Abstract

We investigate the question of whether investing in a child's development by having a parent stay at home when the child is age 5 or younger is correlated with the child's outcomes in adulthood. Specifically, do children with stay-at-home mothers have higher adult earnings or are they more likely to work in a given year in adulthood? We find few significant differences between the adult earnings of children with stay-at-home mothers and those of children with working mothers. We do, however, see a significant positive effect of a low-educated mother working when her child is between the age of 14 and 18 on the child's labor force participation in adulthood. These results are similar for both sons and daughters. We also find that mothers' work decisions are correlated with intergenerational mobility estimates, with boys of working mothers having more mobility and girls of working mothers having less.

JEL Classifications: J13, J22, J24

Keywords: human capital, child development, female labor supply

Is There an Advantage to Working? The Relationship between Maternal Employment and Intergenerational Mobility*

Peter Gottschalk and Martha Stinson†

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1 Introduction

The rise in the labor force participation rate of women, including women with young children, has led to a sharp increase in the proportion of children being raised by working parents[1]. As a result, parents have increasingly relied on purchased inputs, such as day care, as substitutes for the time they would have devoted to their children had they been stay at home parents. Whether the increased reliance on market inputs has had a long term effect on these children is the question we investigate in this paper.

A large body of literature has found a range of results on the impact of a mother working. Some studies have shown that the children of working mothers have lower mean outcomes on a variety of indicators of later success, such as reading and math scores in early elementary school (Ruhm (2004), Baker, Gruber, Milligan (2008), Waldfogel, Han, and Brooks-Gunn (2002))

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†Peter Gottschalk is professor of economics at Boston College. Contact information: Economics Department, Boston College, Chestnut Hill MA 02467; peter.gottschalk@bc.edu. Martha Stinson is an economist at the U.S. Census Bureau, Social, Economic, and Housing Statistics Division. Contact information: 4600 Silver Hill Road, Washington, D.C. 20233; martha.stinson@census.gov.

[1]BLS statistics published in "Women in the Labor Force: A Databook" show a steady increase in the number of employed women with children under the age of six, ranging from 33% in 1975 to 43% in 1982 to 58% in 2012, the latest year for which data are available.

while others have found limited or no effect (Blau and Grossberg (1992), Baker and Milligan (2010), Dustmann and Schonberg (2012)). But this evidence on the sign and magnitude of changes in indicators of later success, such as test scores in school, is not evidence on the sign and magnitude of later success itself, as measured by adult outcomes, such as labor supply or earnings. Our paper fills this gap by using a longitudinal data set that is sufficiently long to allow us to measure the mother's work history before the child was born, during the preschool years (0-5 years old), and throughout the K-12 education period, as well as the child's earnings 30 years after birth. These rich data allow us to examine the relationship between the mother's work history and the child's adult labor market outcomes directly rather than having to rely on intermediary child outcomes as predictors of the child's adult outcome.

As with all papers in this literature, estimating the causal impact of the mother's work behavior on the child's adult outcomes when maternal employment is almost certainly endogenous is challenging. High ability mothers presumably command a higher wage in the labor market and hence are more likely to work. However, they might also be more likely to have high ability children. This will produce a spurious correlation between the mother's work behavior when the child is young and the child's adult earnings. We follow the literature by conditioning on a rich set of observables, including family income of the parents and estimated parameters classifying each parent in the earnings distribution based on their earnings history[2]. We also estimate a within-mother fixed-effects model that relies on variation in mother employment choices over time that differentially impacts siblings and an IV model that uses geographic variation in child care costs and female labor force participation rates to instrument for maternal employment. We consider as well the impact of mother job characteristics such as industry and firm size and the role of birth order and maternal attachment to the labor force prior to the child's birth. We find positive effects on child labor supply in adulthood that are statistically significant but we find few significant effects of a mother working on the child's adult earnings.

These unique longitudinal data also allow us to put our study in the context of the broader literature on intergenerational mobility. As we have argued, the rapid increase in the labor force participation of mothers may have had a causal impact on the adult earnings of their children. If so, then the higher earnings of the working mothers and the higher earnings of their children would lead to a decline in intergenerational mobility. In the limit, children of high earning mothers would become high earners themselves. Children's earnings would be completely determined by parent's earnings, with the result that there would be no intergenerational mobility.

