury/ Poisoning	Had Asthma Episode	Had ER Visit
	(1)	(2)	(3)	(4)
Number of Obs	274,842	98,233	76,362	44,838
Full Sample	.023	.031	.066	.179
Mother Worked	.023	.032	.065	.176
	(.001)	(.001)	(.002)	(.004)
Mother Did Not Work	.025	.030	.067	.185
	(.001)	(.002)	(.003)	(.007)
Married	.022	.031	.061	.163
	(.001)	(.001)	(.002)	(.004)
Not Married	.030	.031	.083	.232
	(.002)	(.002)	(.003)	(.007)
Less Than HS	.028	.019	.056	.208
	(.002)	(.002)	(.004)	(.014)
High School	.025	.028	.063	.185
	(.001)	(.001)	(.002)	(.007)
Some College	.023	.036	.070	.184
	(.001)	(.002)	(.003)	(.005)
BA/Prof Deg.	.018	.037	.072	.145
	(.001)	(.002)	(.003)	(.005)
Black	.025	.018	.078	.208
(Non-Hispanic)	(.001)	(.001)	(.004)	(.009)
White	.024	.038	.065	.178
(Non-Hispanic)	(.001)	(.001)	(.002)	(.004)
Hispanic	.021	.015	.056	.160
	(.002)	(.001)	(.005)	(.010)
Had Hospitalization	1	.068	.149	.719
		(.008)	(.011)	(.015)
Had Injury/Poisoning	.043	1	.117	.674
	(.006)		(.011)	(.016)
Had Asthma Episode	.051	.064	1	.365
	(.004)	(.006)		(.015)
Had ER Visit	.081	.123	.122	1
	(.004)	(.004)	(.005)	

Notes: See Table 2.1 and text.

Table 2.3: Ordinary Least Squares Estimates of the Effect of Maternal Employment on Child Health

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

Notes: Each coefficient is from a separate regression of the child health measure on maternal employment, standard errors (clustered by state) are in parentheses. The rows add covariates successively: Row (1) includes an indicator for post-redesign, Row (2) includes quarter, state, and year fixed effects, Row (3) adds dummy variables for child's age and sex, Row (3b) adds an indicator for low birth weight, Row (4) adds mother's marital status (married or not), the number of children (1-4, 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.

Table 2.4: The Effects of Maternal Employment on Child Health

	N	Mean Outcome	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
Panel A:	**Health Outcome: Hospitalization**						
(1) All Children	88887	.0203	.6205	-.0018	.0842	.0033	.0388
(Youngest Sib 2-8)		(.0010)	(.0127)	(.0011)	(.005)	(.0011)	(.0133)
(2) All Children	66160	.0205	.6235	-.0021	.0706	.0033	.0474
(Youngest Sib 3-7)		(.0011)	(.0126)	(.0014)	(.0054)	(.0012)	(.0173)
(3) All Children	41583	.0211	.6243	-.0035	.0433	.0042	.0974
(Youngest Sib 4-6)		(.0011)	(.0123)	(.0016)	(.0068)	(.0014)	(.0388)
(4) Post Sample	10389	.0163	.6382	-.0071	.0721	.0030	.0417
(Youngest Sib 2-8)		(.0013)	(.0136)	(.0028)	(.0107)	(.0032)	(.0455)
Panel B:	**Health Outcome: Injury/Poisoning**						
(1) Post Children	31960	.0262	.6438	-.0008	.0736	.0038	.0510
(Youngest Sib 2-8)		(.0015)	(.0133)	(.0020)	(.0081)	(.0020)	(.0268)
(2) Post Children	23714	.0259	.6466	-.0010	.0684	.0030	.0443
(Youngest Sib 3-7)		(.0016)	(.0130)	(.0022)	(.0079)	(.0020)	(.0292)
(3) Post Children	14868	.0243	.6477	-.0008	.0428	.0026	.0596
(Youngest Sib 4-6)		(.0017)	(.0127)	(.0024)	(.0094)	(.0024)	(.0545)
(4) Post Sample	10389	.0278	.6382	.0015	.0721	.0022	.0306
(Youngest Sib 2-8)		(.0021)	(.0136)	(.0038)	(.0107)	(.0049)	(.0676)

continued on next page

Table 2.4: *continued*

	N	Mean Outcome	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
Panel C:	**Health Outcome: Asthma Episode**						
(1) Sample Children	20304	.0624	.6164	-.0072	.0856	.0103	.1203
(Youngest Sib 2-8)		(.0028)	(.0132)	(.0045)	(.0085)	(.005)	(.0611)
(2) Sample Children	15205	.0636	.6161	-.0073	.0733	.0109	.1489
(Youngest Sib 3-7)		(.0031)	(.0130)	(.0051)	(.0088)	(.0042)	(.0599)
(3) Sample Children	9642	.0643	.6134	-.0065	.0519	.0106	.2034
(Youngest Sib 4-6)		(.0036)	(.0144)	(.0065)	(.0108)	(.0056)	(.1105)
(4) Post Sample	10389	.0561	.6382	-.0010	.0721	.0107	.1484
(Youngest Sib 2-8)		(.0026)	(.0136)	(.0040)	(.0107)	(.0062)	(.0918)
Panel D:	**Health Outcome: ER Visit**						
(1) Post Sample	10389	.1674	.6382	-.0001	.0721	.0055	.0767
(Youngest Sib 2-8)		(.0055)	(.0136)	(.0079)	(.0107)	(.0127)	(.1778)
(2) Post Sample	7757	.1682	.6377	-.0033	.0701	.0059	.0848
(Youngest Sib 3-7)		(.0062)	(.0135)	(.0099)	(.0106)	(.0119)	(.1716)
(3) Post Sample	4892	.1693	.6366	-.0063	.0523	.0078	.1491
(Youngest Sib 4-6)		(.0063)	(.0149)	(.0134)	(.0126)	(.0136)	(.2697)

Notes: Each coefficient is from a separate regression and includes the covariates listed in Table 2.3 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 2.5: Heterogeneous Effects by Demographic Characteristics of Maternal Employment on Overnight Hospitalizations

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

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Table 2.5: *continued*

Health Outcome: Overnight Hospitalization							
	N	Mean Hospital	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
Children 1998-2004	27242	.017	.643	-.0046	.0741	.0038	.0507
		(.001)	(.013)	(.0018)	(.0087)	(.0017)	(.0238)
Any Health Insurance	23305	.018	.654	-.0060	.0798	.0047	.0587
		(.001)	(.011)	(.0018)	(.0103)	(.0020)	(.0249)
Private Health Insurance	16327	.015	.690	-.0017	.0803	.0054	.0667
		(.001)	(.009)	(.0018)	(.0128)	(.0021)	(.0271)
Public Health Insurance	6978	.028	.541	-.0105	.0709	.0033	.0465
		(.003)	(.020)	(.0053)	(.0134)	(.0044)	(.0646)

Notes: All coefficients are from linear models. Each coefficient is from a separate regression including covariates listed in Table 2.3 Row 5, with standard errors (clustered by state) in parentheses. All samples include children ages 7 to 17 whose youngest sibling was between 24 and 107 months at the scheduled interview date. The rows are represented subsamples as specified. The coefficients presented are: Column (1) regression of child health on maternal employment, Column (2) regression of maternal employment on the youngest child's eligibility for kindergarten (First Stage), Column (3) regression of child health on the youngest child's eligibility for kindergarten (Reduced Form), and Column (4) instrumental variables estimates.

Table 2.6: Means by Labor Force Attachment Quartile

	Lowest Quartile	3rd Quartile	2nd Quartile	Highest Quartile
	(1)	(2)	(3)	(4)
Number of Obs	13347	13346	13347	13346
Hospitalization	.023	.019	.020	.020
	(.002)	(.002)	(.002)	(.001)
Injury/Poisoning	.032	.032	.031	.034
	(.004)	(.003)	(.002)	(.003)
Asthma Episode	.054	.072	.068	.065
	(.009)	(.007)	(.006)	(.006)
ER Visit	.196	.162	.158	.160
	(.014)	(.012)	(.010)	(.009)
Mother Worked	.485	.609	.668	.758
	(.006)	(.006)	(.006)	(.006)
Work Hours if Worked	34.052	32.661	33.982	35.571
	(.384)	(.384)	(.384)	(.384)
Work Hours with Zero's	7.632	11.598	14.880	21.025
	(.403)	(.403)	(.403)	(.403)
Num Kids	4.013	3.14	2.615	2.218
	(.036)	(.036)	(.036)	(.036)
Married	.943	.954	.924	.760
	(.016)	(.016)	(.016)	(.016)
Mother's Age	34.183	35.553	36.061	37.028
	(.133)	(.133)	(.133)	(.133)
Mom HS or Less	.754	.516	.401	.280
	(.014)	(.014)	(.014)	(.014)
Mom Some College or More	.246	.484	.599	.720
	(.014)	(.014)	(.014)	(.014)

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

Table 2.7: Heterogeneous Effects of Maternal Employment on Overnight Hospitalizations by Labor Force Attachment Quartiles

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

Notes: See notes to Table 2.4 for column descriptions. The sample is restricted to children ages 7 to 17, with white mothers, who have at least one younger sibling and whose youngest sibling was between 24 and 107 months at the scheduled interview date. All coefficients are from separate regressions, standard errors (clustered by state of residence) are in parentheses. The sample is broken down by the labor force attachment scale, as described in the text.

Table 2.8: Robustness Checks

Health Outcome: Overnight Hospitalization							
	N	Mean Hosp.	Mean Work	OLS (1)	First Stage (2)	Reduced Form (3)	IV 2SLS (4)
All Children	88887	.020 (.001)	.621 (.013)	-.0018 (.0011)	.0842 (.0050)	.0033 (.0011)	.0388 (.0133)
Child Not Limited	82372	.016 (.001)	.624 (.013)	-.0006 (.0008)	.0857 (.0052)	.0026 (.0010)	.0304 (.0123)
No Kids Limited	74661	.016 (.001)	.631 (.013)	-.0002 (.0008)	.0866 (.0058)	.0025 (.0010)	.0288 (.0117)
2 Children	30006	.021 (.001)	.703 (.010)	-.0024 (.0019)	.0628 (.0051)	.0040 (.0018)	.0876 (.0400)
3+ Children	58881	.020 (.001)	.576 (.014)	-.0014 (.0013)	.1020 (.0063)	.0026 (.0012)	.0333 (.0160)
Mom in Very Good or Excellent Health	58530	.018 (.001)	.652 (.011)	.0005 (.0012)	.0873 (.0076)	.0046 (.0013)	.0525 (.0154)
Boys	45475	.021 (.001)	.620 (.013)	-.0004 (.0017)	.1111 (.0059)	.0045 (.0016)	.0492 (.0181)
Girls	43412	.019 (.001)	.621 (.013)	-.0031 (.0015)	.0939 (.0059)	.0018 (.0018)	.0240 (.0229)
Child 9-17	67342	.020 (.001)	.632 (.013)	-.0015 (.0013)	.0843 (.0056)	.0038 (.0014)	.0456 (.0170)
Child 9-12	42271	.017 (.001)	.628 (.013)	-.0026 (.0015)	.1028 (.0073)	.0051 (.0021)	.0500 (.0211)

Notes: All coefficients are from linear models. Each coefficient is from a separate regression including covariates listed in Table 2.3 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 38.

Table 2.9: Appendix Table A: Kindergarten Eligiblity Cut-off Dates for 1983 and 2004

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

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

Table 2.10: Appendix Table B: Key Variable Definitions

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

Table 2.11: Appendix Table C: Non-linear Models, Compare to Table 2.4

	N	Mean	Mean Work	Probit (1)	First Stage (2)	Reduced Form (3)	IV BiProb (4)
Panel A:	**Health Outcome: Hospitalization**						
(1) All Children	88887	.0203	.6205	-.0017	.0899	.0030	.0255
(Youngest Sib 2-8)		(.0010)	(.0127)	(.0010)	(.0054)	(.0010)	(.0086)
(2) All Children	66160	.0205	.6235	-.0018	.0754	.0030	.0218
(Youngest Sib 3-7)		(.0011)	(.0126)	(.0013)	(.0058)	(.0011)	(.0076)
(3) All Children	41583	.0211	.6243	-.0032	.0465	.0039	.0266
(Youngest Sib 4-6)		(.0011)	(.0123)	(.0014)	(.0073)	(.0013)	(.0107)
(4) Post Sample	10389	.0163	.6382	-.0065	.0770	.0030	.0117
(Youngest Sib 2-8)		(.0013)	(.0136)	(.0022)	(.0110)	(.0027)	(.0081)
Panel B:	**Health Outcome: Injury/Poisoning**						
(1) Post Children	31960	.0262	.6438	-.0008	.0785	.0031	.0236
(Youngest Sib 2-8)		(.0015)	(.0133)	(.0019)	(.0083)	(.0019)	(.0135)
(2) Post Children	23714	.0259	.6466	-.0010	.0729	.0026	.0395
(Youngest Sib 3-7)		(.0016)	(.0130)	(.0020)	(.0082)	(.0017)	(.0191)
(3) Post Children	14868	.0243	.6477	-.0009	.0461	.0023	X
(Youngest Sib 4-6)		(.0017)	(.0127)	(.0021)	(.0073)	(.0020)	(X)
(4) Post Sample	10389	.0278	.6382	.0009	.0770	.0019	.0325
(Youngest Sib 2-8)		(.0021)	(.0136)	(.0034)	(.0110)	(.0041)	(.0512)

continued on next page

Table 2.11: *continued*

	N	Mean	Mean Work	Probit (1)	First Stage (2)	Reduced Form (3)	IV BiProb (4)
Panel C:	**Health Outcome: Asthma**						
(1) Samp. Children (Youngest Sib 2-8)	20304	.0624 (.0028)	.6164 (.0132)	-.0066 (.0042)	.0919 (.0089)	.0093 (.0046)	.0486 (.0151)
(2) Samp. Children (Youngest Sib 3-7)	15205	.0636 (.0031)	.6161 (.0130)	-.0063 (.0047)	.0792 (.0094)	.0099 (.0038)	.0549 (.0182)
(3) Samp. Children (Youngest Sib 4-6)	9642	.0643 (.0036)	.6134 (.0144)	-.0058 (.0059)	.0572 (.0117)	.0098 (.0050)	.0567 (.0283)
(4) Post Sample (Youngest Sib 2-8)	10389	.0561 (.0026)	.6382 (.0136)	-.0013 (.0038)	.0770 (.0110)	.0097 (.0059)	.0498 (.0297)
Panel D:	**Health Outcome: Emergency Room Visit**						
(1) Post Sample (Youngest Sib 2-8)	10389	.1674 (.0055)	.6382 (.0136)	.0008 (.0077)	.0770 (.0110)	.0065 (.0127)	.0816 (.086)
(2) Post Sample (Youngest Sib 3-7)	7757	.1682 (.0062)	.6377 (.0135)	-.0023 (.0097)	.0750 (.0109)	.0071 (.0120)	.0825 (.1259)
(3) Post Sample (Youngest Sib 4-6)	4892	.1693 (.0063)	.6366 (.0149)	-.0058 (.0133)	.0572 (.0133)	.0095 (.0138)	.1447 (.1162)

Notes: See Table 2.4 notes. Columns (1) - (3) are marginal effects from a probit model specification. Column (4) are marginal effects from a bivariate probit estimation.

Table 2.12: Appendix Table D: Estimated Weights for Local Average Treatment Effect

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

Chapter 3

Dads and Daughters: The Changing Role of Fathers in Women's Occupational Choices

Over the last century the labor force participation rate of women has risen threefold and there has been a tremendous increase in the integration of women into male-dominated occupations. We examine whether these phenomena have led to increased intergenerational transmission of occupation from fathers to daughters. We formalize this by estimating whether more recent birth cohorts of women are more likely than older cohorts to enter their father's occupation, controlling for general occupational upgrading of women into traditionally male-dominated occupations. We formulate a model of intergenerational occupation-specific human capital investment by fathers in daughters. When coupled with an assortative mating assumption for which we find support in our data, this model generates an empirical test of increased transmission from fathers to daughters. We compare the trend in the probability that a woman works in her father's occupation with the trend in the probability that a woman works in her father-in-law's occupation. Under reasonable assumptions, we argue that this difference is a lower-bound estimate of the increase in occupation-specific human capital transmission between fathers and daughters. Using three data sets on women and their families covering birth cohorts from 1909 through 1977, we estimate that the difference in trends is statistically significant and, in our full sample, accounts for 13 to 20 percent of the total increase in the probability that a woman enters her father's occupation over our sample period. This result is qualitatively robust to a host of specification checks.

3.1 Introduction

Over the last century, the labor force participation of women has risen threefold.[1] In addition, there has been a tremendous increase in the amount of integration

[1]See, for example, Goldin (1991).

of women in the labor market, so that women are far more likely now to work with men than in previous generations. Although the exact mechanisms for these changes remain somewhat elusive,[2] the fact that more women enter the labor market now and work in the same occupations as men has profound implications for many dimensions of the economy.

Interestingly, there is virtually no previous research examining how rising labor force participation and labor market integration of women have affected intergenerational transmission to daughters. In this paper, we examine changing intergenerational transmission from fathers to daughters by focusing on one key dimension of this change: the increasing probability across birth cohorts that a woman enters her father's occupation. The first contribution of this paper is to document the steady and large rise across birth cohorts of the 20th century in the probability that a woman works in her father's occupation. This suggests that the occupations of women's fathers may have played an increasingly important role in determining women's occupational choices, but does not directly imply that the transmission of what we call "occupation-specific human capital" has increased over this period. Because a woman born in a recent cohort is more likely to work in any traditionally male-dominated occupations relative to a woman in an older cohort, she is more likely than a woman born in an earlier cohort to enter her father's occupation even absent any changes in the transmission of occupation specific human capital from fathers to daughters. As a result, in order to provide evidence that intergenerational transmission of occupation-specific human capital between fathers and

[2]See, for example, Acemoglu, Autor, and Lyle (2004) and Goldin and Katz (2002).

daughters really has increased, we demonstrate empirically that the positive trend in the probability that a woman works in her father's occupation is larger than that which would be predicted just from the fact that women are more likely to enter men's occupations more generally.

The difficulty in demonstrating this is in figuring out the counterfactual for a given woman: What occupations might the woman have entered absent any transmission of occupation-specific human capital from her father? Clearly, for any given woman, some occupations are likely to be closer substitutes than others (and, in particular, closer substitutes to her father's occupation). Moreover, this set of close substitutes may differ across women and across birth cohorts in ways that would require us to place some structure on the substitutability of occupations. Consider the case of a woman whose father is a doctor. If her father is a country doctor who treats (and lives among) many farmers, the daughter may be more likely all else equal to become a farmer (versus, say, a college professor) because her father's labor market and social network puts her in contact with farmers. In contrast, the daughter of a city doctor who treats college professors is more likely to become a college professor rather than a farmer. But for both daughters, if her father increases his investment in her occupation-specific human capital, she becomes more likely to become a doctor than to become either a farmer or a college professor. Of course no data set is going to contain information on the relevant alternative occupations for any given woman. In the end, then, we draw on an identification assumption about assortative mating in order to identify the set of alternative occupations that a woman might enter, thereby distinguishing the country doctor's daughter from the city doctor's daughter. The basic idea is that because of assortative mating, a woman's father-in-law is likely to be working in the set of occupations that a woman might choose to work in, given her general human capital and given her preferences–preferences that may have been shaped by her social network growing up, her father's labor market network, her religious background, etc. To complete the example, the daughter of the country doctor is more likely to marry the son of a farmer than the son of a college professor, whereas the opposite is true for the daughter of the city doctor.

In order to formalize these ideas, we develop a model that combines features of intergenerational job-specific human capital transmission with an occupational choice model. The comparative statics of the model explicitly motivate the use of the information on the occupation of a woman's father-in-law to generate an empirical test of whether daughters have become more likely to enter their fathers' occupations, conditional on the general economic forces that have led to women increasingly entering men's occupations. The resulting empirical test compares the rate of increase in the probability that a woman works in her father's occupation to the rate of increase in the probability that a woman works in her father-in-law's occupation. Within the framework of the model we also are able to examine the empirical implications that arise if assortative mating is not perfect across fathers' occupations. We demonstrate that as long as assortative mating by occupation has not gone down across subsequent birth cohorts, our results are robust. To confirm that assortative mating has not decreased, we show that there has been no change in the probability that a woman's husband works in the same occupation as her father (his father-in-law).