But even if mothers' work had no causal impact on their children's' adult outcomes, the recent rapid increase in labor force participation of mothers could

[2] These parameters are random effects, estimated from log linear earnings regressions using REML. These random effects are not the typical random effects used by economists but rather are predicted values resulting from solutions to Henderson (1953) mixed effects equations. See Appendix A and Searle, Casella, and McCuloch (1992) for details. The appendix to this paper summarizes this literature.

also have a purely mechanical impact on intergenerational mobility. Whether standard measures of intergenerational mobility would increase or decrease as a result of the increased labor force participation of mothers, depends on which mothers started to work. If the mothers who entered the labor market were the mothers of children who would grow up to have high earnings (whether or not the mother worked), then this would look like an decline in intergenerational mobility. The distribution of working mothers and their children would again include a greater proportion of parent-children pairs with similar earnings. If, on the other hand, it was mothers with disadvantaged children who increased their labor force participation, then this would lead to an increase in intergenerational mobility. Our ability to measure the children's adult earnings with our dataset allows us to estimate this purely mechanical impact on intergenerational mobility as well as attempt to identify behavioral links between the mother's increased labor force participation and the child's adult earnings.

Our results are mixed. For girls with working mothers at age 1-5, there is a stronger relationship between daughter and father earnings, meaning less mobility. This may be related to labor supply issues. Girls with working mothers are more likely to work themselves which induces a higher correlation with father earnings. For boys of working mothers, the relationship between father and son earnings is not statistically different than the relationship for boys of non-working mothers. However, the relationship between family and son earnings is different for families with varied mother labor force participation. Sons who had steadily employed mothers during their early childhoods had lower correlation with family earnings, meaning increased mobility.

Our paper proceeds as follows: first, we discuss the relevant literature and our contributions; second, we present a simple analytical framework that focuses on the factors that influence the parent's decision whether to invest their own time in their child's future earnings or to rely more heavily on bought inputs; third, we describe our statistical methods for handling endogenous mother labor force participation and for estimating intergenerational mobility; fourth, we describe our data; fifth, we discuss results. We conclude with an assessment of what we learn from this work.

2 Literature Review

This study builds on the vast literature on intergenerational mobility and specifically on that part of the literature that explores the impact of mother's work on children's' later outcomes. The literature on intergenerational mobility, reviewed recently in Jantti and Jenkins (2014) shows clearly that measurement error has a large impact on measures of intergenerational mobility. The seminal articles by Solon(1992) and Haider and Solon(2006) show the importance of having sufficiently rich data to correct for measurement error when constructing measures of intergenerational mobility. Measures of parental income must be adjusted for life cycle changes in income and for transitory fluctuations in income, which add noise to the data. For example, Solon shows that using a five year average of parental income, rather than a single year, lowers estimates of intergenerational mobility by up to 70 percent.

Ideally one would have the full lifetime earnings of both the parent and the child to construct measures of intergenerational mobility. This would allow the researcher to adjust both for life-cycle changes in income and for transitory fluctuations. Recent work has begun to exploit administrative data to study intergenerational mobility in the United States. Chetty, Hendren, Kline, Saez, and Turner (2014, hereafter CHKST) use IRS tax returns to look at children born between 1971 and 1993 and linked to their parents as a dependent on the parents' tax return. They rank children at age 30 and parents in the years the child was age 15-19 in the overall income distribution and then estimate the rank-rank correlation which is the slope coefficient from an OLS regression of child rank on parent rank. CHKST interpret this slope as the difference in income rank at age 30 for a child from the poorest family compared to a child from the wealthiest family. Their estimates are in the range of .3. We build on this influential analysis by considering the relationship between mobility and maternal employment using similar methods and data.