Using data from the General Social Survey (GSS), the Survey of Income and Program Participation (SIPP), and the Occupational Changes in a Generation (OCG), we document changes in occupation-specific human capital transmission between fathers and daughters spanning birth cohorts from 1909 to 1977. There is clear evidence in the data of an increase in the probability that a woman works in her father's occupation over time. For example, with the baseline definition of occupation that we use, just under 6 percent of women born in our earliest birth cohorts work in their fathers' occupations, while around 20 percent of women born most recently work in their fathers' occupations. We also document an increase over birth cohort in the fraction of women working in the same occupations as their fathers-in-law. However, the increase in the probability over birth cohorts that a woman works in her father's occupation is larger than the increase in the probability that she works in her father-in-law's occupation, and the difference in these trends is statistically significant. In our baseline full sample results, we estimate that around 13 to 20 percent of the total increase in the probability a woman enters her father's occupation over our sample period can be attributed to an increase in the transmission of occupation-specific human capital between fathers and daughters, an estimate that we argue is likely a lower bound.

We perform a number of robustness checks and confirm our key empirical findings. In particular, our results are robust to changing the definition of occupation, accounting for (1) changes in labor force participation rates over time, (2) changes in the age at first marriage and the age of retirement for women, (3) changing educational attainment of women, and (4) the changing composition of male employment. Our key implications are also robust to using alternative definitions of occupation, including one that defines "occupations" to be industries. Finally, in contrast to what we find for daughters, we find no increase over time in the fraction of sons working in their fathers' occupations, nor any evidence that there has been an increased amount of specific human capital transmission over time between fathers and sons. Our results document an experience unique to women.

3.2 Background and Related Literature

3.2.1 Estimates of Intergenerational Transmission

Research on intergenerational transmission between parents and children has a long and rich history across multiple disciplines, going all the way back to Galton's work (1889) on the heritability of height. Becker and Tomes (1979, 1986) present an economic model where the utility of parents is a function of current consumption and the utility of a child. Because the utility of the child is itself a function of the child's general human capital, the parents optimize by choosing between consumption and investments in children. The model in its simplest form generates a straightforward, empirically testable relationship that specifies that the log of the income of the child will be a linear function of the log income of the parent. Actually testing the model empirically, however, is harder than it first appears given measurement error in income, (Solon, 1992), with perhaps the best current estimate of a stable intergenerational income parameter in the United States between fathers and sons standing at 0.6 (Mazumder, 2005).[3] Because these estimates just measure a correlation across generations, they cannot distinguish between a simple model of genetic heritability of traits associated with income and an economic model of investments parents make in children. This point has been made and examined in detail by Mulligan (1999) and Grawe and Mulligan (2002), who derive tests aiming to distinguish between economic models and models of heritability and find some evidence weakly consistent with investments. The model we specify below follows in the tradition of treating intergenerational human capital transmission as arising from parental investments in children, although in our discussion of the empirical results we simply refer to transmission of human capital, be it via investments or heredity.[4]

In sociology, the tradition has been to estimate intergenerational measures of "occupational prestige" and "occupational mobility." Sons may enter their fathers' occupations because of investments that fathers make in sons, because of heritable aspects of occupation-specific skills that lead sons to have comparative advantages in their fathers' occupations, or because of barriers to movement out of a father's occupation. Contingency tables (transition matrices) can be utilized to measure the extent of occupational mobility, where the cells of the contingency table are determined by fathers' occupations and sons' occupations. Occupational mobility

[3] There has been much less research devoted to intergenerational income transmission between pairs other than father-son. One notable exception is Chadwick and Solon (2002). Fernandez et al. (2004) discuss preference formation in an intergenerational transmission framework between mothers and sons.

[4] In discussing our model, we do briefly outline the empirical implications and that would arise under a model of pure heritability, where the main empirical implication would be that we are testing for an increase over time in the return to occupation-specific heritability.

can then calculated as the probability or odds of a son not entering his father's occupation (see, e.g., Ferrie, 2005 and Mosteller, 1968).

Measurements of the intergenerational transmission of occupational prestige involve rankings of occupations along some index, usually determined as functions of average income in occupations, and estimating the correlation in occupational prestige across generations. A few of these studies do examine intergenerational generational transmission between fathers and daughters (see, e.g., DiPrete and Grusky, 1990). While the exact specification of the occupational prestige index may be subject to criticism, using average incomes in an occupation may mitigate some of the problems associated with noisy measures of permanent income that have plagued some of the estimates of intergenerational income transmission in the economics literature.

3.2.2 Changes in Intergenerational Transmission

Sociologists have long been interested in changes in intergenerational transmission across generations, as seen in early work discussed by Hauser and Featherman (1978). Economists have only recently begun to examine this issue, partially as a result of the increasing availability of panel data, such as the PSID, with enough years of data to estimate changes in the transmission parameter over birth cohorts. Evidence on the extent of change in intergenerational transmission of income between fathers and sons is mixed (see Fertig, 2003, Lee and Solon, 2006, and the references therein) and depends to a large extent on the data sets used, how income is measured, and the time span considered. Partially as a result of this, we are careful in this paper to combine data from three different data sets collected over a time span of 29 years to ensure the robustness of our results across data sets that differ in the timing of data collection, the wording of questions, and sampling schemes.

When estimating changes in occupational mobility over (at least) two different generations, researchers distinguish between changes in "prevalence" and changes in "association." Changes in prevalence refer to changes across generations in the marginal distributions of the rows and columns of the contingency tables, whereas changes in association refer to changes that are left over once marginal distributions of contingency tables have been adjusted to be equal. It is changes in association that are generally referred to as changes in occupational mobility over time.[5]

What is absent from much of the empirical investigation into changes in intergenerational transmission is an investigation of underlying changes in behavior. For example, when contingency tables are adjusted for prevalence, so that only changes in association are used to quantify changes in occupational mobility over generations, there is no consideration given for why it may be that the marginal distributions of occupations have changed across the generations. In the case of women and their fathers, the fact that women have become more likely over time to be in male-dominated occupations may be a function of changes in investments made by their fathers. Adjusting contingency tables so that the marginal distribu-

[5] For a summary of statistical methods to adjust for differences in prevalence across contingency tables, see Little and Wu (1991). For a recent study of changes in occupational mobility, see Ferrie (2005) who concludes that occupation mobility in the United States has fallen over the 20th century. For an analogy between these methods and estimation techniques more commonly used by economists, see Hellerstein and Imbens (1999).

tion of women's occupations for recent cohorts looks like that of much older cohorts may adjust away the important changes in the impact of fathers on the occupation choices of their daughters. As a result, we take an entirely different approach.

3.3 The Illustrative Model

In this section, we develop an illustrative model to motivate how fathers' incentives to invest in daughters change as women's labor market opportunities change. To do this, we combine a model of intergenerational transmission and a model of occupation choice. We then use the model to illustrate how daughters' fathers-in-law can be used to control for changes in the marginal distributions of occupations over time. The comparative statics generate an empirical test of changes over time in the transmission of occupation-specific human capital from fathers to daughters.

The model consists of an occupational choice decision nested within a model of human capital investments in children. First, the father chooses the amount of the consumption good to purchase and the amount of investment to make in his daughter's general human capital H and job-specific human capital S, given his income I. The father can only invest in job-specific human capital for his own occupations.

The daughter chooses her occupation conditional on paternal investments that have been made and may decide to remain out of the labor force.[6] We begin with

[6] We explicitly consider the father as the individual decision-maker in this context, given that we are considering transmission of human capital embodied in the occupation of the father. We recognize, obviously, that there are also transmissions between mothers and

the daughter's occupation decision.

3.3.1 The Daughter's Problem

The daughter gains utility from working or not working, given the investment made by her father. We think of the daughter's choice of occupation as arising from the maximization of her latent utility y_j^* over four possible occupations. Occupation 1 is her father's occupation. Occupations differ in their closeness to each other, where occupations 1 and 2 are "closer" in utility terms to each other than occupation 3. Occupation 4 represents the choice of the daughter to remain out of the labor force. The woman's utility in each of the four occupations is represented as:

$$y_1^* = \alpha + \beta H + \gamma S + \epsilon_1$$

$$y_2^* = \alpha + \beta H + \epsilon_2$$

$$y_3^* = \beta H + \epsilon_3$$

$$y_4^* = \beta_o H + \epsilon_4.$$

In this formulation, occupation 2 is close to occupation 1 in the sense that they have the same (presumably positive) intercept shift α in utility. We think about this as reflecting something about a woman's taste for a set of occupations, as defined by her father's occupation, so that occupation 2 is a closer substitute to occupation 1 in utility than is occupation 3. These tastes may reflect factors, such as the social class in which the woman was raised or labor market networks defined by her father's

daughters. For most of the cohorts we consider in our empirical analysis, the daughters were raised by women who did not work, so in this paper we do not explicitly consider the role of mothers in daughters' occupational choices.

occupation, that change the woman's preferences directly or decrease costs of entry in some occupations more than others. To again use the example we discussed in the Introduction, a woman whose father is a doctor treating farmers may find herself, all else equal, more likely to become a farmer than to become a professor.

Note that general human capital pays the same return to a woman in the labor market regardless of what occupation she chooses, but a different return (β_o) if she is out of the labor market. Specific human capital only has a payoff if the woman enters her father's occupation. Alternatively and without loss of generality, y_4^* could instead represent the latent utility in an entirely female occupation. In that case the occupations 1 - 3 represent occupations in which men do work. The error terms ϵ_j can represent differences across occupations in a woman's underlying ability (comparative advantage) in that occupation, or preferences for that occupation.[7]

The daughter will choose the occupation j which yields the maximum value of y^*. If one has data from a sample of daughters that contains information on their occupational choices and those of their fathers, one can formulate an empirical test of whether $\gamma > 0$ without having actual data on S. In particular, one can estimate a discrete choice model of occupation by making functional form assumptions on the ϵ's, and by making the distinction between occupation 2 on the one hand, and occupations 3 and 4 on the other; that is, to model the closeness of occupations.

As is often done in models of occupational choice, assume the ϵ's are i.i.d from

[7] The closeness of occupations could also be modeled through assumptions on the covariance of the ϵ_j's, as for example in a nested multinomial logit model. For our purposes, modeling the closeness explicitly in the utility function through an intercept shift α is sufficient.

a Type I Extreme Value Distribution. Conditional on the H and S with which she has been endowed, the probability that a woman enters her father's occupation is:

$$\Pr(y_1 = 1) = \frac{e^{\alpha+\beta H+\gamma S}}{e^{\alpha+\beta H+\gamma S} + e^{\alpha+\beta H} + e^{\beta H} + e^{\beta_o H}}. \tag{3.1}$$

The probability that a woman enters occupation 2 is

$$\Pr(y_2 = 1) = \frac{e^{\alpha+\beta H}}{e^{\alpha+\beta H+\gamma S} + e^{\alpha+\beta H} + e^{\beta H} + e^{\beta_o H}}. \tag{3.2}$$

Note that if $S = 0$, so that the father has not made any investments in the daughter, then $Pr(y_1 = 1) = Pr(y_2 = 1)$. Therefore, if one does not have data on the investments of fathers, and in particular no data on S, an empirical test of whether fathers are making any specific human capital investments in their daughters can instead involve testing whether $Pr(y_1 = 1) = Pr(y_2 = 1)$. This test presumes no knowledge of S, but it does presume the ability to distinguish occupation 2 from the other occupations. Given a sample of women and a mapping of real occupations (e.g., doctor, farmer, professor, and out of the labor force) to occupations 1-4, this test is operationalized by estimating a multinomial logit regression over the four occupations. It requires including one dummy variable to represent α for the father's occupation, occupation 1, and the occupation that is close to it, occupation 2. A second dummy variable would be included for the father's occupation alone, where an estimated coefficient greater than zero on this dummy variable implies the existence of occupation-specific transmission between fathers and daughters.

Identifying which occupations are close together requires assumptions, however. First, one would have to make the assumption that closeness is the same across all women (at least in unobservable ways). Second, the nature of occupations and women's roles in those occupations themselves has changed over time in ways that may reflect changes in the closeness of occupations. To the extent that we are ultimately interested in measuring changes over time in the transmission of occupation-specific human capital between fathers and daughters, we would need to model changes in the closeness over time. One simple way we could implement this is to assume that α is equal to zero and estimate a standard multinomial logit model of occupation choice, including a dummy variable for father's occupation and, to estimate changes in the probability of entering a father's occupation over time, an interaction between a time (birth cohort) trend and father's occupation. As we discuss below, we have assembled a sample of women for whom we do not have complete information on father's occupation. Nonetheless, for the subset of our estimation sample for whom we do have information on father's occupation we have estimated this kind of simple multinomial logit model. The assumption of the Independence of Irrelevant Alternatives is strongly rejected by the data, substantiating that we do need to model the substitutability of occupations.

Therefore, we instead define which occupations are "close" to each other in utility terms by assuming that assortative mating occurs along the dimension of α, so that a woman whose father is in occupation 1 will always choose a husband whose father is in occupation 1 or occupation 2. This is true regardless of what occupation 2 happens to be for that woman, and whatever its origin (social networks, labor market networks across occupations, etc.). Repeating our example, the daughter of a doctor who treats farmers may be more likely all else equal to become a farmer (versus, say, a professor) because her father's social and labor market networks puts her in contact with farmers. The strict assortative mating assumption implies that this woman would meet and marry a man whose father is either a doctor or a farmer. In contrast, the daughter of a doctor who treats professors is more likely to become a professor (rather than a farmer) and will meet and marry a man whose father is either a doctor or a professor. In our view, a taxonomy of closeness defined by something like social or labor market networks that influence occupational choice and influence assortative mating is much less mutable over time than closeness as defined by the tasks performed in given occupations, for example. The assumption that assortative mating is perfect along occupations 1 and 2 in the model is obviously a strict assumption. Below we discuss the implications of relaxing both the assumption of strict assortative mating and the (less restrictive) assumption of constant assortative mating over time.

Given this assortative mating assumption, the probability that a woman is in the occupation of her father-in-law is:

$$\Pr(y_{father-in-law} = 1) = \Pr(y_1 = 1)\sigma + \Pr(y_2 = 1)(1 - \sigma). \tag{3.3}$$

We assume that the father-in-law works in either occupation 1 or 2. Therefore σ can be thought of as the number of men in the woman's father-in-law's cohort who are in occupation 1 divided by the number of men who are in either occupation 1 or 2 (i.e., the probability the father-in-law is in occupation 1). Similarly, $(1-\sigma)$ is the fraction of men in this population in occupation 2. Note again that if the father makes no specific human capital investments in his daughter so that $S = 0$, then $Pr(y_1 = 1) = Pr(y_{father-in-law} = 1)$. This generates an empirical test of whether $S = 0$, which would involve testing whether $Pr(y_1 = 1) = Pr(y_{father-in-law} = 1)$.

3.3.2 The Father's Problem

We assume that the father gains utility from his own consumption and from the utility of his daughter. The father has a finite level of income I to allocate between his own consumption, general human capital investment (e.g., schooling) in his daughter, and job specific human capital investment in his daughter. We assume that the father can only invest in occupation-specific human capital S for his own occupation. There are many possible forms these specific investments may take. For example, a father could make explicit investments in teaching his daughter his trade (either through teaching her himself or spending money to have her trained by others). He could also spend more time with her, and through their time together, demonstrate the value of working in his occupation. A father could invest in lowering barriers to entry for his daughter in his own occupation. Or, he could give his daughter monetary transfers and a taste for his occupation that she could use to invest in the skills necessary to work in his occupation.

The father's problem takes the following form:

$$\max_{H,S} \left\{ E[u^F(C, max_j y_j^*(H,S))] \right\} \tag{3.4}$$

$$s.t.\ I = C + p_H H + p_S S. \tag{3.5}$$

where $u^F(C, y_j^*(H,S))$ represents the father's utility, a function of own consumption, C, and daughter's utility, $max_j y_j^*$ as determined by her occupation choice, and where p_H is the cost of investments in general human capital and p_S is the cost of investments in occupation-specific human capital.

The father calculates expected utility knowing β, γ, and only the distribution of the ϵ's in the daughter's optimization problem. One could make functional form assumptions about the form of the father's utility function, but this is unnecessary for our purposes.[8]

3.3.3 Comparative Statics and Empirical Strategy

In the model, a father must make predictions about the actions of his daughter and decide on the levels of general and specific human capital investments to make in his daughter in order to maximize his utility. A father's investment decision will change with exogenous changes in the parameters of the model. We focus on changes in β, the return to general human capital, where we can think of an increase in β as representing an overall rise in the return to female labor market

[8] An obvious functional form assumption to make is that $u^F(C, y_j^*(H,S)) = \phi \ln(C) + (1-\phi)E[max_j\{y_j^*\}]$. This, coupled with the assumption that $\beta_0 = 0$ would lead to a closed form solution of $u^F(C, y^*(H,S)) = \phi \ln(C) + (1-\phi)\ln[e^{\alpha+\beta H+\gamma S} + e^{\alpha+\beta H} + e^{\beta H} + 1] + E$, where E is Euler's constant (see, e.g., McFadden, 1981).

participation. Because we have no direct data on investments of H or S that fathers make in daughters, we cannot directly examine what happens to these investments over time. Instead, we derive a comparative static that shows that if a father's investment in S increases with β, then the probability that a woman will enter her father's occupation increases relative to the probability a woman enters her father-in-law's occupation.[9]

From the daughter's problem, we derive the following comparative static from considering how the probability a woman enters her father's occupation changes with respect to β:

$$\frac{\partial \ln[Pr(y_1 = 1)]}{\partial \beta} = \gamma \frac{\partial S}{\partial \beta} + \frac{\partial \ln[Pr(y_2 = 1)]}{\partial \beta}, \tag{3.6}$$

This shows that if $\frac{\partial S}{\partial \beta} > 0$, the rate of change at which the daughter enters her father's occupation, occupation 1, due to a rise in β is larger than the rate of change at which she enters occupation 2.[10]

[9] While this discussion focuses specifically on the effects of changes in β, our empirical strategy looks at changes over time. Given the extensive empirical evidence in the literature, we assume that $\frac{\partial \beta}{\partial t} > 0$. These cases are identical as long as $\frac{\partial \gamma}{\partial t} = 0$. It is theoretically possible that $\frac{\partial \gamma}{\partial t} > 0$, that is, that there has been a rise in the return to occupation-specific human capital. Our empirical results cannot actually distinguish between a rise in S and a rise in γ. This is an example of the more general problem of disentangling heredity from investments that is a common feature of empirical work on human capital transmission. We find increases in investment to be a more compelling interpretation of the observed phenomena than simply increasing returns.

[10] Given the functional form of the utility function suggested in footnote 8, an interior solution for the optimal level of specific human capital S is $S = \frac{\ln[p_S \beta] + \ln[1+e^{-\alpha}] - \ln[p_H \gamma - p_S \beta]}{\gamma}$. One of the requirements for an interior solution is therefore the sensible condition that $\frac{\gamma}{\beta} > \frac{p_S}{p_H}$, or that the relative return to investing in specific rather than general human capital is greater than the relative price. The relevant comparative static is $\frac{\partial S}{\partial \beta} = \frac{\gamma p_H}{\beta(\gamma p_H - \beta p_S)}$ which must be greater than zero.

It is also the case that,

$$\frac{\partial \ln[Pr(y_{father-in-law} = 1)]}{\partial \beta} = \frac{\sigma\gamma\frac{\partial S}{\partial \beta}e^{\gamma S}}{(\sigma e^{\gamma S} + (1-\sigma))} + \frac{\partial \ln[Pr(y_2 = 1)]}{\partial \beta}, \tag{3.7}$$

so that the rate of change at which she enters her father-in-law's occupation is also positive if $\frac{\partial S}{\partial \beta} > 0$. The difference between these two comparative statics is:

$$\gamma\frac{\partial S}{\partial \beta}(1 - \frac{\sigma e^{\gamma S}}{(\sigma e^{\gamma S} + (1-\sigma))}), \tag{3.8}$$

which is positive as long as $\frac{\partial S}{\partial \beta}$ is positive, and zero otherwise. Therefore, an empirical test of whether fathers' specific human capital investments in daughters have increased over time can be cast as examining the difference between Equation 3.6 and Equation 3.7. Moreover, because the last term in parentheses in Equation 3.8 is positive, the difference between Equation 3.6 and Equation 3.7 actually provides a lower bound estimate for the rate at which changes in S increase the probability that a woman works in her father's occupation, occupation 1, relative to occupation 2.[11]

[11] We recognize that this model is simple in many ways and potentially could be extended along a number of interesting dimensions. For example, it incorporates no dynamics of the form of increasing β leading to increasing H and S which lead to further changes in the returns to H and S (similar in spirit to Fernandez et al., 2004). It would also be interesting to expand our model to incorporate a search model of marriage with the intergenerational transmission of human capital framework. Ermisch et al. (2006) contains a model of general human capital investment and marriage. It would also be interesting to consider investments that vary by family structure (e.g., family size, marital status of parents). We do not have data that would allow us to investigate these differences empirically, so we do not consider them further.

3.3.4 Imperfect Assortative Mating

Recall that our strict assortative mating assumption requires that a woman's father-in-law be chosen from occupation 1 or occupation 2 and not from occupation 3. Of course, in reality assortative mating is not perfect along the dimensions of sets of occupations as defined in the model with the parameter α, and so it is important to understand how violations of strict assortative mating might impact our results. One obvious way in which imperfect assortative mating could occur is if there is some probability that the woman will marry a man whose father is in occupation 3. If this probability is unchanging over time, however, this will simply lead to an intercept shift down in the probability that the woman is in her father-in-law's occupation, and more specifically will not alter the comparative statics as β rises. Alternatively, assortative mating could be stronger than we assume, so that the woman may be more likely to marry a man whose father is in her father's occupation than in any other. To the extent that this is true, the changing rate at which a woman is in her father-in-law's occupation will bias upward the estimate of Equation 3.7, and therefore will lead us to further underestimate the extent to which increased specific human capital investments have induced women to enter their fathers' occupations.[12] Finally, it is possible that, in reality, assortative mating patterns themselves have changed over time. To the extent that women are more

[12] This is also the reason why we use the probability that a woman is in her father-in-law's occupation rather than in her husband's occupation in our empirical test. If, for example, successive cohorts of men are increasingly more (less) likely to enter occupation 1, we will understate (overstate) the importance of transmission between fathers and daughters if we use husbands rather than fathers-in-law in our empirical test. By using fathers-in-law as the "counterfactual" for fathers, we draw men from the same cohort as fathers with the same underlying distribution of occupations.

likely than previously to marry a man whose father is in her own father's occupation, this again will cause us to underestimate the extent to which father's specific human capital investments have increased. Thus, the key identification assumption with respect to assortative mating in our model is that assortative mating by occupation cannot have gone down across birth cohorts.

Our strong sense is that assortative mating by occupation has not decreased. As women's education and labor force participation rates have risen, there is more contact between women and men in the same occupation and in places where they learn skills specific to their occupations (e.g. graduate school). If anything, this would lead to increased assortative mating on husbands' and wives' occupations, and hence on the occupations of fathers-in-law and fathers.[13] In the context of our model, the assortative mating assumption can be thought of as occurring along the dimension of "α." In words it implies that a woman's father-in-law works in an occupation that is "close" to her father's occupation. The identification assumption necessary for our empirical test to be valid is simply that assortative mating along α has not decreased over time. In practice, one implication of this assumption is that the probability that a father and father-in-law work in the same occupation is not declining over time. Because data constraints prevent us from testing this directly, we instead estimate whether there has been a change across birth cohorts in the probability that a woman's husband works in her father's occupation (his father-in-law). This approach is similar in spirit to a test of assortative mating used

[13]There is a long literature on the extent of assortative mating, particularly by education, and its change over time (see, e.g., Mare, 1991, and the references therein, and Rose, 2001).

by Lam and Schoeni (1994) who examine the extent of correlations in incomes of fathers-in-law (that is, women's fathers) and sons-in-law (women's husbands) as a measure of assortative mating.[14]

In order to search for any evidence of declines in assortative mating in our data, we estimated a series of regressions where we regressed a binary variable indicating whether a woman's father and husband work in the same occupation (using the measure of occupation described in Section 3.4) on a variety of controls and on the husband's birth cohort. We modeled the husband's birth cohort alternatively as a linear term and as a more flexible set of cohort dummy variables. We find that the probability that a man works in the same occupation as his wife's father is high (around 25 percent), but there is no evidence in any specification of a decline across birth cohorts in this probability.[15] This supports the key identifying assumption required for our analysis of the changing impact of fathers on women's occupation choices – assortative mating by occupation has not decreased over time. We discuss (and show graphically) the basic results of this analysis once more in Section 3.5, after a full discussion of our data set, our empirical methodology, and the results for fathers and daughters.

[14]Lam and Schoeni (1994) compare the intergenerational income correlation between fathers and sons and fathers-in-law and sons-in-law in the United States and Brazil. The father-son correlation is higher than the father-in-law-son-in-law correlation in the United States, but the opposite is true in Brazil. They argue that in Brazil assortative mating is so strong as to match husbands to fathers-in-laws who are more similar to them than the husband's own fathers, but that this is not true in the United States.

[15]This is true using simple regression as well as regression methods that control for changes over time in the marginal distribution of the occupations of the fathers-in-law. Full regression results are available upon request. Details on the method used for adjusting the marginal distributions can be found in Hellerstein and Imbens, 1999.

3.4 Data and Summary Statistics

3.4.1 The Data Sets

To create our sample, we combined data from three sources: the 1973 Occupational Changes in a Generation (OCG), the General Social Survey (using years 1975-2002), and the Survey of Income and Program Participation (1986-1988, Wave II). In the Data Appendix we provide an explanation of how the main variables of interest, labor force participation and occupation, were defined.

We chose to focus on more than one survey, and on these three surveys in particular, for a few reasons. First, these surveys are similar in that they are cross-sectional in nature and all ask information about a wife's occupation and the occupation of at least her father or her father-in-law, and sometimes both. Both the SIPP and OCG samples were specifically designed to to capture intergenerational information, and the GSS has the advantage that because it consists of repeated cross-sections, it helps us to separate age and cohort effects. Second, together the three samples comprise a large enough sample to allow us to obtain precise estimates. Third, because we use data spanning the years 1973 to 2002 and focus on individuals between the ages of 25 and 64, we are able to estimate effects for birth cohorts spanning a long time period: 1909 to 1977. Finally, using multiple data sets allow us to examine the robustness of our estimates. This is important given the heterogeneous findings in research on intergenerational income transmission for men. That said, we can only compare results across these data to the extent that the cross-sectional data sets do not confound age and cohort effects, something we return to below.[16]

The GSS has the distinct advantage of being drawn from a series of nationally representative cross-sectional data sets over a long period of years. Because of this, when a series of GSS's are linked together, there are observations on individuals at different ages who were born in the same birth cohort, allowing analyses that separately identify age and cohort effects. This is vital in our context because our aim is to identify how the relationship between fathers' occupations and daughters' occupations has changed across birth cohorts, conditional on the age of the women in the sample. This analysis obviously cannot be done conditional on age with only one cross-section of data. The GSS does have a few shortcomings, however. First, it is a small data set, even when surveys are pooled over multiple years. Second, the unit of observation in the GSS is an individual and not a household, so while information is collected on the occupation of the respondent, the respondent's father, and the respondent's spouse there is no information on the occupation of the respondent's father-in-law. As a result, one cannot use data from the GSS to estimate a full-blown multinomial occupational choice model, where one estimates whether a given woman is more likely to go into her father's occupation and her father-in-law's occupation, relative to other occupations. But the GSS data can be used for our empirical test. We utilize data from the GSS surveys of 1975-2002.[17]

The 1973 OCG, while no longer a well known data set, is an obvious candidate

[16]We considered adding information from longitudinal data sets like the NLS and PSID, but they are different enough in structure and do not contain information on in-laws of sample individuals or families that we chose not to utilize them in this paper.

[17]1975 was the first year that the GSS employed standard probability sampling

survey for this paper because it was a large survey that was designed specifically to capture intergenerational relationships (see Featherman and Hauser, 1978, for more information). Because we combine data from the OCG with later surveys, we concorded the 1970 occupation codes that are used in the OCG to 1980 so that the occupations would be comparable. More details on this are given in the Data Appendix.[18] The SIPP Personal History Topical Modules in 1986, 1987, and 1988 were designed to mimic the OCG and are therefore complements to the OCG, as they contain similar information on cohorts of individuals 13-15 years after the OCG. Because these SIPP topical modules were all conducted in Wave II, there is very little of the attrition that sometimes plagues studies that use the SIPP. Unfortunately, these particular topical modules have not been repeated for more recent years.

The OCG was conducted as a supplement to the Current Population Survey (CPS) in March 1973. Questionnaires were mailed out to male CPS respondents, specifically asking information about their family and their background, including the occupation of their father when they were 16 and, for married respondents, the occupation of their wife's father when their wife was 16. These responses, combined with the occupation responses and other background variables given as part of regular CPS survey, allow us to have for our sample the key information that we need to conduct our analysis: occupations of the husband and wife, the occupations of each of their fathers, and the ages of the husband and wife. The SIPP data that we use naturally contains similar information.

[18]There was a 1962 OCG survey as well, but we have chosen not to use it because it would have required yet another concordance, of 1960 occupations to 1980 occupations.

Because our analysis relies on using information on the occupation of fathers-in-law, we necessarily restrict the data to contain only married respondents. We further restrict the sample to only whites, so as not to confound occupational changes that are unique to women with those that are due to changing opportunities for blacks. Finally, we restrict our baseline sample to those between the ages of 25 and 64 in order to obtain information on individuals during their prime working years. But because the age at first marriage has risen over time and the age of retirement has declined over time, we examine the robustness of our results to limiting the age range to those between the ages of 35 and 55.[19]

3.4.2 Female Labor Force Participation

In order to get some sense of how comparable the data are across surveys and how they reflect general trends, we first examine female labor force participation by birth cohort in each data set. We provide a graph in Figure 3.1 that shows the fraction of women who were employed in each year for each survey. Because we

[19]It is worth noting that because we are combining data across multiple surveys, we have a limited amount of demographic information that is consistent across surveys. This, along with issues related to small sample sizes when we disaggregate the data, limits the scope of questions we can explore as well as the set of controls we can add to the regressions. For example, we would expect that changes in family structure over time should affect the transmission of human capital from fathers to daughters in ways that theoretically could be tested. Rising divorce rates should have reduced the average transmission over time from fathers to daughters if divorce leads daughters to have less exposure to fathers. On the other hand, declining family size may increase a father's transmission to the focal daughter, since there is less competition for resources from siblings, absent changes in the return to general human capital in the labor market. But we do not know consistently in our data whether a woman was raised in a household with her father present, nor do we know how many siblings she had (nor their gender composition), so we cannot examine these issues directly. What we do estimate then is an average trend across family structures in the rate of transmission of occupation-specific human capital over time.

treat “out of the labor force” as an occupation in itself (one that daughters do not, by our definition, ever share with their fathers), it is important to examine female labor force participation rates in the context of occupation transmission between fathers and daughters.

We do not expect our data sets to provide identical female labor force participation rates for each birth cohort because of age effects. We therefore also graph female labor force participation rates by birth cohort for the 1970-2000 Decennial Census Public Use Micro Samples (PUMS), four nationally representative data sets drawn from years similar to our three data sets. Our samples consist of married, white women between the ages of 25 and 64 who report that they are not in school and are either working or out of the labor force (we exclude “unemployed” women and women in school).[20] We also restrict our attention to women who are either the head of household or the spouse of the head of household.

It is useful to begin by comparing data from the PUMS samples. For the birth cohorts that overlap between the samples, it is clear that overall female labor force participation rates increased over time. Looking at birth cohorts separately, one can see that for earlier birth cohorts there is exit out of the labor force as women age toward retirement while for later birth cohorts female labor force participation clearly increased over the decades. For all four data sets, a dip in female labor force participation exists for women in their 30’s, presumably as a result of child-rearing. The changing labor force participation rates of women through their lifetimes fore-

[20]Note that the PUMS definition of labor force participation is closest to that of the SIPP. See the Data Appendix for exact definitions of female labor force participation across data sets.

shadows the importance of controlling for age in our analysis of intergenerational occupation between fathers and daughters.

Data from the GSS surveys of 1975-2002 provide the longest time period over which to examine labor force participation by birth cohort. The GSS spans the data from the SIPP and OCG, and nearly spans our Census years as well. As the graph in Figure 3.1 indicates, the GSS labor force participation rates do cut through those of the other data sets and rise from well below 20 percent for the birth cohorts early in the 20th century to well above 60 percent for women born in the 1960s and thereafter.

The OCG contains information on the labor force participation in 1973 of women born between 1909 and 1948. Average labor force participation of women in the OCG lies between the 1970 and 1980 PUMS graphs, as it should. Similarly, the SIPP profile of female labor force participation, derived from data collected between 1986 and 1988, is between the two PUMS profiles from 1980 and 1990, and is closer to 1990, as would be expected. In total, female labor force participation in our data reflects that seen in PUMS data, and across our three data sets the trends in female labor force participation over time by birth cohort are consistent with age effects of retirement and child-rearing.

3.4.3 The Definition of Occupations

Until this point we have been vague as to what we mean by an occupation and how to operationalize it. Following standard practice, we define occupation using

Census definitions. In our baseline results, we use the six major occupation groups as defined by the 1980 Census Occupation Codes: Managerial and Professional Specialty; Technical, Sales, and Administrative Support; Service; Farming, Forestry, and Fishing; Precision Production, Craft, and Repair; and Operators, Fabricators, and Laborers. As in our model, for women we also include a seventh occupation group, Out of the Labor Force, which includes women who are not working, are not in school full time, and are not unemployed or looking for work. As part of our robustness checks we disaggregate the list of occupations further, to 13 occupations listed as subheadings of the three-digit 1980 Occupation Codes.[21] Clearly, the more we disaggregate, the less power we have to detect changes in father-daughter occupation transmission, so we do not consider levels of occupational disaggregation below this. Perhaps more importantly, as mentioned above, the theoretical notion of occupation-specific human capital does not map directly to Census occupation classifications. For example, just as the literature on job-specific human capital can be recast to be about industry-specific human capital (see, e.g., Neal, 1995), so our definition of occupation can be recast to map into industries. We therefore also present our main results using an indicator of a woman being in the same industry as her father or father-in-law.

Table 3.1 contains summary statistics for our pooled sample, as well as for each data set. The statistics cover the occupational breakdown of women, fathers, and fathers-in-law, as well as age and birth year of women in our sample. The pooled

[21] See Appendix Table 3.6 for a mapping between the six and thirteen occupation category groupings.

data set is our estimation sample, so that for all women we have information on the occupation of her father or her father-in-law.[22] In our pooled data set, almost half (46.2 percent) of women are out of the labor force, with the next most populated occupation being Technical, Sales, and Administrative Support, comprising 22.8 percent of the sample. By comparing the proportions in each occupation across data sets, and particularly by comparing women in the OCG and women in the SIPP, one can clearly see how the occupational distribution of women has changed over time. In the OCG, 57.0 percent of women are coded as out of the labor force, whereas only 37.3 percent of women are in the SIPP. Moreover, conditional on labor force participation, women in the SIPP are more likely than their earlier counterparts to be either managerial and professional occupations or in technical, sales, and administrative support (46.1 percent in the SIPP versus 27.7 percent in the OCG).

The occupation distributions of fathers and fathers-in-law are extremely similar within each data set, as they should be absent non-random sampling or differential response rates by occupation of parents and in-laws. Over time for these fathers and fathers-in-law there are also changes in the occupational distribution; for example, these men are less likely to be in farming in the SIPP relative to the OCG. Because of this, and because fathers in different occupations may transmit different amounts of specific human capital to children, we show results below with and without occupation controls for fathers and fathers-in-law.

[22]The distribution of occupation for women is very similar when women for whom we have no information on fathers or fathers-in-law are included.

Below the distributions of occupations in Table 3.1 we present summary statistics on the fraction of women who are in their father's and father-in-law's occupations in each data set. Overall, 10.7 percent of women in the data work in their father's occupation, and 9.9 percent work in their father-in-law's occupation. While these differences are not large in absolute terms, they are in percentage terms. Moreover, across data sets, it becomes clear that the differences grow over the birth cohorts in our sample: in the OCG, where the mean birth year of women is 1931, the difference between the two means is 0.2 percentage points, whereas in the GSS, where the mean birth year is 1946, the difference is 1.8 percentage points.

3.5 Empirical Implementation and Results

Our basic empirical strategy is to compare the trends over birth cohorts in the probability that a woman works in her father's occupation relative to the probability that a woman is in her father-in-law's occupation. We formulate this as a single regression equation, pooling observations where we observe a woman and her father with observations where we observe a woman and her father-in-law:

$$Prob(same = 1)_i = \delta_0 + \delta_1 * DIL_i + \delta_2 * D_i * Y_i + \delta_3 * DIL_i * Y_i + \delta_4 * D_i * A_i + \delta_5 * DIL_i * A_i + \varepsilon_i. \tag{3.9}$$

In this specification, *same* is an indicator which equals one if a woman is in the same occupation as her father or father-in-law, DIL is a dummy variable that equals one if the observation contains information on a woman (daughter-in-law) and her father-in-law, D is (1-DIL), Y is the birth year of the woman, and A is the age of the woman. The empirical prediction of the theoretical model suggests that we should be comparing rates of change in the probabilities over time, rather than absolute changes. But, as we show below, the estimate of δ_1 is small, and, when statistically significant, is positive. This indicates that the baseline probability for fathers and daughters to be in the same occupation is the same or lower as that for fathers-in-law and daughters-in-law, so that a statistically significant differences in the absolute change (a difference between δ_2 and δ_3) alone implies that fathers have increased investments over time in occupation-specific human capital of daughters.

Controlling for age (when possible) is important because women may transition into their "final" occupations as they gain experience in the labor market and, more importantly, as women move in and out of the labor force when they have children. Theoretically, it is quite possible for the coefficients on age, δ_4 and δ_5, to be different if, for example, a woman moves into her father's occupation as she gains experience in the labor market.

For two of our data sets, the OCG and the SIPP, we often observe information on the occupation of a woman and those of her father *and* her father-in-law, contributing two observations to the regression, so we always calculate robust standard errors clustering on observations where the same woman is observed. We present results for linear probability models. Marginal effects from logit models are virtually identical and therefore are not presented.

In Table 3.2 we show basic results for this regression specification for all three data sets together and then the three data sets separately. Because we cannot separately identify age and cohort effects in the OCG and SIPP, we do not include separate controls for age in this table. Column 1 contains results for the full sample. The estimated coefficient on the daughter's birth year, δ_2, is statistically significant and indicates that the probability that a woman enters her father's occupation increases by 0.27 percentage points per year. To put this in perspective, the fraction of women in their father's occupation born over the first decade of our sample (1909-1919) is only 0.058, so that we estimate each year thereafter leads to a very large 4.59 percent increase in the probability that a woman works in her father's occupation.

The coefficient estimate on the daughter-in-law's birth year is 0.21 and is also statistically significant.[23] The fact that this point estimate is also large in magnitude, a finding repeated throughout the empirical results to follow, highlights the importance of controlling for overall trends in women's labor market entry when teasing out the distinct impact of the change in the extent of occupation-specific human capital transmission between fathers and daughters. We estimate nonetheless that $\delta_2 - \delta_3$, the annual change in the probability of a daughter being in her father's occupation *relative* to the equivalent change for a daughter-in-law/father-in-law pair, is a statistically significant 0.05 percentage points, or a difference of 1.19 percent. This difference, a measure of the impact of increased transmission in specific human capital on the shift toward women working in their fathers' occupations, accounts for approximately one fifth of the overall change over time in the probability that a woman works in her father's occupation.

Figure 3.2 is the graphical representation of Table 3.2, column (1), except that

[23]The fraction of women in their father-in-law's occupation born over the first decade of our sample (1909-1919) is 0.063, which is actually statistically indistinguishable from the fraction of daughters in their fathers' occupations.

instead of using linear regression, we generate these results using locally weighted least squares. There are two important things to take away from this figure. First, the probability that a woman is in her father's occupation is very slightly below that of fathers-in-law and daughters-in-law early in the period, but grows over the period of our sample to be above that of fathers-in-law and daughters-in-law. Second, the time trends in both of these probabilities are indeed close to linear, as we model them in equation 3.5. Figure 3.3 presents the same results as in Figure 3.2, but in terms of rates of change rather than absolute changes. Here, trends show the probability a woman is in her father's (father-in-law's) occupation relative to the fraction of women in their father's (father-in-law's) occupation in 1909-1919. As in Table 3.2, on average, each decade leads to approximately 50 percent increase in the probability that a woman works in her father's occupation.

Column 2 of Table 3.2 shows results for only the OCG sample of women who were born between 1909 and 1948. Of the women in this sample, 57 percent are recorded as being out of the labor force in 1973. The gradient of the probability of a woman working in her father's occupation is relatively flat over this period, with a precisely estimated increase of 0.09 percentage points every year. The estimated increase in the fraction of women entering their father-in-law's occupation is lower, 0.06 percentage points. The difference between these two is not statistically significant. This is not surprising given that women born in these years largely remained out of the labor force.