Our narrower focus on the impact of mothers working when their children are young also builds on prior studies. Time spent at home by the mother in the early years of a child's life provides more opportunity for direct parental involvement in child human capital production but comes at the cost of lower family earnings. Much recent literature (see Almond and Currie (2010) for a summary) has focused on whether early childhood investment (or negative shocks) produces effects that can be measured later in life. Some of this work, such as Conti and Heckman (2012), suggests that all human capital investments are not equal and investments early in life are particularly effective at producing good long term outcomes for children. The benefit of early investment in children is supported by many studies that find small negative effects of maternal employment in the first five years of a child's life on early childhood educational outcomes. For example, Bernal and Keane (2010), Berger, Hill, and Waldfogel (2005), Ruhm (2004), Baker, Gruber, and Milligan (2008), Han, Waldfogel, and Brooks-Gunn (2001), Waldfogel, Han, and Brooks-Gunn (2002), and Gregg, Washbrook, Propper, and Burgess (2005) all find small negative impacts of maternal employment during preschool years on the cognitive test scores of children into their early elementary school years while Blau and Grossberg (1992) and James-Burdumy (2005) find negative effects of employment in the first year of the child's life. Baker and Milligan (2010) is an exception and finds no effect of early maternal employment. Blau and Currie (2004) find a positive effect of maternal employment on school-age children if those children participate in high-quality after school care. The major drawback of all these studies is that they are only able to follow children for a relatively short amount of time and leave unanswered what the long term effects of maternal employment are. Only Ruhm (2008) follows children as far as age 11 and finds mixed results of maternal employment depending on the education of the mother.

In contrast, the effect of income on child development is also generally found to be positive as reported by Blau (1999), Baum (2003), and Dahl and Lochner

(2012). This is not surprising as higher income means more resources to spend on purchasing inputs into child human capital production. However some of the effects estimated in these studies are quite small, which may explain why maternal employment is almost never reported to have a positive effect. It is possible, though, that maternal income loss does not affect early childhood outcomes but does have a longer lasting effect as children age. If the mother is absent from the labor market for a significant number of years and reduces her earnings potential by lowering her experience levels, family resources may be lower when the child is in high school or college, when purchased inputs are more important. Many maternal employment studies may not extend long enough to capture the full effect.

Interest in longer run outcomes has given rise to recent papers using European data to follow children late into high school, with heavy reliance on changes in maternity leave laws and administrative data. Rasmussen (2010), Liu and Skans (2010), Carneiro, Loken, and Salvanes (2011), Dustmann and Schonberg (2012), and Bettinger, Haegeland, and Rege (2014) all use variation in maternal employment caused by changes in length of time and cash benefits allowed for maternity leave in Denmark, Sweden, Norway, Germany, and Norway, respectively, to estimate the effect on child high school GPA and graduation rates. Dunifon, Hansen, Nicholson, and Nielsen (2013) also use Danish administrative data but rely on various econometric techniques to estimate the casual effect of mothers working on high school outcomes. The results of these studies are mixed, with Rasumussen (2010) and Dustmann and Schonberg finding no effects of maternal employment, Liu and Skans (2010), Carneiro, Loken, and Salvanes (2011), and Bettinger, Haegeland, and Rege (2014) finding benefits to mothers staying home, and Dunifon, Hansen, Nicholson, and Nielsen (2013) finding positive effects of mothers working. We build on this literature by looking at children's labor market outcomes in their early thirties and by using administrative data from the United States. We control for maternal employment and family income at every stage of the child's life and make use of firm-level administrative data to better classify the type of job held by the mother. Our primary contribution is to estimate a relationship between maternal employment and a measure of adult well-being directly and to consider the impact on intergenerational mobility.