Column 3 shows the baseline results for the GSS for women in birth cohorts spanning 1911 to 1977 (although with very few observations for women at the tails of this distribution). The point estimate on the increased probability of father-daughter occupation transmission is 0.33 percentage points. Relative to the baseline over the 1909-1919 period, this represents almost a 6 percent increase in this probability per year of the sample. The increase in the probability that a woman works in her father-in-law's occupation is smaller, at 0.27 percentage points. Finally, the relative difference between these two is 0.06 percentage points per year, which while not statistically significant, is 17.8 percent of the overall increase in the probability that a woman works in her father's occupation. In column 4 we report results for the SIPP sample, representing women born 1921-1963. These results are similar to those for the GSS.

In Table 3.3 we examine results for various specifications of the model in the full sample of pooled data. Column 1 replicates the baseline results of Table 3.2 but includes controls for the survey from which the observation comes. If survey questions differ in a way that might affect the baseline probability of a woman being in a man's occupation, the inclusion of these controls should pick that up.[24] The point estimates of the father-daughter and father-in-law-daughter-in-law trends are somewhat smaller than in the previous table, but still are large and statistically significant. Moreover, the difference in the trends between fathers and daughters and fathers-in-law and daughters-in-law again is 0.06 percentage points per year and is statistically significant. There are a few other things to note in this specification. First, there are statistically significant differences in the constant terms across data

[24]For example, the GSS asks the respondent to report the occupation of her father while she was growing up, while the SIPP and OCG ask for the occupation of her father when she was 16 years old.

sets. As we show below, however, this result does not hold up in other specifications when we include more controls. Second, the dummy variable for the daughter-in-law equation constant, δ_1 in the regression equation, has a coefficient of 0.02 and is statistically significant. This result is also not robust.

In column 2 of Table 3.3 we include variables for age separately for daughters and daughters-in-law, as in Equation 3.5. The estimates of the coefficients on the age variables are highly significant, almost identical (0.25 and 0.23 percentage points), and statistically indistinguishable from one another. They imply that every for 10 years that the woman ages the probability that a woman enters her father or father-in-law's occupation increases by over a healthy 2 percentage points. Given this result, and given that birth year and age are negatively correlated in these data, the inclusion of age into the model should cause the coefficients on the birth year trend variables to go up. Indeed, the estimates more than double, indicating large changes between birth cohorts in the probability that a woman works in both the occupations of her father and her father-in-law. The coefficient on the birth year of a daughter rises to 0.44 percentage points (from 0.22), while the coefficient on the birth year of a daughter-in-law rises to 0.37 percentage points. The former result can be interpreted as a 7.6 percent increase per year relative to the baseline probability, while the latter yields a 5.9 percent increase. The estimate of the relative difference in the two trend variables is 0.07 percentage points and again is statistically significant.

The fact that the dummy variables for the survey of origin and the dummy variable for the daughter-in-law are all statistically significant in column 2 leads to the specification in column 3, where we interact the survey of origin dummy variables with the daughter-in-law dummy variable. The point estimates on birth year are slightly closer together and slightly less precise, so while the estimate of the relative difference between the two is very close to the previous specifications (0.05 percentage points), it is not statistically significant. The full set of interactions between the survey dummies and the daughter-in-law constant leads to small and insignificant differences across the board in these coefficients. Because they are small and statistically insignificant, we drop the interaction terms in the columns that follow, in order to gain more power in estimating the difference in the trends between daughters and daughters-in-law. Similarly, we constrain the coefficient on daughter's age, δ_4, to equal that on daughter-in-law's age, δ_5. These results are presented in column 4, where the point estimates on the birth year coefficients are virtually unchanged, but more precise, so that the estimated difference of 0.05 percentage points between the two coefficients is statistically significant (standard error of 0.02). This difference accounts for 13.1 percent of the overall change in the probability that a woman works in her father's occupation.

Because the distribution of the occupations of fathers and fathers-in-law has changed over time as well (see Table 3.1) in ways that may affect the probability that a woman is in one of these men's occupations, and because we can only estimate the impact of average investments made by fathers across different occupations, in column 5 we include a full set of controls for the occupations of fathers and fathers-in-law. To the extent that it is fathers transmitting occupation-specific human capital rather than fathers-in-law, there is no reason to expect the coefficients on these dummy variables to be the same for these two groups, and indeed (in results not shown) they are not. Including these dummy variables reduces the point estimates on the birth year variables and the variable for women's age. The coefficient on the birth year of daughters is 0.28 percentage points, a 4.8 percent increase per year over the baseline father-daughter probability, while that of the daughter-in-law is 0.23 percentage points. Both remain highly statistically significant. The difference between these two is 0.05 and is again statistically significant. This represents 16 percent of the overall increase in the probability that a woman works in her father's occupation and, once again, implies that there has been an substantial increase in the transmission of occupation-specific human capital between fathers and daughters.

The last two columns of Table 3.3 replicate the specifications in columns 4 and 5, but they also include controls for the educational attainment of women. Education partially determines occupational choice, and educational attainment is also correlated with the occupations of fathers and fathers-in-law. Therefore, a woman's educational attainment may represent an important omitted variable in the regressions. Of course, education is also endogenously chosen and we have no way to account for this, so we view these results as a robustness check. As anticipated, the coefficient estimates on the probability a woman is in her father's or father-in-law's occupation decline with the inclusion of education controls, but the difference in these trends is still significant and is similar to the specification without education controls. In both the specification without and with the controls for father's and father-in-law's occupation, the difference in trends represents 19 percent of the increase in the probability a woman is in her father's occupation.

The different specifications reported across columns in Table 3.3 vary the set of covariates included in the model. In Table 3.4 we vary other aspects of the model, presenting results that parallel those in columns 4 and 5 of Table 3.3. In columns 1 - 4 of Table 3.4 we vary the samples over which we estimate the model. Recall that the age range of women in our baseline sample is 25 to 64. By necessity, all of these women are married. Because the age at first marriage has been rising over time and because the age of retirement has been falling, there may be compositional changes over time in who is included in this sample. To test whether this has an effect on our results, in columns 1 and 2 of Table 3.4 we restrict the age range of our sample to 35 to 55, an age range where the vast majority of people (particularly whites) have gotten married, and where early retirement is not yet a major factor.[25] This reduces our sample size considerably, from 63,076 to 34,544. The specification in column 1 mimics that of Table 3.3, column 4, and therefore includes separate controls for survey and constrains the effect of age to be the same for daughters and daughters-in-law. The coefficient on the birth year variables in this column (0.46 percentage points for daughters and 0.40 percentage points for daughters-in-law) are very similar to those in the previous table and are again statistically significant. The estimate of the difference between the two, 0.06 is also statistically significant. Column 2 adds controls for the occupations of fathers and fathers-in-law, paralleling the specification of Table 3.3, column 5. Here, the point estimates are again similar to those of Table 3.3, column 5, but the difference between the two of 0.04 has a larger standard error than in Table 3.3, presumably because the sample size has been

[25] According to the authors' calculations using pooled CPS data from 1970-1999, less than 6 percent of women ages 35-55 report having never been married. As a comparison, just under 9 percent of women ages 25-64 fall into this category.

cut almost in half. In total, we interpret these specifications as providing evidence that our results are robust to these sample selection issues.

The theoretical model in Section 3.3 differentiates between women who are out of the labor force and women who are in a set of occupations in which men work. As mentioned above, we could recast the occupation of women who are out of the labor force in our model (which we labeled occupation 4) to be traditionally female occupations where men never (or almost never) work, such as nursing. The model would yield the same implications. Moreover, our model suggests that if investments in specific human capital S increases as the return to general human capital in the labor market increases, we should see an increase in the probability that a woman works in her father's occupation, relative to that of her father-in-law, even conditional on labor market participation. In columns 3 and 4 we therefore explore this empirically by including in the sample only women who are in the labor force. This again causes the sample to fall by almost one half. Column 3 replicates the specification of column 4, Table 3.3. Because so much of the change over time in the probability a woman enters the occupation of her father or father-in-law is due to labor force entry, the coefficients on the birth year trend are lower when we restrict the sample in this way. The coefficient on birth year of daughters is 0.33 percentage points and that of daughters-in-law is 0.25. Both are statistically significant and, importantly, the difference between the two is 0.08 and is statistically significant.

In column 4 we add controls for father and father-in-law occupations. While the birth year coefficients themselves become small and statistically insignificant, the relative difference between the two remains of the same magnitude as the full sample result at 0.05. This estimate is only marginally significant, due to the much lower sample size. In summary, we interpret the results in columns 3 and 4 as showing that our full sample results are not being driven solely by entry into the labor market, and, as our model suggests, that occupational changes over time by women in the labor market are affected by the transmission of specific human capital between fathers and daughters.

In the last four columns of Table 3.4 we vary the definition of "occupation." In Columns 5 and 6 we refine the definition of occupation to consist of 13 occupation categories (rather than 6).[26] Perhaps the most important distinction between the categorizations is that the two broad occupations in which most women work conditional on labor market participation, "Managerial and Professional Specialty" and "Technical, Sales, and Administrative Support," are each broken up. In column 5, the estimate on the coefficient on birth year for daughters is 0.19 percentage points and is statistically significant. The estimate on the father-in-law trend coefficient is smaller, at 0.15 percentage points, and the difference between the two is 0.04 and is statistically significant. Column 6 includes father's and father-in-law's occupation controls. While the coefficients on the trends fall and the difference between the two falls to 0.03, it is still statistically significant. Again, this implies there has been an increase in the transmission of occupation-specific human capital from fathers to daughters.

Finally, as mentioned previously, while our results thus far have used Census occupation codes, this may not correspond to our theoretical notion of occupation-

[26]For a list of the 6 and 13 occupation groupings see Appendix Table 3.6.

specific human capital. Therefore, in Columns 7 and 8 of Table 3.4 we report results from specifications paralleling Columns 4 and 5 of Table 3.3, now using industry to classify the notion of specific human capital.[27] The magnitudes of the estimates in these columns are similar to that using the more disaggregated occupation categories. The baseline probabilities that a woman is in her father's and father-in-law's occupation are .046 and .049 respectively. Again, we calculate the baseline probability as the fraction of women born between 1909 and 1919 that are in the same occupation as their father (father-in-law). Therefore the results in Column 7 should be interpreted as indicating that for every year there is a 4.84 percent increase in the probability that a woman works in her father's industry. The difference in trends between a woman entering her father's versus father-in-law's industry is .04 percentage points, representing almost 19 percent of the increase in probability a woman enters her father's industry. Though the trends are flatter in Column 8, when controls are added for father's and father-in-law's industry, the difference is still statistically significant and large. This evidence is consistent with increased specific-human capital transmission from fathers to daughters.

In total, the results are remarkably robust across specifications and samples. There has been a large increase over time in the probability that a woman enters

[27]In order to make the definition of industry consistent over time, we collapse the 15 major industries categories from the 1980 Census into 13 categories: (1) Agriculture, Forestry, and Fishing, (2) Mining, (3) Construction, (4-5) Manufacturing (combined Nondurable and Durable Goods), (6) Transportation, Communications, and Other Public Utilities, (7-8) Wholesale Trade (combined Durable and Nondurable Goods), (9) Retail Trade, (10) Finance, Insurance, and Real Estate, (11) Business and Repair Services, (12) Personal Services, (13) Entertainment and Recreational Services, (14) Professional and Related Services, and (15) Public Administration.

her father's occupation. Moreover, this increase is not due simply to changes in the marginal distribution of women's occupations, but is due, at least partially, to increased transmission of occupation-specific human capital from fathers to daughters. Our results imply that the increase in the probability a woman is in her father's occupation is about 13 to 20 percent larger than the increased probability that a woman will enter her father-in-law's occupation, and this is a lower bound estimate of the impact of increased transmission.

In the last two columns of Table 3.3 we explored the robustness of our results to the inclusion of education controls for women, noting that occupation and education are correlated. In Figure 3.4 we explore this possibility more explicitly by using education instead of occupation to generate results analogous to those in Figure 3.2. That is, we present locally weighted least squares estimation of the probability a woman has the same level of education as her father and as her father-in-law by her birth cohort. If the results for education look like those for occupation, one might be worried that we are not picking up something about specific human capital transmission between fathers and daughters but instead something more akin to general human capital. Figure 3.4 looks nothing like Figure 3.2. The change over time in the probability a woman has the same education level as her father (or father-in-law) is u-shaped with respect to her year of birth.

Our final check on the link between our empirical results and the main motivation for this paper is illustrated in Figure 3.5. We have claimed that something special changed in the relationship between fathers and daughters as a result of the increased entry of women into the labor market and into traditionally male occupations, and we framed our model to be changing incentives for fathers to invest in the specific human capital of their daughters. To the extent that this is true, we should not see the same trends in the probability that sons work in their fathers' occupations.

Figure 3.5 presents results from locally weighted least squares that are analogous to those in Figure 3.3.[28] We contrast the rate of growth of the fraction of daughters in the same occupation as their father and their father-in-law to the respective rates of growth for sons. The top two "lines" illustrate growth rates in the probability that a woman is in her father or father-in-law's occupation over time, while the bottom "lines" show the same trends but for sons. There are several things to note in this graph. First, while the baseline probability that a son enters his father or father-in-law's occupation is much higher than that for women, the growth rate across birth cohorts for men are essentially flat. There is a slight rise toward the end of the period for fathers and sons, but this is an artifact of a nonlinear increase in the probability of fathers and sons working in the same occupation in the SIPP. This increase is not seen in the GSS over the same birth cohorts. Second, as noted previously in our discussion of assortative mating, the flat trend for sons and fathers-in-law supports the identifying assumption that assortative mating by occupation has not changed over time. We also estimated regressions for sons that

[28]We exclude from our sample men who are not working. We think it unlikely that most of these men are actively engaged in home production, but instead that they are temporarily out of the labor force, so that the fact that they are not in their father's occupation at the time of the survey is transitory. This eliminates very few men in practice and including a separate out-of-labor force category for these men does not affect the results.

parallel those of Tables 3.3 and 3.4, and they confirm that there is no evidence of a rise in the probability that a son enters his father's occupation across birth cohorts, and no robust evidence that this probability has increased faster than the probability that a son enters his father-in-law's occupation.[29] Finally, and most importantly, the rate of growth in the probability a woman is in her father's occupation dwarfs any changes that men experienced over the same time period.[30]

3.6 Conclusion

The labor market in the 20th century was profoundly affected by the increase in female labor force participation. One potential implication of increased female labor force participation is that it changes the incentives for fathers to invest in their daughters. In particular, it can increase the incentive to invest in human capital that is specific to a father's occupation, causing a rise in the probability that a woman enters her father's occupation.

Simply documenting that there has been an increase over time in the propensity of a woman to enter her father's occupation is not enough to determine whether there has been increased occupation-specific human capital transmission between

[29] Results available upon request.

[30] There is one caveat to using men as a falsification test of our results. Until this point, our model and discussion have focused on one-child families. If human capital transmission within the family is a purely private good, and if fathers over time invest more in their daughters, they may invest less in their sons. This will itself affect the results in Figure 3.5, rendering it a flawed falsification test of our model and results for women. Moreover, if fathers invest less in sons, and if there is assortative mating in marriage by occupation, this could lead over time to fathers-in-law becoming a poorer control for fathers in our analysis of women. The OCG and SIPP do contain data on the number and sex mix of siblings which could potentially be used to examine whether the impact on boys of having sisters has changed over time, but of course family size and sex mix are endogenous.

fathers and daughters. A woman will be more likely to enter her father's occupation even absent such an increase, because she will be more likely to enter any number of traditionally male-dominated occupation, including her father's.

We demonstrate that under the assumption that assortative mating by fathers' occupation has not decreased over time, an assumption for which we find support in our data, we can compare the rates of change over time in the probabilities that a woman enters her father's occupation and her father-in-law's occupation to determine whether there has been an increase in the transmission of occupation-specific human capital from fathers to daughters.

We combine three data sets containing information collected between 1973 and 2002 spanning birth cohorts born between 1909 and 1977. We show that over time the probability that a woman enters her father's occupation has increased significantly and substantially. For the full sample of married women, we estimate that with each successive year, the probability that a woman born in a particular year would enter her father's occupation increased by somewhere between 0.22 and 0.44 percentage points. The fraction of women entering their father-in-law's occupation increased anywhere from 0.16 to 0.38 percentage points. Across our many specification checks, the increase in the probability that a woman enters her father's occupation is always larger than the probability that a woman enters her father-in-law's occupation, a finding that we interpret as evidence of increased transmission of occupation-specific human capital between fathers and daughters. For the full sample of women, our results imply that the increase in the probability that a woman enters her father's occupation is around 20 percent higher than the increased probability that she enters another occupation in her choice set, an estimate that is likely a lower bound.

It is natural to speculate as to the form of transmission of occupation-specific human capital from fathers to daughters. Unfortunately, there is not much direct evidence that we know of that helps in this regard. For example, for the case of specific human capital investments, perhaps the most obvious form is investments in time. While there is some information from time-use diaries on how much time parents spend with children, the earliest reliable data come from time-use diaries from 1965, so we have no information on many of the birth cohorts in our sample. There is some evidence that fathers did not spend more time on primary childcare in the mid-1980's than they did in the mid-1960's, but that there has been an increase more recently. Of course, this is an increase that would not be represented in the birth cohorts of our sample. Disappointingly, there is no systematic evidence over time in how this time that fathers spend with children is allocated across daughters and sons, making it difficult to draw any inferences about investments of time in daughters specifically. (See the review in Raley and Bianchi, 2006, for more on what has been learned from time-use surveys on parental time with children.) Dahl and Moretti (2005) present evidence that the ways in which fathers are part of the lives of sons and daughters have changed over time (e.g., via divorce, custody, single parenthood), but these various ways all indicate that fathers have preferences for sons and do not suggest how this manifests itself in terms of changing investments in daughters over time.

We have focused on transmission from fathers to daughters in this paper because for many of the birth cohorts in our sample, the vast majority of their mothers were out of the labor force. Therefore, any maternal investments (or transmission) that affected the occupation choice of daughters is difficult to formalize in this way and is perhaps second order to those made by fathers. However, as recent cohorts of women with high levels of labor force attachment themselves become mothers, there should be changing incentives for these women to make investments of occupation-specific human capital in their own daughters (and sons). It will be quite interesting to examine for future cohorts how potentially "competing" transmission of the occupation-specific human capital of fathers and mothers affect the occupation choices of children, and in particular how they affect the occupation decisions of daughters relative to sons.

3.7 Data Appendix

3.7.1 Core Sample Description

In each data set we restrict to white, married men and women. We exclude respondents who are younger than 25 years old or who report being married to someone under age 25. While we exclude women who are older than 64 years in the regressions (and make similar restrictions for men in the male sample regressions), we do allow women who are married to men older than 65. One reason for this asymmetric treatment is that restricting to men and women older than 25 helps control for data quality, since occasionally children are incorrectly coded as spouses. The results are insensitive to restricting to *couples* that are between 25-64 years old, but relaxing the upper bound restriction allows for a larger sample. One effect of this asymmetric treatment is that the male sample is slightly smaller than the female sample, since men tend to marry women who are younger. All of our results are for married, white individuals who report being either the head of household or the spouse of the head of household.

3.7.2 Labor Force Participation and Occupations

Appendix Table 3.5 describes how we define who participates in the labor force and who is dropped from the sample across each data set. As described in the text of the paper, we consider women who have decided not to work as a separate occupation "Out of Labor Force". This category includes women who are "keeping house," as the OCG and GSS categorize them. The OLF category should not include women who are unemployed, looking for work, in school, or doing something else that is distinct from choosing to remain out of the labor force. We run sensitivity tests restricting regressions to include only women who report working and to include an OLF category for men and our results are qualitatively consistent across these samples. Note that we never include OLF for fathers or fathers-in-law, since we do not have an employment status code for fathers and therefore can not distinguish between item non-response and a non-working father.

The SIPP is distinct from the OCG and GSS in how employment status is coded. In the SIPP, work status is asked separate of school enrollment or other activities. A respondent could be coded as having a job and being enrolled in school, while the GSS and OCG only report one employment status per individual. We feel someone enrolled in school, either part-time or full-time, is likely to not be in their final occupation, even if a valid occupation code is given. Because of this, we restrict the SIPP sample to include only individuals who are not currently enrolled in school. While we were not able to make an identical restriction in the OCG and GSS, sensitivity tests including only individuals who reported working full-time provide similar results.