3 Analytical Framework

3.1 Endogeneity Concerns in a Basic Model

In a static household utility maximization framework, mothers choose amounts of time to spend at home with their children and in paid employment, conditional on the wage they are able to earn in the labor market and their child-human-capital production abilities[3]. The solution to the utility maximization

[3] In a dynamic framework, a mother will base her labor supply decisions not only on the current period wage but also on the effect of working this period on future wages. This makes

problem stipulates that the marginal utility of the marginal increase in child human capital will equal the cost of that increase, the mother's foregone wages. Likewise, market inputs into the child's human capital production function will be expanded until the marginal gain is equal to the marginal cost, subject to the amount of money available to spend given the labor supply decision and money earned by other household members. A mother will work until the marginal return of an additional hour would not buy adequate market goods to replace her time in the production of child human capital. Likewise, a mother will spend time with her child until the increase in child human capital is too small to compensate for the lost consumption. Thus labor supply will vary across mothers in a cross section due to differences in the wage a mother can earn, differences in child raising ability, differences in the mother's utility from child human capital, and differences in the utility of consumption. Parents who are highly productive in the labor market will tend to substitute away from own time spent with children towards purchased inputs which include day care arrangements and also extra activities such as SAT prep classes, music lessons, and academic enrichment activities. Other parents who earn less in the labor market but are equally productive in child care will stay at home with their children since their opportunity cost of staying home is low.

Ideally we would like to estimate how much money a woman needs to earn to compensate for lost time at home working in child human capital production. However this is not straight forward because child human capital also has a genetic component and we expect a positive correlation between parent and child ability, independent of time inputs. Since parent ability is largely unobserved and is correlated with labor market participation, there is likely to be a spurious correlation between working and child outcomes. Considering how to hold unobserved ability constant is thus the thrust of any econometric specification that uses cross-sectional covariation in mother's work choices and eventual labor market outcomes of the child.

Another confounding factor in measuring the return to maternal time investment is the ability and earnings of the father. A father's unobserved ability both helps to determine his earnings and exacerbates the problem of unmeasured genetic transmission of ability to the child. The father's earnings in turn produce variation in the mother's work choices due to income constraints. If the father's earnings are sufficiently high that the marginal benefit of additional consumption has been driven below the benefit of increased child human capital, the mother will choose not to work. If we could control for men and women's abilities, we could compare high-earning men who have a wife who stays at home to men with working wives who together earn the same amount and could estimate the maternal time/earnings trade-off in relation to eventual child outcomes. A negative relationship between women working and child outcomes would mean that family income needs to rise in order to compensate for the mother being gone from home. However without such controls, any type of marriage sort-

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the utility maximization problem more complicated but does not change the fundamental trade-off the mother faces between producing child human capital and earning money for consumption.

ing pattern that tends to link women of a particular ability level to men of a particular ability level (for example assortive mating) causes this comparison to break down. Taken together, these issues make it essential to also account for father unobserved ability and family income in addition to mother unobserved ability when estimating returns to the mother's time at home.

3.2 Identification Strategies

We hypothesize the following linearized version of the relationship between the child's adult earnings, Y_{it}, and mother labor force participation when the child is young, W_i :

$$Y_{it} = X_{it}\beta + \gamma W_i + \theta_i + \eta_{it} \tag{1}$$

where X_{it} represents time-varying characteristics of the child, θ_i represents time-invariant unobserved child characteristics, and η_{it} captures time-period specific variation. The key to obtaining consistent estimates of γ, the effect of the mother working, is to control for parents' ability in some way which takes account of likely correlation between W_i and the unobserved θ_i. In other words, we need to find variation in parent work patterns that is independent of the parents', and hence the child's, unobservable traits. We describe four methods that we employ, each commonly used in the literature on maternal employment.

First, as described in equation 2, we control for parent characteristics, specifically mother and father education levels, average family earnings during segments of the child's life, and an estimate of parental ability from a parent earnings equation.

$$\begin{aligned} Y_{it} &= X_{it}\beta^c + \gamma W_i + \beta^1 MotherEducation \\ &\quad + \beta^2 \widetilde{\theta_i^m} + \beta^3 FatherEducation + \beta^4 \widetilde{\theta_i^f} + AverageFamilyEarnings + \eta_{it} \end{aligned} \tag{2}$$

To create parental ability estimates, we first use a mixed-effects model of the following form to estimate earnings equations for mothers and fathers:

$$Y^m = X^m\beta + Z\theta^m + \eta \tag{3}$$

$$Y^f = X^f\beta + Z\theta^f + \eta \tag{4}$$

We describe mixed-effects models in detail in Appendix A. These models are more general than the fixed effects or random effects models common in the econometrics literature. In particular they do not impose orthogonality between X and Z and they not only provide $\widehat{\beta}$ but also $\widetilde{\theta_i^m}$ and $\widetilde{\theta_i^f}$. We treat these predicted values of the random effects, $\widetilde{\theta_i^m}$ and $\widetilde{\theta_i^f}$, as measures of mother and father unobserved ability. By definition they are centered at zero and rank mothers and fathers relative to each other in terms of earnings that are not explained by observable characteristics. For fathers we use all earnings years from 1978 to 2011 to estimate these effects. For mothers, we use earnings from years between when the youngest child turned 11 and 2011 in order to try to mitigate the impact of unobserved part-time work which deflates earnings in a way unrelated to ability. We control for parent age, race, education, labor force experience, industry, firm size, and calendar year in X. Our end year of 2011 was chosen due to the availability of the firm size and industry information. More detail about the firm characteristics data is given in Section 4.

Next we consider an IV estimation strategy that relies on state-year-level female labor force participation rates, state-level per capita counts of child care facilities, and state-level averages of payroll per employee at child care facilities. These variables represent local conditions faced by the mother that may cause exogenous shifts in labor supply if mother geographic location is not correlated with mother and child unobserved ability. Our hypothesis is that if child care is more plentiful or cheaper or jobs are more plentiful and working is more common among a woman's peers, then she will be more likely to work.

Third, we consider sub-groups of parents and children that are likely to be more similar to each other than our sample in general. In order to compare parents with the same labor force attachment, we restrict our sample to children with fathers who worked every year when the child was between the ages of 1 and 5 and mothers who worked the year before the child was born. Next, we further restrict our sample to oldest children. For these children, the mother was already employed and faced the first decision of whether to stay home or work. The father was steadily employed and so the effect of the mother's decision can be looked at more independently.

We also attempt to make use of the birth of a younger sibling to identify some exogenous variation in mother labor supply. For example, consider a working mother who uses purchased inputs to replace some of her time when she has only one child. If she has an additional child the following year, there is the possibility that the marginal utility of her time in child human capital production now exceeds her wage because time at home helps two children and hence she will choose to stay home. This suggests that parents change their labor market behavior in response to the externalities from taking care of an additional child. Some parents gain greater externalities by expanding their own input into their children's future labor market outcomes than by expanding bought inputs. Hence working parents will decrease their labor market activity when the second child is born if the economies of scale for own inputs are sufficiently high relative to bought inputs. This variation, however, is only exogenous if parental decisions about spacing between births is unrelated to unobserved ability. If high ability mothers who are more likely to work tend to space their children further apart to diminish costs of purchasing care for preschoolers, the birth of a second child may not have an exogenous impact on mother labor supply.

In spite of this concern, we make use of the presence of a younger sibling in several ways. First we include in our IV model an indicator for presence of a sibling under age 5 for every year from age 1 to age 18 as another predictor of mother labor supply. This measure captures both older siblings not yet age 5 when the child in question is born and younger siblings who follow after the child's birth. Next, we consider the sample of oldest children whose mothers worked when the child was age 1 and whose fathers worked every year from when the child was age 1 to age 5. Among these, we select a sub-sample of children who had younger siblings born before they turned 5. We then include an indicator for whether the mother worked the full first 5 years of the oldest child's life or stopped sometime after age 1. While this model is not a natural experiment, it is in the spirit of Bettinger et al. (2014) who use a change in Norwegian maternity leave payments to look at what happens to older siblings when parents choose to stay home after the birth of a younger child. The large increase in government payments to stay-at-home mothers changed the relative costs of working and staying home and created an exogenous change in mother labor supply which the authors exploit to determine the effect on the older sibling. In our model, under the assumption that the birth of a second child when the first child is still under age 5 is not an endogenous event, we hypothesize that variation across states in child care costs might produce variation in mother labor supply which we can exploit to determine the impact on the oldest child.