3.7.3 Occupation Coding and Concordances

Because our three surveys contain different occupation codings, we had to find a way to get a consistent definition of "occupation" for our analyses. For each decennial census a new set of occupation codes are defined. Though these tend to be similar, they are not identical across years. The 1973 OCG reports 1970 (and 1960) Census Occupation Codes, while the SIPP reports 1980 Occupation Codes. The GSS, on the other hand, uses 1970 codes for some years, 1980 codes for later years, and both for the middle years. To get a consistent definition of occupation we created a concordance from the 1970 to 1980 Census Occupation Codes. In the GSS survey years 1975-1990 the 1970 occupation codes are reported, while 1980 codes are provided for survey years 1988-2002. This provides us with 3 survey years (1988, 1989, and 1990) for which both 1970 and 1980 occupation codes are given to create a concordance.

To create the concordance we take the 1980 occupation code that is most frequently matched to each 1970 occupation code, choosing the smallest code by default in a tie. Once we have this mapping from 1970 to 1980, we merge the 1980 occupation codes onto the early years of the GSS with only 1970 occupation codes and onto the OCG. Tests of the sensitivity to using categorizations of the 1970 and of the 1960 codes provided consistent results.

Appendix Table 3.6 lists the occupation groupings used in our analysis.

Figure 3.1: Female Labor Force Participation by Birth Cohort

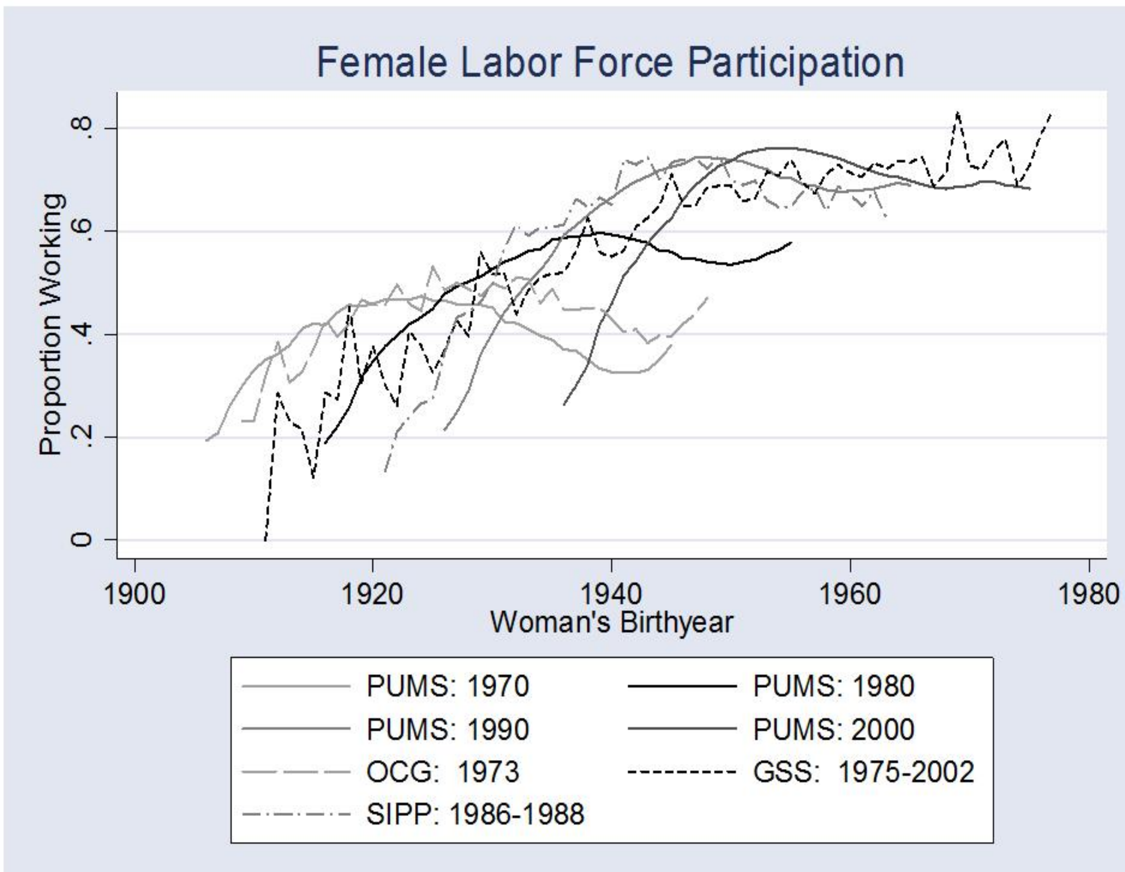

Figure 3.2: The fraction of women in the same occupation as their father and their father-in-law

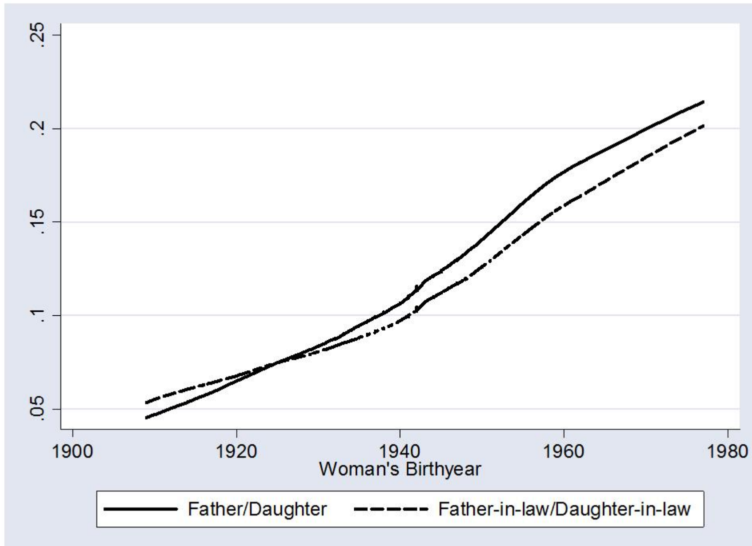

Figure 3.3: The rate of growth of the fraction of women in the same occupation as their father and their father-in-law

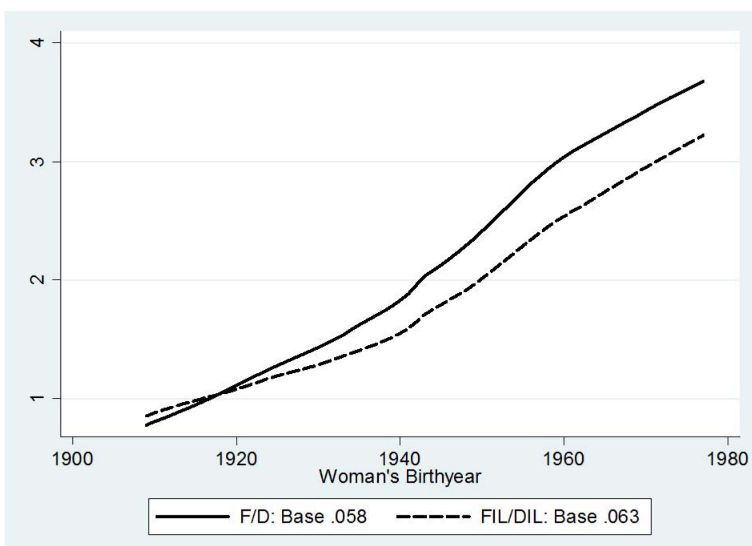

Figure 3.4: The fraction of women in the same education level (measured in 4 broad categories) as their father and their father-in-law

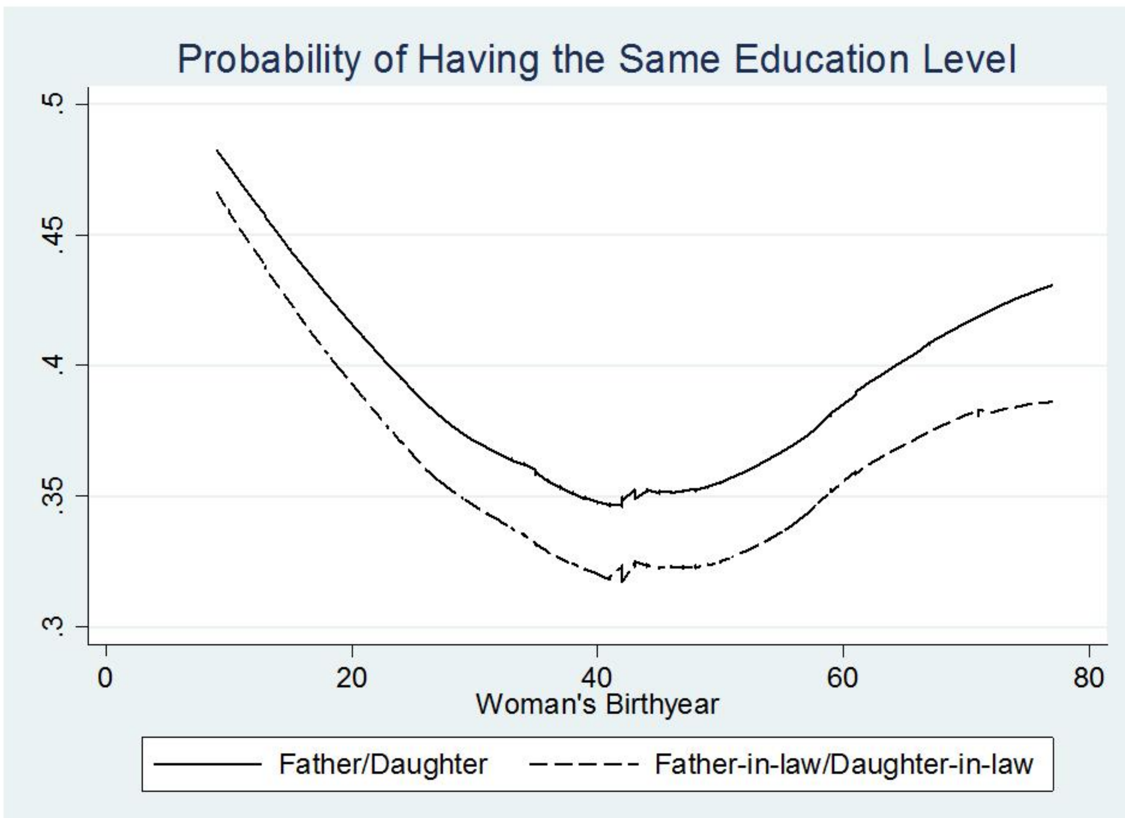

Figure 3.5: A comparison of the rates of growth of the fraction of women versus men in their father's occupation and father-in-law's occupation

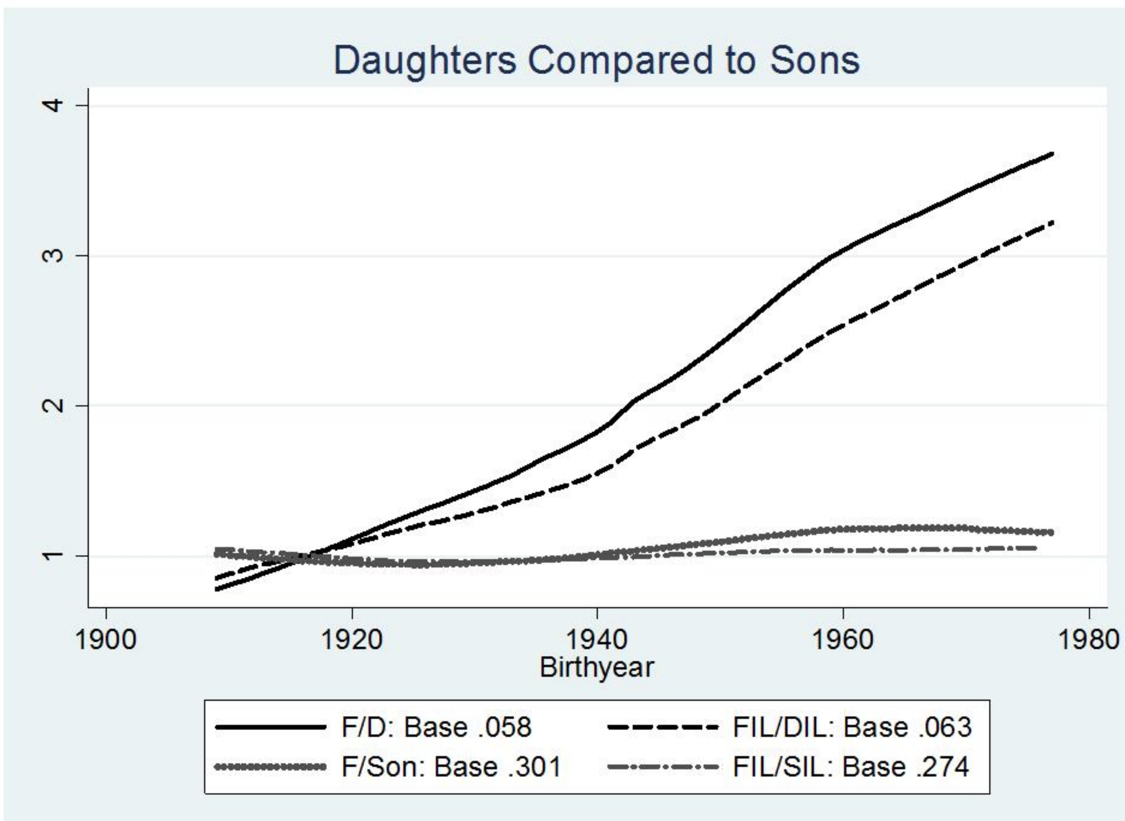

Table 3.1: Summary Statistics for Women

		ALL	OCG	GSS	SIPP
Women	(1) Managerial and Professional Specialty	0.155	0.103	0.215	0.178
	(2) Technical, Sales, and Admin. Support	0.228	0.174	0.254	0.283
	(3) Service	0.078	0.068	0.082	0.088
	(4) Farming, Forestry, and Fishing	0.009	0.010	0.006	0.010
	(5) Precision Production, Craft, and Repair	0.011	0.008	0.012	0.015
	(6) Operators, Fabricators, and Laborers	0.058	0.067	0.050	0.052
	(7) Not in Labor Force	0.462	0.570	0.381	0.373
Fathers	(1) Managerial and Professional Specialty	0.191	0.180	0.223	0.190
	(2) Technical, Sales, and Admin. Support	0.119	0.096	0.131	0.147
	(3) Service	0.050	0.055	0.040	0.048
	(4) Farming, Forestry, and Fishing	0.195	0.232	0.156	0.157
	(5) Precision Production, Craft, and Repair	0.230	0.224	0.240	0.235
	(6) Operators, Fabricators, and Laborers	0.216	0.213	0.210	0.224
Father-	(1) Managerial and Professional Specialty	0.184	0.172	0.225	0.185
in-Laws	(2) Technical, Sales, and Admin. Support	0.111	0.090	0.117	0.145
	(3) Service	0.048	0.050	0.040	0.048
	(4) Farming, Forestry, and Fishing	0.222	0.263	0.174	0.174
	(5) Precision Production, Craft, and Repair	0.223	0.214	0.242	0.228
	(6) Operators, Fabricators, and Laborers	0.213	0.212	0.202	0.221
Fraction of Women in Father's Occupation		0.107	0.079	0.138	0.134
Fraction of Women in Father-in-Law's Occupation		0.099	0.077	0.120	0.128
Woman's Age		42.100	41.800	42.300	42.300
		(10.9)	(10.7)	(11.0)	(11.2)
Woman's Birth Year		1939.1	1931.2	1946.2	1944.4
		(13.6)	(10.7)	(13.3)	(11.2)
Sample Size of Women		40,360	17,617	11,006	11,737

Table 3.2: Baseline Results for Probability of Daughters in Same Occupation as Father

Dependent variable: In same occupation as father or father-in-law

	Pooled	OCG	GSS	SIPP
	(1)	(2)	(3)	(4)
Birthyear Daughter	0.268	0.088	0.328	0.305
	(0.013)	(0.019)	(0.031)	(0.028)
	[4.591]	[1.504]	[5.629]	[5.235]
Birthyear Daughter-in-law	0.213	0.065	0.270	0.250
	(0.013)	(0.019)	(0.032)	(0.031)
	[3.399]	[1.031]	[4.310]	[3.990]
Const.	0.005	0.051	-.014	-.0008
	(0.005)	(0.006)	(0.014)	(0.012)
DIL Eqn Dummy	0.013	0.005	0.009	0.015
	(0.006)	(0.007)	(0.020)	(0.016)
Relative Diff F/D vs FIL/DIL	0.055	0.023	0.058	0.055
	(0.017)	(0.023)	(0.045)	(0.037)
	[1.192]	[0.473]	[1.319]	[1.245]
Obs.	63076	32700	11006	19370

Notes: Standard errors are in parentheses. Rates of increase (relative to baseline 1909-1919 birth cohorts fraction of women in the same occupation as their father or father-in-law, 0.058 for daughters and 0.063 for daughters-in-law) are in brackets. Results are from linear probability models. Coefficients on birth year, age, and the relative difference in slopes are in percentage point terms. Standard errors are robust and account for correlation across observations that arise from a daughter and daughter-in-law representing the same woman.

Table 3.3: Full Specification for Women

Dependent variable: In same occupation as father or father-in-law

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Birthyear Daughter	0.218	0.441	0.434	0.435	0.279	0.297	0.242
	(0.014)	(0.042)	(0.055)	(0.040)	(0.038)	(0.040)	(0.038)
	[3.743]	[7.571]	[7.440]	[7.464]	[4.793]	[5.091]	[4.147]
Birthyear Daughter-in-law	0.160	0.371	0.379	0.378	0.234	0.242	0.196
	(0.014)	(0.042)	(0.056)	(0.040)	(0.038)	(0.040)	(0.038)
	[2.554]	[5.924]	[6.053]	[6.037]	[3.737]	[3.860]	[3.127]
Const.	0.034	-.173	-.164	-.166		-.108	
	(0.006)	(0.037)	(0.048)	(0.035)		(0.034)	
SIPP	0.004	0.009	0.004	0.009	0.007	0.010	0.008
	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)
OCG	-.023	0.011	0.007	0.011	0.003	0.007	0.002
	(0.004)	(0.006)	(0.009)	(0.006)	(0.006)	(0.006)	(0.006)
DIL Eqn Dummy	0.015	0.028	0.009	0.015		0.014	
	(0.006)	(0.025)	(0.069)	(0.006)		(0.006)	
DIL Dummy*SIPP			0.009				
			(0.008)				
DIL Dummy*OCG			0.008				
			(0.013)				
Daughter's Age		0.247	0.239				
		(0.044)	(0.057)				
Daughter-in-law's Age		0.226	0.235				
		(0.045)	(0.058)				
Constrained D/DIL's Age				0.237	0.190	0.155	0.158
				(0.041)	(0.038)	(0.040)	(0.038)
F/FIL Occ Cntrls	No	No	No	No	Yes	No	Yes
Own Ed Cntrls	No	No	No	No	No	Yes	Yes
Relative Diff F/D vs FIL/DIL	0.058	0.070	0.055	0.057	0.045	0.055	0.046
	(0.017)	(0.031)	(0.079)	(0.017)	(0.016)	(0.017)	(0.016)
	[1.188]	[1.647]	[1.386]	[1.428]	[1.056]	[1.231]	[1.020]
Obs.	63076	63076	63076	63076	63076	63032	63032

Notes: See Table 2.