Finally we use a mother fixed-effects model to estimate the effect of the mother working from within-family variation. We estimate this model using a sub-sample of sons with brothers and daughters with sisters also born in the window of five years when we have available a long enough time series to see the children turn 30 (see Section 4 for details). Having children born relatively close together is an advantage because family conditions in general were likely to have been similar for these siblings. However these restrictions do produce a small sample both for sons and daughters and hence this estimation strategy may suffer from lack of precision due to not enough observations. In this model, mother and father fixed-effects are one and the same as we observe families at a point in time when they were interviewed by the SIPP and use biological children who had lived with both parents their whole lives. Thus there is no possibility for the siblings to have different fathers.

In addition to earnings, we consider labor force participation as another child outcome variable. We estimate logistic models to predict annual participation from the child's 18th year to calendar year 2012. For these models we use the same parent controls and also split our sample into more homogeneous subgroups.

Our final analysis uses our longitudinal earnings history to estimate rank-rank correlations between parent and children earnings in the same manner as CHKST (2014). We calculate average earnings between ages 28 and 32 and between ages 43 and 47 for both mothers and fathers[4]. We then rank the fathers of children in our sample relative to all SIPP 1984, 1990-1993, 1996 panel male respondents born in the same birth cohort and ever having children. We divide the men into cohorts born between 1923-1929, 1930-1935, 1936-1940, 1941-1947, 1948-1950,1951-1955, 1956-1966. For sons, we calculate average earnings between ages 28 and 30 and rank them relative to other male SIPP respondents born between 1978 and 1982. We follow the same process for

[4]Since our time series begins in 1978, there are some parents who are already past the ages of 28-32 or 43-47 in this year. For these individuals we use their 1978-1982 earnings to replace the unavailable years.

mothers and daughters. Finally we create a measure of average family earnings by adding the mother and father earnings each year when the father was age 28 to 32 and age 43 to 47 and averaging. After obtaining each individual's rank, we regress the child's rank on the father's rank, the mother's rank, and the family earnings rank. We interact the parent/family rank with an indicator for whether the mother worked all years when the child was age 1 to 5 and with an indicator for whether the mother worked some but not all years when the child was age 1 to 5. These interactions will allow us to determine the effect of the mother working on mobility measures.

4 Data

We use a previously unexploited source of data, the Survey of Income and Program Participation (SIPP) linked to the Social Security Administration (SSA) and Internal Revenue Service (IRS) Detailed Earnings Record Extract (DER) which contains W-2 earnings records from 1978-2012. These linked data provide the two essential pieces necessary to study our question. From the survey, we obtain links between parents and children and from the DER, we obtain a longitudinal history of labor market outcomes that spans much of the lives of the family members. For our sample, we select children born between 1978 and 1982 because, for this group, we can observe parents' earnings from the year of the child's birth forward, and, at the same time, the children turn 30 by the end of our earnings time series in 2012. We also selected children who were observed to be living with both of their biological parents during a SIPP panel between 1984 and 1996 at which point they ranged in age from 5 to 18 years old[5]. For children with step-parents, the SIPP does not interview the non-resident biological parent and thus we are unable to obtain a W-2 earnings history for this parent. This prevents us from calculating total family earnings prior to the survey and for this reason we exclude children with step-parents from our analysis. We also exclude children where either parent was less than 16 or the mother was 50 or older or the father was 56 or older at the time of the child's birth, out of concern for data error in the survey-reported family relationships.

We rely on the W-2 earnings to measure labor force participation and earnings for all family members before, during, and after the survey. We follow parents' earnings and labor force participation from the birth of the child through early adulthood and construct annual measures of maternal and paternal labor force participation and total family earnings from age 0 (birth year) to age 18 of the child. Children are also followed forward through 2012, and we construct

[5] For the 1984 panel we imposed one additional restriction and took only children who were born in 1978 and 1979. This restriction ensured that the kids were at least 5 years old at the beginning of the SIPP panel and that the family had not broken up during the first 5 years of the child's life. For the 1990 panel, kids range in age from 12 to 8, for the 1991 panel ages 13-9, for the 1992 panel ages 14-10, for the 1993 panel ages 15-11, and for the 1996 panel ages 18-14. An 18 year old child living with his or her parents in a SIPP panel conducted in the 2000s will not be 30 by 2012 so we did not use children from any later SIPP panels.

the same labor force participation and earnings variables for them. Unfortunately we do not observe final schooling outcomes or eventual family formation decisions of the child because the SIPP panel ends before any child is older than 22 years old and we have no available administrative data for these topics.