Table 3.4: Robustness Checks

Dependent variable: In same occupation as father or father-in-law

	Prime Age		In Labor Force		13 Occupations		15 Industries	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Birthyear Daughter	0.455	0.256	0.333	0.079	0.191	0.122	0.224	0.150
	(0.056)	(0.052)	(0.065)	(0.057)	(0.029)	(0.028)	(0.034)	(0.032)
	[6.075]	[3.414]	[2.007]	[0.479]	[5.922]	[3.794]	[4.841]	[3.241]
Birthyear Daughter-in-law	0.399	0.218	0.255	0.030	0.150	0.092	0.182	0.110
	(0.057)	(0.052)	(0.065)	(0.057)	(0.028)	(0.027)	(0.034)	(0.032)
	[5.282]	[2.893]	[1.446]	[0.172]	[4.189]	[2.580]	[3.688]	[2.237]
Const.	-.170		0.015		-.067		-.082	-.087
	(0.049)		(0.057)		(0.025)		(0.029)	(0.028)
SIPP	0.004	0.004	0.004	0.002	0.005	0.005	0.003	0.008
	(0.006)	(0.005)	(0.006)	(0.006)	(0.003)	(0.003)	(0.004)	(0.004)
OCG	0.012	-.0007	0.022	0.020	0.004	-.0008	0.013	0.008
	(0.009)	(0.009)	(0.012)	(0.010)	(0.005)	(0.004)	(0.005)	(0.005)
DIL Eqn Dummy	0.013		0.016		0.011		0.016	0.012
	(0.011)		(0.013)		(0.004)		(0.005)	(0.008)
Constrained D/DIL's Age	0.248	0.166	0.105	0.074	0.106	0.087	0.135	0.103
	(0.062)	(0.057)	(0.066)	(0.059)	(0.029)	(0.028)	(0.034)	(0.033)
F/FIL Occ Cntrls	No	Yes	No	Yes	No	Yes	No	Yes
Relative Diff F/D vs FIL/DIL	0.056	0.037	0.078	0.049	0.041	0.030	0.042	0.039
	(0.032)	(0.030)	(0.031)	(0.029)	(0.012)	(0.012)	(0.015)	(0.014)
	[0.793]	[0.521]	[0.562]	[0.307]	[1.733]	[1.214]	[1.153]	[1.004]
Obs.	34544	34544	33242	33242	63076	63076	58039	58039

Notes: Standard errors are in parentheses. Rates of increase are in brackets. In columns (1) and (2) the relevant baseline fractions of women in the same occupation as their father or father-in-law, using the 1918-1928 birth cohort, were 0.075 for both the daughter and daughter-in-law. For columns (3) and (4) the baselines were 0.166 for the father-daughter pair and 0.176 for the father-in-law-daughter-in-law. The baselines for columns (5) and (6) were 0.032 and 0.036 for the father-daughter and father-in-law-daughter-in-law respectively. The final two columns used the relevant baseline of the fraction of women in the sample in the same industry as their father or father-in-law, 0.046 and 0.049 respectively. Results are from linear probability models. Coefficients on birth year, age, and the relative difference in slopes are in percentage point terms. Standard errors are robust and account for correlation across observations that arise from a daughter and daughter-in-law representing the same woman.

Table 3.5: Appendix Table A1: Description of Labor Force Definitions

	OCG	GSS	SIPP	PUMS
Question wording	"What was _ doing most of LAST WEEK?"	"Last week were you working full time, part time, going to school, keeping house, or what?"	Work status: Month 4; School: During any of the past 4 months	Employment Status Recode (empstatg): Previous week; School attendance: During the past 2 months
Possible Responses	(1) Working (2) With a job, but not at work (3) Looking (4) Housework (5) School (6) Unable to work (7) Other	(1) Working full time (2) Working part time (3) With a job, but not at work b/c of temp illness, vacation, or strike (4) Unemployed (5) Retired (6) In School (7) Keeping House (8) Other (9) No answer	With a job the entire month: (1) worked all weeks (2) missed one or more weeks, no time on layoff (3) missed one or more weeks, spent time on layoff With a job 1+ weeks: (4) no time spent looking/layoff (5) spent one or more weeks looking or on layoff No job during month: (6) spent entire month looking or on layoff (7) spent one or more weeks looking or on layoff (8) no time spent looking/layoff	Employment Status Codes: (1) Employed (2) Not Employed (3) Not in Labor Force
Women	Working: (1) or (2) Out of Labor Force: (4) or (7) Dropped: (3), (5), (6)	Working: (1), (2), or (3) Out of Labor Force: (5), (7), or (8) Dropped: (4), (6), or (9)	If not enrolled in school: Working: (1) - (5) Out of Labor Force: (8) Dropped: (6) or (7)	If not enrolled in school: Working: (1) Out of Labor Force: (3) Dropped: (2)
Men	Working: (1) or (2) Dropped: (3) - (7)	Working: (1), (2), or (3) Dropped (4)-(9)	If not enrolled in school: Working: (1) - (5) Dropped: (6) - (8)	N/A

Table 3.6: Appendix Table A2: 1980 Census Occupation Code Groupings

1980 Census Occupation Codes	
Six Occupation Categories	**13 Occupation Categories**
(1) Managerial and Professional Specialty	(1) Executive, Administrative, and Managerial Occupations and Management Related Occupations (2) Professional Specialty Occupations
(2) Technical, Sales, and Administrative Support	(3) Technologists, Technicians and Related Support Occupations" (4) Sales Occupations (5) Administrative Support Occupations, Including Clerical
(3) Service	(6) Service Occupations, Private Household Occupations (7) Protective Service Occupations (8) Service Occupations, Except Protective and Household
(4) Farming, Forestry, and Fishing	(9) Farming, Forestry, and Fishing Occupations (5) Precision Production, Craft, and Repair (10) Precision Production, Craft, and Repair Occupations
(6) Operators, Fabricators, and Laborers	(11) Machine Operators, Assemblers and Inspectors (12) Transportation and Material Moving Occupations (13) Handlers, Equipment cleaners, Helpers, and Laborers"

Chapter 4

A Critique of "Mothers and Sons: Preference Formation and Female Labor Force Dynamics"

In reference to the article:
Published in the Quarterly Journal of Economics, November 2004
Authors: Raquel Fernandez, Alessandra Fogli, and Claudia Olivetti
Full Title: "Mothers and Sons: Preference Formation and Female Labor Force Dynamics"

4.1 Introduction

In their 2004 paper, "Mothers and Sons: Preference Formation and Female Labor Force Dynamics," Fernandez, Fogli, and Olivetti (hereafter referred to as FFO) present a model whereby a woman's participation in the labor force is partially determined by her mother-in-law's work experience. The men in the model have preferences for the working behavior of their wife formed through their mother's work experience, either by mothers having endowed them with skills that make them better partners for a working woman or inclining them to prefer a working wife. Women in the model acquire market skills in order to be able to marry this growing subset of men that prefer working women. Assortative mating is not incorporated into the model. In addition, a woman's own preferences for working are not modeled to be a function of her own background characteristics, such as her own mother's labor supply. The model has dynamic implications, since the new incentive for wives to work yields more mothers that work, creating more sons who will eventually want working wives. While the empirical results presented in FFO support this story, in this paper I present empirical evidence that directly contradicts their findings. In Section 4.5 I discuss a possible interpretation of this new empirical evidence.

FFO present three sets of results that support their model. First, the General Social Survey (GSS) is used to show a conditional correlation between the working behavior of men's mothers and that of their wives. While I am not able to exactly replicate these results, I present qualitatively similar regressions confirming this relationship. However, while FFO correctly describe that the GSS data only include the working behavior of the respondent's mother and contain no mother-in-law labor force information, they do not present a parallel set of regressions using female respondents and the working behavior of wives' mothers. In addition, FFO only use data from 1988 and 1994, when a particular variable on mother's work was collected. However, similar variables were collected in other years. An expanded analysis of the GSS confirms a significant conditional correlation between mother-in-law and daughter-in-law work experience in some specifications, although always with a smaller magnitude than that presented in FFO. More importantly, the conditional correlation between mother's and daughter's work experience is similar in magnitude and is also statistically significant, in contrast with FFO's main findings. It is this relationship between the labor force participation of mothers and daughters that calls into question FFO's interpretation of the dynamic results as simply about men's changing preferences.

The second set of results that FFO present uses data from the Female Labor

Force Participation and Marital Instability survey (FLFPMI).[1] The data allow the authors to include the working behavior of both the husbands and wives' mothers in the regression on the wives' labor force participation. I am able to replicate most of their results. I confirm that in these particular specifications there is a conditional correlation between mother-in-law and daughter-in-law work behavior and, once the work behavior of the spouse's mother is included in the regression, there is no conditional correlation between mother and daughter work behavior. However, I will demonstrate that these results are highly sensitive to specification. In particular, a small change in the definition of mother's work causes both estimates to become statistically insignificant. I am not able to account for why the conditional correlation between the labor force participation of mothers and daughters is zero (and at times negative), besides noting potential omitted variables, selection bias, and measurement error made more problematic due to small sample sizes. To address this specifically I present a parallel analysis using the much larger, and potentially more reliable, data set the Survey of Income and Program Participation (SIPP). The SIPP results demonstrate that the marginal effect of own mother's work experience on a woman's labor force participation is positive and significant in almost all specifications, contradicting the conclusions drawn in FFO.

The final evidence FFO present leverages PUMS data to test the dynamic

[1]The FLFPMI is the first wave of a six wave survey by Booth, Johnson, Amato, and Rogers that is generally referred to as the "Marital Instability over the Life Course" or "Work and Family Life" surveys. Citation: Booth, Alan, et al. MARITAL INSTABILITY OVER THE LIFE COURSE [UNITED STATES]: A FIVE-WAVE PANEL STUDY, 1980, 1983, 1988, 1992-1994, 1997 [Computer file]. ICPSR02163-v2. University Park, PA: Alan Booth et al., Pennsylvania State University [producers], 1998. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2001.

implications of their male preference formation model. The results show that in regions where more mothers work due to an exogenous shock (variation in mobilization rates during World War II) the next generation of women are more likely to work. Similarly, in states where the average fertility rate of working mothers compared with non-working mothers is higher, the model predicts that the next generation will have higher female labor supply, since relatively more children are raised by working mothers. While this is interpreted as an effect of the preferences of men in FFO, it may also be seen as evidence supporting the intergenerational effect of mothers' work experience on their own daughters. FFO refute this competing explanation by highlighting their cross sectional evidence showing a zero marginal effect of own mother's work behavior on daughter's labor force participation. As I hope to provide sufficient evidence to call this result into question, I will not further analyze the intergenerational evidence presented in Section III of FFO and simply suggest that the effect found may be due, at least in part and perhaps entirely, to the intergenerational effects of mothers on daughters.

4.2 Evidence from the General Social Survey (GSS)

To begin, I replicate the GSS analysis presented in Section II of FFO. Table A shows that my sample is similar qualitatively to FFO's, but they are not identical. The first column is a copy of the values presented in Appendix I of FFO. Column (2) of Table A shows the means and standard deviations of the variables used in my replication. The third column uses the same sample, but does not place the restriction that all variables must have valid entries. Notice that the restriction placed by having all the variables present creates a sample with fewer working wives and mothers, a potential source of bias.

Table B presents the specifications reported in Table I of FFO. Though the sample sizes differ by at most three observations and the coefficients are remarkably similar, the marginal effects of MAWORKH are consistently smaller in the replicated results. My results still show a positive and significant effect of mother-in-law's work behavior on daughter-in-law's labor force experience, between 12.6 and 23.8 percent, which is considerably smaller than the 15.7 to 32.3 percent effect shown in FFO. Similarly, footnote 21 in FFO cites "The correlation between a wife working and her mother-in-law working is 0.17, significant at the 1 percent level." I find the correlation in the full sample (column 3 of Table A) to be only 0.12 (sig. level 5.45 percent) and in the regression sample (column 2 of Table A) to be 0.11 (sig. level 12.3 percent). Though I am unable to exactly match the FFO sample and coefficients, my results are at least consistent with FFO's finding of a conditional correlation between mother-in-law and daughter-in-law work behavior.

In the GSS the respondent is chosen from among adults in a selected household randomly according to a sampling procedure, so there are approximately equal numbers of male and female respondents.[2] In my extended analysis, I include a parallel set of regressions for female respondents.[3] Though these regressions cannot address

[2]In the full sample there are 8,666 (52.74 percent) female and 7,767 (47.26 percent) male respondents. These numbers vary by specification but give a close approximation to the baseline sample available.

[3]In the survey, the respondent is asked to identify who he (she) considers the household head and his (her) relationship to that head. In order to ensure that the observation is for

the link between the work behavior of mothers-in-law and daughters-in-law, they do provide evidence contradicting the finding that there is no statistically significant relationship between the work behavior of mothers and daughters in FFO.

In FFO's analysis using the GSS, the key variable, MAWORKH, is defined from the survey question "Did your mother ever work for pay for as long as one year after you were born and before you were 14?" This variable was only collected in 1988, 1994, and 2002, and only the 1988 and 1994 surveys were used in FFO. The full sample of the GSS contains six separate variables that describe the working behavior of the respondent's mother: that the mother worked for as long as a year (1) after she was married, (2) before the respondent was in first grade, (3) after the respondent was born and before he/she was in first grade, (4) when the respondent was around age 16, (5) while the respondent was growing up, and (6) after the respondent was born and before the respondent was age 14. Each variable is only provided in some of the survey years, as shown in Table C.[4] Table C also provides

the core family in the household, I restrict the sample to heads of households and their spouses. Though most male respondents report that they are the head of the household, around 8 percent (over 600) of the male respondents in the sample responded that they are the spouse of the household head. Likewise, around 11 percent of female respondents in the married sample report that they are the household head. FFO restrict their attention to "male heads of households." For my extended analysis, I will include both household heads and spouses of household heads in the male respondent and female respondent samples.

[4]To demonstrate the overlap between these variables I compared MAWORK14 (did your mother work for as long as a year after you were born and before you were age 14) and MAWRKGRW (did your mother work for as long as a year while you were growing up). These variables seem qualitatively similar in what they are trying to capture. In the two survey years that have both variables, 1994 and 2002, the correlation between MAWORK14 and MAWRKGRW is 0.78. Eighty observations (8.3 percent) reported that the respondent's mother worked while growing up but NOT before age 14 and 27 observations (2.8 percent) reported that the respondent's mother worked before age 14 but NOT while growing up. The variables are slightly more consistent in 1994 than 2002.

the sample sizes and the means of the mother's work variables (restricted to having the other regression variables present) and the means of the wife's work variables by year.

The regression results from the extended GSS analysis are provided in Table D. It should be noted that I did not include data from the 2002 survey, since several variables are defined differently in that year and FFO did not use these data. The regression specifications that I present include only controls for husband and wife's age and husband's education (HUSB_AGE, HUSB_EDUC, WIFE_AGE) and the number of children under six in the household (BABIES).[5] I chose to omit the wife's years of education, since it is endogenous. Including WIFE_EDUC in either the men or women's samples only changes the coefficients on mother's work slightly and does not effect significance except in the women's column 5 (MAWKGRW). In the survey the respondent is asked about his or her own parents' education and contribution to household income, but no parallel spousal information was gathered. Controlling for the wife's income in the women's regressions does not make sense, since wife's labor force participation is the dependent variable. Similarly, household income is also correlated with the wife's labor force participation. Because of this, I chose to present results in Table D for men and women that were comparable, so I did not include any parental information or income variables. The sample is restricted to married, white men or women with the wife's age between 30 and 50 years.[6] I did not make a restriction on husband's age in either the male or female

[5]BABIES represented the number of children under age six in the household while CHILDREN is the number of children the respondent has ever had.

[6]There are two race variables present in the years prior to 2002, individual and house-

respondent sample.

Table D provides six specifications each for men and women using the different definitions of mother's work. Notice that the sample sizes vary dramatically between the columns, as only some survey years contain each of the variables. The means of the mother's work variables are provided in the bottom rows of Table C. Notice that the two variables MAWKBABY and MAWKBORN have similar means, around 35 percent of mothers worked under these definitions. Though MAWKBABY and MAWKBORN are defined similarly ("before first grade" versus "after born and before first grade"), the marginal effects are strikingly different in both the male and female samples. This could be due to real differences in how adding "after born" changes the responses or it could be a year effect. Dummy variables are included in every GSS specification to capture year fixed effects, but this only captures level differences between the years a particular variable definition is available and not across different years of the full sample. For the men's sample, the conditional correlation between mother-in-law and daughter-in-law work behavior is large and significant when mother's work is defined as after the son was born and before he was in first grade or after the son was born and before he was 14 years old. The marginal effect is insignificant if mother's work is defined as working for a year while the son was growing up. On the other hand, in the women's sample the mother/daughter

hold race. Though around 5 percent of the sample reports their individual race to be non-white and their household race to be white, these variables agree for just less than 80 percent of the sample. FFO do not specify which variable was used to restrict the sample to "white males." In both the replication results in Table B and the new regression in Table D, I restricted to respondents who answered white for both individual and household race, though the results were not sensitive to this decision.

conditional correlation is positive and significant when mother's work is defined as any time after marriage, after the daughter was born and before she was in first grade, or while the daughter was growing up. These results demonstrate that the relationship between the work behavior of mothers and daughters may also be significant, depending on specification, and that both mother-in-law/daughter-in-law and mother/daughter estimates are highly sensitive to specification.

FFO discuss sensitivity analysis on the GSS results on pages 1270-1272. They acknowledge that using MAWORK or MAWKBORN to define the mother's work variables make the coefficient on mother's work insignificant. FFO argue that this suggests sons preferences are formed later in their childhood (and that the "after marriage" variable is too vague to use). This may or may not satisfactorily explain why the variables "while growing up" or "around 16" are also not significant in the mothers-in-law/daughters-in-law regressions. These full results demonstrate that the findings that FFO present are not robust across different definitions of mother's work and different subsamples of the GSS and that the mother/daughter work behavior correlation may be just as important as that between mothers-in-law and daughters-in-law.

4.3 Evidence from the Marital Instability Survey (FLFPMI)

The second data set employed by FFO is the first wave of the Marital Instability over the Life Course survey, referred to as the Female Labor Force Participation and Marital Instability survey (FLFPMI). This survey of married persons was conducted in 1980 by telephone interviews. The respondent was chosen to be the husband or wife depending on the last digit of the telephone number (even for wife, odd for husband). Though the full sample (restricted to married white couples) has almost 1,800 observations, some of the regression samples are nearly half that size.

Unlike the GSS, I was able to almost exactly replicate the sample used in FFO, as shown in Table E. All values reported in column (1) of Table E match those included in Appendix 1 of FFO, with the exception of HUSB_INCOME. My replicated value for HUSB_INCOME has mean 23.14 (standard deviation 13.74) while Appendix 1 in FFO shows a mean of 23.0 (standard deviation 13.5).[7] Although the means are not exact, I obtain identical regression coefficients for this variable. In column (2) of Table E I include the means of the variables from the full sample (without restricting to having each variable present) to demonstrate any differences in the sample due to selection. The regression sample is slightly younger, has more education, and has fewer children, but is not dramatically different than the full sample.

In Table F I demonstrate an exact replication of the first four columns of Table II in FFO, with the exception that my log/likelihood row is different than that reported in FFO in all but column (ii). The next three columns of FFO's Table II include controls for religion and for father's socioeconomic status. Since the means of these variables were not presented in FFO, I do not know whether or

[7]In the text of FFO, HUSB_INCOME is described to be the husband's contribution to family income (defined by multiplying the husband's percent contribution by the family income) measured in thousands. Family income is taken as the midpoint of the categories with the top category multiplied by 1.2.

how the variables in my sample replicate those used in the original. I defined the PASOCEC variables from the father and father-in-law occupation status variables, as described in FFO. The religion variables consist of four dummies: PROTESTANT, CATHOLIC, NONE, and OTHER. A question about the respondent's religion is asked in the FLFPMI with these categories, but the spouse's religion was only identified as the same or different than that of the respondent. Religion could therefore be coded for most respondents, but only some of the spouses, which explains why the sample size decreases between columns (i) – (iv) and (v) – (viii). By construction, the observations dropped due to missing religion are more likely to have different religions than their spouse, a potentially important selection effect (note that the coefficient on MAWORKH more than doubles from earlier estimates in columns (vii) and (viii)).[8] My attempted replication of columns (v) – (vii) is compared to those from FFO in Table G. These columns are particularly important because wife's mother's work behavior, MAWORKW, is only included in columns (vi), (vii), and (viii). I did not attempt to replicate column (viii) because of the addition of two other controls, RESIDENCE and REGION. Though I could not replicate the FFO results (presented in Column 1 of Table G), my results are qualitatively similar.

In the FLFPMI analysis the main identifying variables, MAWORKH and MAWORKW, are defined from the variables labeled "Time Mother Worked When Respondent (Spouse) Grew Up." There are five possible responses to each of these

[8]Note that including a flag for the husband and wife having a different religion in addition to the religion variables or instead of the religion variables does not change the coefficient on MAWORKH.

questions: (1) All the Time, (2) Most of the Time, (3) About Half, (4) Less than Half, or (5) Never. FFO define MAWORKH/W as equal to 1 if the respondent answered "(1) All the time" and zero otherwise. Table H provides the frequency of responses coded as "husband" and "wife."[9]

Table I contains the results of two regression specifications using four sequential definitions of mother's work. The first is that used in FFO, the mother worked all the time. I then include the second category, so MAWORK2 equals 1 if the mother worked all or most of the time. The third category indicates whether the mother worked all, most, or about half the time. And the final category, MAWORK4, is zero if the mother never worked and 1 if the mother worked at all. Because of my inability to replicate the controls used in specifications (v) – (viii), the sensitivity analyses I present use a modified version of column (iv) that includes the wife's mother's work experience variable, MAWORKW, labeled (iv'). Notice that the regressions using the second definition of MAWORK, reported in columns (3) and (4) of Table I, show no significant effect of mother or mother-in-law's work behavior on the woman's labor force participation. On page 1276, FFO describe their findings under this test:

> If we use as an indicator of the husband's mother's working history not whether she worked "all the time" while her son was growing up, but instead whether she worked "most of the time," the mother's working behavior still enters positively and significantly in determining the probability that the son's wife works, but its marginal effect is about 11 percentage points.

[9]Questions about own and spouse's mother are asked of both male and female respondents, which are then combined and recoded as husband and wife variables. I found some evidence that men and women respond to these questions differently, but it does not appear that this has an impact on the estimated marginal effects.

This is not consistent with the findings I report in Table I. As it seems unlikely that the preference formation channel that FFO are attempting to detect should be sensitive to the difference in perception between mothers working all the time versus most of the time while the respondent or spouse was growing up, this finding calls the FLFPMI estimates into question.

The variable for whether the wife works is derived from the response to two questions in the survey. The first item reports whether the woman worked for pay with responses yes or no. If yes, a separate question asks whether the woman works (1) full time, (2) part time, or (3) both. The regressions thus far have restricted the definition of wife working to women who worked for pay (yes to the first question) and who reported working full time (response 1 in the second question) or working both full time and part time (response 3 in the second question). I test the sensitivity of the results to an alternative definition of whether the wife works in Table J. Here the dependent variable, WIFEWORK2, is equal to one if the wife worked for pay at all (responded yes to the first question), regardless of the response to the second question. I include specifications using the original FFO definition of MAWORK and those using the variable indicating whether the mother worked at all, MAWORK4. In the text of the paper on pages 1275 and 1276 and in footnote 35, FFO describe their results from a similar test:

> As before, our results are robust to alternative definitions of the dependent variable: whether we define a wife as working when she works full time or when she just works for pay, we obtain similar results. [Footnote 35:] The marginal effect of MAWORKH decreases to 17 percentage points using this looser definition but remains significant at the 1 percent level.

I found a smaller effect under this new definition of whether the wife worked which is significant under the restrictive definition of mother work and not significantly different from zero when mother's work is defined as working at all.

A final concern with the FLFPMI study is the neglect to address the range of valid ages. The GSS sample is restricted to women ages 30-50 because "women in this age interval are more likely to have completed their education and are still far from retirement considerations."(pg. 1267) However, no similar restriction was imposed in the FLFPMI section and the age range of women in the full sample was 15 to 55 years old. Table K illustrates how the responses to the mother's work question vary between three age ranges: 15 – 29, 30 – 50, and 51 – 55. The small sample sizes do not allow enough power to detect differences across samples.

Table L presents the results when the sample is broken down by the three age groups listed above. Notice that using the original FFO definition of MAWORKH and MAWORKW that mother-in-law's work behavior is only significant for the 30-50 year old sample while own mother's work behavior is now significant but only for the 15 – 29 and 51 – 55 sample. What is most surprising is that in the over 50 sample own mothers' work enters negatively under each definition of mother's work. The FLFMPI result of a zero effect of mother's work behavior on their daughter's labor force participation could be just a consequence of the averaging of a positive and negative effect for two different cohorts. The sample size (68 observations in the 51-55 year old wives sample) is extremely small for this type of analysis, so a larger data set might produce more accurate and precise estimates broken down by age. It is beyond the scope of this essay to determine why the marginal effects of own mother's work are different by age group, but further investigation of age and cohort effects is clearly warranted. The lack of attention paid to differences in marginal effects of maternal work behavior both across cohorts and throughout the life-cycle raises further concerns over the validity and robustness of the FFO findings.[10]

4.4 New Evidence from the Survey of Income and Program Participation (SIPP)

Because there is a discrepancy in the robustness of the FLFPMI results to alternative definitions of mother's work and because the zero coefficient on own mother's work experience is not consistent with evidence found in the GSS, I present results from a third data set, the Survey of Income and Program Participation (SIPP). The SIPP provides a much larger sample size, yielding more precise estimates. Although the SIPP does not directly report mother's work intensity, questions were asked about the occupation of the respondent's mother and work behavior can be inferred. The parental occupation questions were asked in the Family Background Topical Module conducted as part of Wave II of the 1986, 1987, and 1988 surveys. The data are therefore a pooled sample from these three cross-sections. The survey is answered by all members of the household (and proxy response is allowed), so we

[10]All coefficients reported in these tables represent how a marginal change in a independent variable affects the probability that the dependent variable (WIFEWORK) is equal to one. I present the results from measuring the marginal effect at the mean (considering binary variables as if they were continuous), as is standard practice (using Stata's DPROBIT command). An alternative method for calculating the marginal effect for binary explanatory variables involves instead evaluating the marginal effect for each observation, and then taking the mean over the entire sample. Marginal effects using this second method are virtually identical, so are not presented here.

have data for both the mother and mother-in-law as reported by the husband and wife.

In the survey the respondent is asked, "When ____ was 16, what was ____ 's (mother/stepmother's or person marked in TM8564) occupation?"[11] A flag variable (TM8576) equals to "-1" if the mother did not work and "0" if the mother did work. For this analysis I define mother's work (MAWORKH/MAWORKW) equal to 1 if the flag is set to zero and 0 if the flag is set to "-1." The dependent variable WIFEWORK is defined from the employment status recode variable that gives the woman's employment activities during the prior month (ESR-4). There are eight possible codes:

(1) With a job entire month, worked all weeks

(2) With a job entire month, missed one or more weeks, no time on layoff

(3) With a job entire month, missed one or more weeks, spent time on layoff

(4) With job one or more weeks, no time spent looking or on layoff

(5) With job one or more weeks, spent one or more weeks looking or on layoff

(6) No job during month, spent entire month looking or on layoff

(7) No job during month, spent one or more weeks looking or on layoff

(8) No job during month, no time spent looking or on layoff

WIFEWORK is set equal to 1 if the employment status recode is (1)-(5), 0 if the employment status recode is (8), and missing otherwise. This matches as closely as possible to the "wife working for pay full or part time" while eliminating women

[11] The variable TM8564 asks who the head of household when ____ was 16 was, so is only different from mother or stepmother if the respondent was living with a female head of household that was not the respondent's mother or stepmother.

who are unemployed and actively looking for work. The SIPP contains parental information for respondents between ages 25-64 only, so the sample is restricted to husbands and wives between those ages. Table M presents the means of the full sample and then of the sample broken down by wife's age. The education variables have four possible values, though the husbands and wives in our sample always report attending at least some school: (0) never attended school, (1) attended elementary and some high school, (2) has high school degree, and (3) has at least some college education. Because a woman's educational attainment is endogenous to her decision to work, as discussed above, I have chosen not to include WIFE_EDUC in all but the full specification of the SIPP.[12]

Table N presents the SIPP results using WIFEWORK as the dependent variable. The first two columns show the effects of mothers' work when entered independently, while the third and fourth column include both mothers' work variables. The first three columns include controls for husband and wife's age, husband's education, and the number of children. The fourth column includes controls for parental education levels.[13] We see in the first four columns of Table N that the effect of husband's mother's work appears to be slightly larger than wife's mother's work for the full sample, but the differences between the two coefficients are not statistically

[12]Inclusion of wife's education lowers the coefficients on the work behavior of the mother-in-law slightly and does not affect the estimate of the impact of the work behavior of the mother. As would be expected, education always enters positively and significantly when less than high school is the omitted category. It is interesting to note that inclusion of a woman's educational attainment has an asymmetric influence on the estimate of the impact of mother versus mother-in-law's work experience, but the effects are not statistically significant.

[13]Notice that in this analysis, education is treated nonlinearly, so that dummy variables are added for three categories, no school, less than high school, high school degree, and some college, as shown at the bottom of Table M.

significant in columns (3) or (4). Notice that the magnitudes of the estimated effects of mother and mother-in-law work decrease when both are included, suggesting there is at least some correlation. These results support the hypothesis that both mother and mother-in-law work experience significantly affects a woman's decision to work.

The next three columns of Table N show similar results broken down by wife's age. Recall that the SIPP only includes parental information for respondents ages 25-64, so the sample is thus restricted. In column (5) we see that for the sample of women ages 25-29, own mother's work experience is highly significant. Although the magnitude appears much larger than the effect of mother-in-law's work behavior, the coefficients are not statistically significantly different. Similarly, for women ages 30-50, the sample FFO identifies as most reliable due to concerns over childrearing and retirement, the effects of mother's and mother-in-law's work are both positive and statistically significant, and are again not statistically significantly different from each other. The sixth column of Table N shows that for women ages 51-64 there is no discernible effect of mother's or mother-in-law's work behavior on labor force participation, perhaps because levels are so low. When interpreting the age-group results in Table N, it is important to remember that the SIPP only contains 3 consecutive years of data from the 1980's, so it is impossible to isolate age and cohorts affects.

It is interesting to note in Table N that wife's age is statistically significant in each of the samples broken out by age group. This implies that the labor force participation of women is highly non-linear in age, as expected. The coefficients suggest a downwards U-shape to women's labor force participation, with rates increasing by age at first, then approaching flat, then decreasing. The final column in Table N includes higher order terms in wife's age. I include up to a third degree polynomial in wife's age, which is highly significant (tests including higher powers yielded insignificant coefficients for those variables). Notice that including a polynomial in wife's age does not significantly alter the coefficients on mother or mother-in-law's work behavior. This at least suggests that the heterogeneous affects of mother and mother-in-law's work behavior by age group on a woman's labor force participation is not being driven solely by differences in levels of female labor force participation. A more detailed analysis is necessary to identify the age-specific affects of mother and mother-in-law's work behavior, but the results presented in Table N suggest that a woman's age is an important factor in understand the relationship between a woman's labor force participation and that of her mother and mother-in-law.

The SIPP results confirm the conclusions drawn from an extended analysis of the GSS, that the working behaviors of women and their own mothers are correlated. This directly contradicts the conclusions FFO draw from the FLFPMI results. With this in mind, the intergenerational evidence presented in FFO could be reflecting the transmission from mothers to daughters as well as (or even instead of) that from mothers to sons.

4.5 Discussion and Conclusion

Perhaps the most surprising result in the empirical portion of FFO is that mother's and mother-in-law's labor supply are not highly correlated. There is a large literature on assortative mating, with many researchers finding strong correlations in observable characteristics of a husband and wife (see, e.g., Mare, 1991, Rose, 2001, Lam and Schoeni, 1993). I find that there is a strong correlation between the labor supply of a woman's mother and mother-in-law, which is more consistent with previous findings in assortative mating.

Lam and Schoeni (1993) present a model whereby a man's father-in-law's education level is a strong predictor of the man's earnings because it is correlated with unobserved characteristics of the man. In fact, in their empirical results using data from Brazil, they find that father-in-law's education level is a better predictor of a man's wage than the education level of his own father, once the man's schooling and other demographics are included as covariates. They present a model of assortative mating that suggests that unobserved characteristics of a man are more correlated with the man's father-in-law's schooling, since the effects of a man's father's schooling on his wages can be more completely captured through his own schooling level.

Future work will explore how a similar model to Lam and Schoeni's, which explicitly models the marriage market and assortative mating, can be employed to characterize the relationships between a woman's mother-in-law's labor supply, her own mother's labor supply, and her own choice to work. If the mother-in-law's labor force participation is correlated with unobserved characteristics of the woman, in a similar fashion to the wage equation in Lam and Schoeni (1993), then an alternative model of female labor force dynamics can be generated. This model would more explicitly address the role of assortative mating and of a woman's own preference formation. The empirical results presented here may better fit this type of model.

While the results presented here do not point to a clear answer about the relative strength of the relationships between mothers and daughters and mothers-in-law and daughters-in-law work behavior, they do suggest that the empirical results presented in FFO are not conclusive. FFO's model of preference formation in sons suggests that there is a causal link between a man's mother's work experience while he was growing up and the labor force participation of his wife. While some specifications support this story, the results are not robust to small changes in variable definitions and do not hold for all survey years within the data sets. In addition, the link between a woman's own mother's work experience while she was growing up and her labor force participation is found to be equally important.

The regressions presented in FFO and in this paper may be confounded by other elements of a woman's decision to enter the labor force which either mask or bias upward the estimated intergenerational correlations. While FFO do add controls for wife's age, these level effects may not adequately account for the changing life cycle labor force participation profile of women who may leave the labor force temporarily for child-rearing. The data span over 25 years of surveys, during which time the labor force participation of women (and mothers) has changed in ways that may not be properly controlled for by simply adding age. A more thorough study might investigate how the marginal effects differ by age cohorts (the preliminary analysis presented here suggests that this may be quite important) and by birth cohorts. Unfortunately, data constraints make distinguishing age versus cohort effects exceedingly difficult.

Another issue not accounted for in FFO (or in this essay) is selection into marriage and divorce. It is especially troubling that no age restriction was used in FFO's FLFPMI analysis, so women in the sample that reported ages as young as 15 years old were included. Although the correlation between young wives and their mothers-in-law may be an important piece, it seems likely that this relationship differs, not only throughout the lifecycle, but also between people who differ in unobserved ways that might also make them marry younger or be more likely to divorce. FFO emphasize the importance of religion in a woman's labor force participation decisions, since some religions may encourage or discourage woman to work. Since religion is likely to be highly correlated across generations, it might be an important mechanism by which mother and/or mother-in-law work behavior is correlated with a woman's labor force decisions. Though efforts were made to control for level effect differences across four major religion categories (including "other"), selection effects into the sample due to underreporting of religion may bias upward the marginal effects of maternal work experience.

The SIPP results (and the FLFPMI in some specifications) demonstrate that when the working behavior of both the mother and mother-in-law are included, the coefficients decline relative to when they are included alone. Because of assortative mating, we would expect mother and mother-in-law work experience to be at least somewhat correlated, so the inclusion of both should have this diminishing effect. However, in FFO the marginal effect of the husband's mother's work behavior actually increases when the wife's control variables (which include wife's mother's work behavior) are included. As FFO report, when mother's work is defined as "all the time" the correlation between mothers and mothers-in-law is "basically zero" at 0.05, though I find this is significant at the 5 percent level. In the SIPP, the correlation between mother and mother-in-law's work experience is 14.2 percent (significant at the 1 percent level). FFO describe their test as finding no evidence for the "network effect", where mother-in-law's work behavior is only significant because it is correlated to the mother's work behavior. More work is needed to establish how the correlation between mother and mother-in-law work experience is different by age and through time and especially how this correlation may confound measurement of the effects of mother and mother-in-law work behavior on a woman's work decisions.

While FFO present an interesting model to explain a mechanism by which women's labor force participation increases over time, the empirical findings are not as robust to specification changes as is indicated in the text. I find that a significant conditional correlation between mother-in-law and daughter-in-law work behavior can be found across several different surveys, but the level may be smaller and less robust than the results in FFO indicate. In addition, the importance of own mother's work experience in a woman's decision to work should not be discredited due to the FLFPMI results. The mother/daughter conditional correlation is found in both the GSS and the SIPP and is reasonably robust to specification changes. In light of this, the intergenerational effects posited in Section III of FFO can be reinterpreted as including the effect of intergenerational transmission from mothers to daughters in addition to that from mothers to sons. Future work will explore how a model based on assortative mating can more clearly characterize female labor force dynamics and can better explain the empirical findings.

Table 4.1: Comparison of GSS Samples

	FFO Appendix 1 N = 189		**Replication Sample N = 188**		**Full Sample With Missing Values**		
Variable	**Mean**	**S.D.**	**Mean**	**S.D.**	**N**	**Mean**	**S.D.**
WIFEWORK	.53	.50	.53	.50	406	.54	.50
MAWORKH	.50	.50	.49	.50	254	.52	.50
HUSB_AGE	41.00	6.40	41.40	6.93	414	42.46	7.59
HUSB_EDUC	14.40	3.00	14.27	2.88	414	13.90	2.85
HUSB_INCOME	33.40	21.20	32.35	20.28	366	32.73	22.36
WIFE_AGE	38.00	5.70	38.35	6.01	414	39.06	6.13
WIFE_EDUC	13.60	2.70	13.53	2.59	408	13.44	2.41
CHILDREN	2.20	1.30	2.21	1.39	413	2.24	1.38
MAEDUCH	11.30	3.10	11.29	3.16	380	11.31	3.05
PAEDUCH	11.10	3.80	11.02	3.78	346	11.08	3.70
BABIES	.37	.69	.37	.69	411	.36	.68

Notes: Variable definitions match the descriptions in FFO as closely as possible. WIFEWORK = 1 if the woman is employed full time or is temporarily away from her job due to illness, vacation, or strike. MAWORKH = 1 if the husband's mother worked for pay for as long as one year after he was born and before he was fourteen. HUSB_INCOME is the labor earnings of the male respondent in 1986 dollars, measured in thousands. CHILDREN is the number of children the respondent has ever had and BABIES is the number of children present in the household under six. The education variables are the number of years of schooling and have values between 0 and 20.

Table 4.2: Replication of FFO Table I, GSS Results

	Marginal Effects						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MAWORKH	.126*	.140**	.129*	.161**	.171*	.198**	.238**
	(.067)	(.065)	(.068)	(.072)	(.094)	(.097)	(.101)
HUSB_AGE	.004		.001	.010	.005	-.004	.010
	(.005)		(.008)	(.009)	(.006)	(.013)	(.015)
HUSB_EDUC	.015		-.006	-.023	.005	-.039*	-.051**
	(.013)		(.016)	(.017)	(.018)	(.023)	(.025)
HUSB_INCOME	-.007***		-.007***	-.007***	-.008***	-.008***	-.008***
	(.002)		(.002)	(.002)	(.003)	(.003)	(.003)
WIFE_AGE		.005	.004	-.007		.009	-.007
		(.005)	(.009)	(.011)		(.015)	(.017)
WIFE_EDUC		.026**	.045***	.054***		.087***	.091***
		(.013)	(.017)	(.018)		(.027)	(.030)
CHILDREN				-.093***			
				(.031)			
BABIES				-.192***			
				(.063)			
MAEDUCH					-.001	-.013	-.022
					(.020)	(.019)	(.022)
PAEDUCH					.015	.017	.027
					(.017)	(.017)	(.019)
RELIGION					Yes	Yes	Yes
INCOME					Yes	Yes	Yes
RESIDENCE					Yes	Yes	Yes
REGION					Yes	Yes	Yes
Observations	229	248	228	227	162	162	161
Pseudo R2	.054	.027	.077	.158	.183	.227	.293

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Table 4.2: *continued*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log/likelihood	-149.07	-165.91	-144.93	-131.60	-91.23	-86.36	-78.49

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. Marginal effects from probit models are reported. The dependent variable is whether the wife worked. Year fixed effects are included in each specification. Variable definitions match the descriptions presented in FFO as closely as possible, as described in the text. These results are for a sample where both individual and household race are restricted to be white. As in FFO, RELIGION is the religion in which the respondent was raised (3 dummies), INCOME is the respondent's self-assessment of relative family income at age 16 (3 dummies), RESIDENCE is the type of residence (city, farm, etc) the respondent lived in at age 16 (6 dummies), and REGION is the geographic region of the respondent's residence at age 16 (9 dummies).