Our linked survey-administrative database is an internal data product created by the Census Bureau and is called the SIPP Gold Standard File (GSF). It contains all SIPP respondents from the 1984, 1990, 1991, 1992, 1993, 1996, 2001, 2004, and 2008 SIPP panels. For a subset of the questions asked by the survey, consistent variables are created across all nine panels. For the panels we use in this paper, the Census Bureau asked each SIPP respondent at the time of the survey to provide a Social Security Number (SSN). SSA then compared demographic information (name, sex, race, and date of birth) from the survey reports and the Numident, an administrative database containing demographic information collected upon issuance of the SSN. If a respondent's name and demographics were deemed to match between the two sources, the SSN was declared valid. For individuals where a validated SSN was obtained, they were then linked to IRS and SSA administrative data on earnings, OASDI, and SSI benefits[6]. For individuals where no validated SSN was obtained, we multiply-impute their earnings history to create four separate data sets with no missing values, called completed implicates. Results for this paper were obtained using the first completed Gold Standard implicate. A future draft will use the remaining three implicates to create more accurate measures of variance using the standard Rubin formulae for variance estimation using multiple implicates. In our sample, 18.6% of sons had missing SSNs, with 10.2% of their fathers, and 11.3% of their mothers also having missing SSNs. The corresponding percentages for daughters were 21.3%, 11.2%, and 11.8% respectively.

Parent and child relationships are taken from the household roster compiled at the time of the interview[7]. From the survey, we also make use of the mother's self-reported fertility history to determine if the child was the oldest, youngest, or a middle child. We use self-reported race of both the parents and the children and the self-reported education of the parents. For date of birth, we use the date recorded in the Numident, preferring this as the more reliable measure instead of the survey self-report. We define a person as working in a given year if he or she has positive W-2 annual earnings, and we create years of labor force experience measures by summing these work indicators over time. Mother's age at birth

[6] The SIPP GSF is the base file used to create the SIPP Synthetic Beta (SSB), a public-use product that uses data synthesis methods to protect the confidentiality of the linked data and provide access outside secure Census facilities.

For more information on the SSB and how to use these data, see http://www.census.gov/programs-surveys/sipp/methodology/sipp-synthetic-beta-data-product.html

[7] In the 1984 - 1993 SIPP panels, the relationship between a child and one parent is reported in the core data files by including the parent person number on the child's record. Most commonly this parent identifier links to the mother. The second parent's information must be obtained from the topical module that reports household relationships in a matrix form, i.e. person A's relation to person B, person C., etc. Beginning in 1996, links to both parents are included on a child's record in the core files and the topical module is not necessary for defining the parent-child relationship.

of the child is the difference between the mother and child administrative birth dates.

Our final data source is the Longitudinal Business Database (LBD) which is a research version of the Census Business Register, edited to be longitudinally consistent over time. These data contain industry, firm size, and payroll information for almost all of the businesses in the United States from 1976-2011. Firms are identified by an Employer Identification Number (EIN) which in turn links to the W-2 record of an individual worker. Utilizing this SSN-EIN link, we merge LBD data about firm characteristics onto the earnings histories of mothers and fathers in our sample and create an annual summary that measures percentage of earnings during the year in each firm size and industry category. We use these for three purposes. First, annual measures of firm size and industry sector serve as control variables in the parent mixed effects models that we use to estimate the random parent effect that proxies for ability. Second we calculate the number of years a mother spent in various industry groups and firm size categories during the first 5 years of a child's life and include these more detailed work measures in the child outcome regressions. For individuals without SSNs, we are unable to match them to the LBD because we do not yet have a methodology to multiply impute assignment to a particular firm. Thus for the child outcome regressions, we drop children whose mothers do not have valid SSNs when we include LBD variables. For the parent earnings equations, we assign earnings-year observations without firm size or industry information to a "missing category." Finally we use the LBD to calculate the number of child care businesses (NAICS code = 62441) per capita and average payroll per employee at these businesses by state for every year from 1978 to 2011. We merge this onto each parent's annual records using the state of the employer (EIN) where they worked in a year. If a parent worked at more than one employer we merge earnings-weighted average state values. If the state of the employer was unknown either due to missing information in the LBD or failure to link to the LBD due to missing parental SSN, then we used a state-female population weighted national average. These variables become the instruments in our IV estimation. For each child, we create a measure of local conditions for each age from 1 to 18 by choosing the state-level variables merged to the father's record for the appropriate year, or if they are missing, the mother's. If both parents are missing, we are then forced to use the population weighted national average. Just over 20% of sons at age 1 have parents with missing state of employment and so have the population weighted national average assigned.