Table 4.3: GSS Mother's Work Variables

YEAR	**Num** Obs	**Mean** Wife Work Work	**MAWORK** After Married	**MAWK BABY** Before 1st Grade	**MAWK BORN** After Born and Before 1st Grade	**MAWK 16** Around Age 16	**MAWRK GRW** While Growing Up	**MAWORK 14** After Born and Before Age 14
1975	428	0.27	X	X		X		
1976	434	0.24	X	X		X		
1977	862	0.34	X	X		X		
1978	846	0.29	X	X		X		
1980	764	0.34	X	X		X		
1982	748	0.35	X	X		X		
1983	808	0.35	X	X		X		
1984	682	0.34	X					
1985	740	0.38	X					
1986	700	0.35	X					
1987	666	0.44	X		X			
1988	658	0.39	X		X			X
1989	703	0.42	X		X			
1990	594	0.43	X		X			
1991	646	0.38	X		X			
1993	697	0.43	X		X			
1994	1214	0.46			X		X	X
1996	1002	0.48					X	
1998	1019	0.47					X	
2000	972	0.48					X	
2002	930	0.42					X	X
Men Sample		Mean	0.634	0.323	0.384	0.610	0.624	0.523
		N	2,093	461	599	479	951	262
Women Sample		Mean	0.699	0.363	0.342	0.691	0.658	0.533
		N	2,433	611	748	632	1,128	304

Table 4.4: GSS Alternative Mother's Work Variables and Women's Results

	MEN SAMPLE (Mothers-in-law)					
	(1)	(2)	(3)	(4)	(5)	(6)
MAWORK	-.006 (.024)					
MAWKBABY		.005 (.050)				
MAWKBORN			.114*** (.042)			
MAWK16				.006 (.047)		
MAWRKGRW					.011 (.034)	
MAWORK14						.178*** (.064)
HUSB_AGE	-.002 (.003)	-.001 (.006)	-.003 (.005)	.000 (.006)	.001 (.003)	.002 (.007)
HUSB_EDUC	.006 (.004)	.006 (.008)	.010 (.007)	.004 (.008)	-.008 (.006)	.001 (.011)
WIFE_AGE	-.003 (.003)	.001 (.007)	-.001 (.006)	-.000 (.007)	-.006 (.005)	-.010 (.009)
BABIES	-.177*** (.020)	-.154*** (.042)	-.133*** (.033)	-.159*** (.042)	-.156*** (.028)	-.279*** (.058)
Observations	2,093	461	599	479	951	262
Pseudo R2	.058	.055	.034	.055	.030	.086
Log/likelihood	-1356.21	-285.33	-397.70	-297.16	-63.25	-165.34

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Table 4.4: *continued*

WOMEN SAMPLE (Mothers)						
	(1)	(2)	(3)	(4)	(5)	(6)
MAWORK	.053**					
	(.023)					
MAWKBABY		.020				
		(.042)				
MAWKBORN			.080**			
			(.040)			
MAWK16				.019		
				(.042)		
MAWRKGRW					.065**	
					(.032)	
MAWORK14						.096
						(.059)
HUSB_AGE	-.004	-.002	-.001	-.002	.001	.008
	(.002)	(.004)	(.004)	(.004)	(.003)	(.006)
HUSB_EDUC	-.004	-.006	-.003	-.005	-.016***	-.016
	(.003)	(.007)	(.007)	(.007)	(.006)	(.011)
WIFE_AGE	-.001	-.006	-.008	-.006	-.003	-.007
	(.003)	(.006)	(.006)	(.006)	(.004)	(.008)
BABIES	-.155***	-.120***	-.223***	-.114***	-.202***	-.171***
	(.019)	(.036)	(.034)	(.034)	(.026)	(.053)
Observations	2,433	611	748	632	1,128	304
Pseudo R2	.043	.038	.059	.037	.061	.056
Log/likelihood	-1594.14	-395.68	-485.93	-409.39	-724.73	-195.57

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. Marginal effects from probit models are reported. The dependent variable is whether the wife worked. Year fixed effects are included in each specification, but are not reported.

Table 4.5: FLFPMI Sample

	Regression Sample (FFO)			**Full Sample**		
	(1)			**(2)**		
Variable	**N**	**Mean**	**S.D.**	**N**	**Mean**	**S.D.**
WIFEWORK	969	0.45	0.50	1574	0.46	0.50
MAWORKH	969	0.12	0.33	1747	0.12	0.32
MAWORKW	969	0.09	0.29	1760	0.11	0.31
HUSB_AGE	969	35.92	9.01	1786	37.01	9.35
HUSB_EDUC	969	14.53	2.70	1789	13.88	2.91
HUSB_INCOME	**969**	**23.14**	**13.74**	1682	22.88	13.83
WIFE_AGE	969	33.82	8.89	1786	34.65	9.15
WIFE_EDUC	969	13.77	2.17	1787	13.22	2.25
CHILDREN	969	1.92	1.48	1784	2.09	1.61
MAEDUCH	969	11.49	2.97	1489	11.37	2.99
PAEDUCH	969	11.36	3.68	1411	11.11	3.78
MAEDUCW	969	11.68	2.76	1586	11.39	2.84
PAEDUCW	969	11.60	3.57	1522	11.26	3.71

Note that the first column of this table matches the means and standard deviations presented in column 2 of Appendix 1 in FFO, with the exception of HUSB_INCOME. The second column is the raw means in the sample without restricting so that each variable is present. WIFEWORK = 1 if the wife was working for pay at the time of the interview. MAWORK = 1 if the husband (or wife)'s mother worked "all the time" when he (or she) was growing up.

Table 4.6: Replication of Columns (i)-(iv) of Table II FFO, FLFPMI Sample

Dependent variable is WIFEWORK				
	(1)	**(2)**	**(3)**	**(4)**
MAWORKH	.093**	.103***	.091**	.089**
	(.041)	(.039)	(.042)	(.042)
HUSB_AGE	.004**		.009**	.011***
	(.002)		(.004)	(.004)
HUSB_EDUC	-.002		-.025***	-.028***
	(.005)		(.006)	(.007)
HUSB_INCOME	-.012***		-.013***	-.012***
	(.001)		(.001)	(.001)
WIFE_AGE		-.002	-.005	-.002
		(.001)	(.004)	(.004)
WIFE_EDUC		.014**	.048***	.042***
		(.006)	(.008)	(.008)
CHILDREN				-.065***
				(.011)
Observations	1454	1535	1453	1449
Pseudo R2	.060	.007	.081	.098
Log/likelihood	-943.75	-1052.14	-921.96	-902.50

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects from probit models are reported. The dependent variable is whether the wife worked.

Table 4.7: Replication of Columns (v)-(vii) of Table II FFO, FLF-PMI Sample

	FFO Reported Marginal Effects			Replication of FFO			No Religion, PASOCEC Cntrls		
	(v)	(vi)	(vii)	(v)	(vi)	(vii)	(v)	(vi)	(vii)
MAWORKH	.092 (.048)	.164 (.052)	.211 (.063)	.113 (.050)	.125 (.044)	.162 (.054)	.094 (.047)	.118 (.043)	.126 (.051)
HUSB_AGE	.006 (.002)		.001 (.008)	.006 (.002)		.004 (.006)	.005 (.002)		.004 (.005)
HUSB_EDUC	-.001 (.007)		-.034 (.011)	-.003 (.007)		-.019 (.009)	-.001 (.006)		-.017 (.008)
HUSB_INC	-.013 (.001)		-.011 (.002)	-.012 (.002)		-.012 (.002)	-.013 (.002)		-.013 (.002)
WIFE_AGE		-.002 (.002)	.004 (.008)		-.001 (.002)	.004 (.006)		-.001 (.002)	.003 (.005)
WIFE_EDUC		.016 (.009)	.049 (.014)		.022 (.008)	.047 (.011)		.017 (.007)	.043 (.010)
MAEDUCH	.003 (.007)		.011 (.011)	-.001 (.007)		.003 (.008)	.004 (.007)		.006 (.008)
PAEDUCH	.001 (.006)		-.019 (.010)	.001 (.006)		-.005 (.007)	.004 (.006)		-.003 (.006)
PASOCECH	.0003 (.0007)		.002 (.001)	.001 (.001)		.001 (.001)			
MAWORKW		.001 (.064)	-.056 (.082)		-.021 (.051)	-.0002 (.063)		-.020 (.048)	-.003 (.056)
MAEDUCW		.006 (.008)	.007 (.010)		.006 (.007)	.002 (.008)		.009 (.006)	.01 (.007)
PAEDUCW		-.009 (.007)	-.0002 (.009)		-.008 (.005)	-.004 (.007)		-.014 (.005)	-.010 (.006)
PASOCECW		-.002 (.001)	-.002 (.001)		-.002 (.001)	-.001 (.001)			
PRTSTH	yes		yes	.199 (.068)		.330 (.206)			

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Table 4.7: *continued*

	FFO Reported Marginal Effects			Replication of FFO			No Religion, PASOCEC Cntrls		
	(v)	(vi)	(vii)	(v)	(vi)	(vii)	(v)	(vi)	(vii)
CATHH	yes		yes	.151 (.076)		.247 (.250)			
NONEH	yes		yes	.309 (.075)		.131 (.286)			
OTHERH	yes		yes	drop		drop			
PRTSTW		yes	yes		.221 (.055)	-.312 (.159)			
CATHW		yes	yes		.156 (.063)	-.265 (.163)			
NONEW		yes	Yes		.310 (.069)	drop			
OTHERW		yes	Yes		drop	-.215 (.224)			
Observations	1072	796	530	1010	1200	825	1120	1258	972
Pseudo R2	.062	.026	.106	.075	.029	.107	.062	.012	.086

Robust standard errors in parentheses, *'s omitted because of space constraints. Marginal effects from probit models are reported. The dependent variable is whether the wife worked.

Table 4.8: FLFPMI Frequencies of Mothers' Work Behavior

	Husband			**Wife**		
MOTHER'S WORK WHEN GROWING UP	**Freq**	**Pct**	**Cum. Percent**	**Freq**	**Pct**	**Cum. Percent**
(1) ALL THE TIME	205	11.73	11.73	188	10.68	10.68
(2) MOST OF TIME	195	11.16	22.9	227	12.9	23.58
(3) ABOUT HALF	188	10.76	33.66	193	10.97	34.55
(4) LESS THAN HALF	242	13.85	47.51	275	15.63	50.17
(5) NEVER	917	52.49	100	877	49.83	100
Total	1,747	100		1,760	100	

Table 4.9: Sensitivity Tests FLFPMI, Mother's Work

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Baseline						
	(i)	(iv')	(i)	(iv')	(i)	(iv')	(i)	(iv')
WORKED ALL THE TIME:								
MAWORKH	0.093**	0.096**						
	(0.041)	(0.042)						
MAWORKW		0.003						
		(0.045)						
ALL OR MOST OF THE TIME:								
MAWORKH2			0.012	0.013				
			(0.032)	(0.033)				
MAWORKW2				0.023				
				(0.033)				
ALL, MOST, OR HALF THE TIME:								
MAWORKH3					0.064**	0.065**		
					(0.028)	(0.029)		
MAWORKW3						-0.021		
						(0.029)		
WORKED AT ALL:								
MAWORKH4							0.051*	0.048*
							(0.027)	(0.028)
MAWORKW4								-0.020
								(0.028)
HUSB_AGE	0.004**	0.011***	0.004**	0.011***	0.004***	0.012***	0.004***	0.012***
	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)
HUSB_EDUC	-0.002	-.027***	-0.002	-.027***	-0.002	-.027***	-0.002	-.027***
	(0.005)	(0.007)	(0.005)	(0.007)	(0.005)	(0.007)	(0.005)	(0.007)
HUSB_INC	-.012***	-.012***	-.012***	-.012***	-.012***	-.012***	-.012***	-.012***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)

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Table 4.9: *continued*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Baseline						
	(i)	(iv')	(i)	(iv')	(i)	(iv')	(i)	(iv')
WIFE_AGE		-0.001		-0.001		-0.002		-0.002
		(0.004)		(0.004)		(0.004)		(0.004)
WIFE_EDUC		0.041***		0.041***		0.041***		0.041***
		(0.008)		(0.008)		(0.008)		(0.008)
CHILDREN		-.067***		-.067***		-.066***		-.067***
		(0.012)		(0.012)		(0.012)		(0.012)
Observations	1454	1428	1454	1428	1454	1428	1454	1428
Pseudo R2	0.060	0.097	0.058	0.095	0.060	0.097	0.059	0.096
Log/likelihood	-943.75	-890.37	-946.29	-892.64	-943.75	-890.26	-944.53	-891.27

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. Marginal effects of the probit on whether a wife works are presented.

Table 4.10: FLFMPI Age Test Tabulations

	HUSBAND					
	WIFE AGE 15-29		**WIFE AGE 30-50**		**WIFE AGE 51-55**	
MOTHER'S WORK	Freq	Pct	Freq	Pct	Freq	Pct
(1) ALL THE TIME	80	13.16	116	11.13	9	9.57
(2) MOST OF TIME	75	12.34	110	10.56	9	9.57
(3) ABOUT HALF	82	13.49	101	9.69	5	5.32
(4) LESS THAN HALF	104	17.11	129	12.38	9	9.57
(5) NEVER	267	43.91	586	56.24	62	65.96
Total	608	100	1,042	100	94	100

	WIFE					
	WIFE AGE 15-29		**WIFE AGE 30-50**		**WIFE AGE 51-55**	
MOTHER'S WORK	Freq	Pct	Freq	Pct	Freq	Pct
(1) ALL THE TIME	60	9.82	124	11.8	4	4.21
(2) MOST OF TIME	92	15.06	125	11.89	10	10.53
(3) ABOUT HALF	86	14.08	101	9.61	6	6.32
(4) LESS THAN HALF	119	19.48	146	13.89	10	10.53
(5) NEVER	254	41.57	555	52.81	65	68.42
Total	611	100	1,051	100	95	100

Table 4.11: FLFPMI Age Test Regressions

	Ages 15-29	Ages 30-50	Ages 51-55	Ages 15-29	Ages 30-50	Ages 51-55
	(1)	(2)	(3)	(4)	(5)	(6)
MAWORKH	.074	.151***	-.112			
	(.069)	(.057)	(.185)			
MAWORKW	.169**	-.083	-.291*			
	(.075)	(.054)	(.149)			
MAWORKH2				-.051	.065	-.018
				(.055)	(.043)	(.153)
MAWORKW2				.123**	-.025	-.215
				(.054)	(.042)	(.141)
MAWORKH3						
MAWORKW3						
MAWORKH4						
MAWORKW4						
HUSB_AGE	.011	.011**	.103**	.012	.011**	.090**
	(.008)	(.005)	(.042)	(.008)	(.005)	(.040)
HUSB_EDUC	-.046***	-.021***	-.048*	-.047***	-.021***	-.048*
	(.013)	(.008)	(.027)	(.013)	(.008)	(.027)
HUSB_INC	-.017***	-.011***	-.010*	-.017***	-.011***	-.010*
	(.003)	(.002)	(.006)	(.003)	(.002)	(.005)
WIFE_AGE	.007	.004	-.073	.005	.004	-.061
	(.013)	(.005)	(.059)	(.013)	(.005)	(.059)
WIFE_EDUC	.046***	.035***	.019	.046***	.036***	.023
	(.017)	(.010)	(.038)	(.017)	(.010)	(.038)
CHILDREN	-.150***	-.058***	-.022	-.153***	-.057***	-.012
	(.030)	(.014)	(.035)	(.029)	(.014)	(.034)
Observations	503	857	68	503	857	68
Pseudo R2	.143	.100	.158	.143	.095	.156
Log/likelihood	-298.90	-53.22	-38.45	-298.81	-533.21	-38.55

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Table 4.11: *continued*

	Ages 15-29	Ages 30-50	Ages 51-55	Ages 15-29	Ages 30-50	Ages 51-55
	(7)	(8)	(9)	(10)	(11)	(12)
MAWORKH						
MAWORKW						
MAWORKH2						
MAWORKW2						
MAWORKH3	.028	.099***	.049			
	(.049)	(.038)	(.145)			
MAWORKW3	.023	-.048	-.117			
	(.049)	(.038)	(.139)			
MAWORKH4				.032	.073**	.015
				(.048)	(.036)	(.136)
MAWORKW4				.049	-.057	-.181
				(.049)	(.036)	(.133)
HUSB_AGE	.010	.013***	.091**	.010	.013***	.096**
	(.008)	(.005)	(.038)	(.008)	(.005)	(.040)
HUSB_EDUC	-.046***	-.021***	-.051*	-.046***	-.021***	-.058**
	(.013)	(.008)	(.028)	(.013)	(.008)	(.029)
HUSB_INC	-.017***	-.011***	-.008	-.016***	-.011***	-.007
	(.003)	(.002)	(.005)	(.003)	(.002)	(.006)
WIFE_AGE	.007	.002	-.072	.008	.002	-.068
	(.013)	(.005)	(.056)	(.013)	(.005)	(.058)
WIFE_EDUC	.044**	.036***	.022	.044**	.037***	.031
	(.017)	(.010)	(.037)	(.017)	(.010)	(.038)
CHILDREN	-.151***	-.057***	-.010	-.152***	-.057***	-.012
	(.029)	(.014)	(.034)	(.029)	(.014)	(.036)
Observations	503	857	68	503	857	68
Pseudo R2	.136	.100	.146	.137	.099	.159
Log/likelihood	-301.40	-53.41	-39.02	-30.97	-531.32	-38.44

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. Marginal effects of the probit on whether a wife works are presented. MAWORK = 1 if mother worked "all the time", MAWORK2 = 1 if mother worked "all the time" or "most of the time", MAWORK3 = 1 if mother worked "all the time", "most of the time", or "about half", and MAWORK4 = 1 if mother worked "all the time", "most of the time", "about half", or "less than half."

Table 4.12: SIPP Means - Part 1

	Full Sample (N = 12,869)		**Restricted Sample on Wife's Age**					
			Wives 25 - 29 (N = 1,894)		Wives 30 - 50 (N = 8,122)		Wives 51 - 64 (N = 2,853)	
Variable	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**
WIFEWORK	0.648	0.478	0.673	0.469	0.693	0.461	0.504	0.500
MAWORKH	0.380	0.485	0.463	0.499	0.397	0.489	0.278	0.448
MAWORKW	0.413	0.492	0.488	0.500	0.434	0.496	0.301	0.459
HUSB_AGE (25-64)	43.502	10.797	30.248	4.007	41.474	7.424	58.072	4.196
WIFE_AGE (25-64)	41.079	10.433	27.167	1.385	38.942	5.925	56.398	3.683
CHILDREN	2.304	1.489	1.344	1.089	2.241	1.322	3.119	1.721

Table 4.13: SIPP Means - Part 2

	FULL SAMPLE				
	Never Attended	**Some Elem. or High**	**High School Degree**	**Some College**	**Total**
HUSB_EDUC	0	2,105	4,411	6,353	12,869
	0.00%	16.36%	34.28%	49.37%	100.00%
WIFE_EDUC	0	1,786	5,511	5,572	12,869
	0.00%	13.88%	42.82%	43.30%	100.00%
MAEDUCH	179	3,179	3,831	1,617	8,806
	2.03%	36.10%	43.50%	18.36%	100.00%
PAEDUCH	188	3,691	2,752	1,838	8,469
	2.22%	43.58%	32.49%	21.70%	100.00%
MAEDUCW	176	3,888	3,970	1,784	9,818
	1.79%	39.60%	40.44%	18.17%	100.00%
PAEDUCW	182	4,113	2,922	2,125	9,342
	1.95%	44.03%	31.28%	22.75%	100.00%

Table 4.14: SIPP Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
	Full Sample				Wife's Age			Non-linear Wife's Age	
					Wives 25 - 29	Wives 30-50	Wives 51-64		
MAWORKH	.038*** (.009)		.035*** (.009)	.031** (.014)	.040* (.022)	.039*** (.011)	.010 (.021)	**MAWORKH**	.035*** (.009)
MAWORKW		.031*** (.009)	.027*** (.009)	.029** (.013)	.071*** (.022)	.023** (.010)	-.005 (.021)	**MAWORKW**	.025*** (.009)
HUSB AGE	.001 (.001)	.001 (.001)	.001 (.001)	.000 (.002)	-.007** (.003)	-.001 (.001)	.001 (.003)	**HUSB AGE**	-.001 (.001)
WIFE AGE	-.004*** (.001)	-.004*** (.001)	-.004*** (.001)	-.003* (.002)	.023*** (.008)	.006*** (.001)	-.032*** (.003)		
CHILDREN	-.035*** (.003)	-.035*** (.003)	-.035*** (.003)	-.029*** (.005)	-.140*** (.011)	-.049*** (.004)	-.006 (.006)	**CHILDREN**	-.042*** (.003)
HUSB HSD	.070*** (.012)	.069*** (.012)	.069*** (.012)	.043* (.025)	.041 (.037)	.071*** (.016)	.054** (.024)	**HUSB HSD**	.063*** (.013)
HUSB COL	.072*** (.012)	.073*** (.012)	.072*** (.012)	-.016 (.027)	.039 (.037)	.057*** (.016)	.042* (.024)	**HUSB COL**	.058*** (.012)
WIFE HSD				.089*** (.026)					
WIFE COL				.180*** (.028)					
MAEDUCH				yes				**(WIFE AGE/ 100)**	-8.551*** (2.245)
PAEDUCH				yes				**(WIFE AGE/100)2**	27.704*** (5.375)
MAEDUCW				yes				**(WIFE AGE/100)3**	-26.797*** (4.137)
PAEDUCW				yes					
Observations	12,869	12,869	12,869	5,605	1894	8122	2853		12,869

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Table 4.14: *continued*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample				**Wife's Age**			**Non-linear Wife's Age**
					Wives 25 - 29	Wives 30-50	Wives 51-64	
Pseudo R2	.025	.025	.026	.035	.088	.020	.040	.047
Log/likelihood	-8135.0	-8137.9	-8130.4	-3402.9	-1091.6	-4907.4	-1898.4	-7959.2

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1% Marginal effects of the probit model are presented. The dependent variable is WIFEWORK, as described in the text. Dummy variables for the four education levels of husbands and wives parents are included in the fourth specifications, none of which had significant coefficients. The last three columns present results disaggregated by the age of the wife.

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