In Table 1A we show summary statistics for our sample of boys with overall means and standard deviations given in the far right column. Of the almost 7,000 boys in our sample, 8% are black, 38% are the first-born child of their mother, and their mother's average age at the time of their birth was 26.9 years. Among these sons, 51% had mothers with only a high school degree or less, and 45% had fathers with a high school degree or less. Average total mother and father earnings when the boy was age five or under was approximately $61,000 in 2012 dollars.[8] At age 30, average total earnings of these boys was around $45,000, in 2012 dollars, and average labor force experience was 12.7 years. The average predicted father random effects were .01 on average whereas the predicted mother random effects were just slightly below zero on average. By design in the mixed effects model, the estimated mother and father random effects have zero mean when averaged across mothers. However in this table we are averaging across sons and hence the average changes because some mothers and fathers have multiple sons in our sample, effectively re-weighting the random effects. This result means that mothers with lower $\widetilde{\theta^m}$ had more sons and fathers with higher $\widetilde{\theta^f}$ had more sons.

To see whether there are observable differences between sons with working mothers and those whose mothers stay home, we divide our sample into three groups: 1. sons with mothers who never worked when the son was age 5 and under; 2. sons with mothers who worked some years when the son was age 5 and under; 3. sons with mothers who worked all years when the son was age 5 and under. Hereafter we refer to these groups as No Work, Some Work, and All Work. We report means for boys in the No Work group and the differences in means between the Some Work group and the No Work group and between the All Work group and the No Work group. These comparisons highlight observable differences in a single dimension while allowing us to differentiate between different types of maternal working patterns. For most characterstics, sons in the Some Work and All Work groups are different from sons in the No Work group in statistically significant ways. They are more likely to be black, oldest children, have younger fathers, and have siblings under the age of 5. Their mothers are more educated but have lower random effects. Their mothers continue to work more years throughout the son's school-age years. Sons in the All Work group on average have higher family earnings when they are preschool age and sons in both the All Work and Some Work groups have higher family earnings on average when they are school age. The differences for father's education are more muted. Sons in the All Work group are more likely to have a father with at least some college whereas most of the differences in education levels for the Some Work group are not siginficant. However fathers of sons in both the All Work and Some Work groups have lower random effects on average. There are no statisically significant differences in average son earnings between the No Work group and either of the Work groups but sons in the All Work group have worked, on average, about half a year longer by age 30 than sons in the No Work group.

In Table 1B we report the same summary statistics for our sample of girls. The most noteworthy thing about this table is that our sample of approximately 6600 girls looks remarkably similar to the sample of boy in Table 1A in almost every respect. Race, oldest child status, mother age at birth of daughter, parental education, average combined parent earnings when the daughter was

[8] While this number for total family earnings may seem high, we remind the reader that our sample of boys comes from parents who remain together in the same household till the son is at least 5 years old and more often in his early teens.

under 5, and percentages of mothers who worked are very similar on average to boys. Similar trends hold that girls in the All Work and Some Work groups also have more highly educated mothers (14 and 6 percentage points difference in college degree category respectively) compared to the No Work group; slightly more educated fathers (6 and 5 percentage points difference in some college category); and fathers with lower unobserved ability. The only major disimilarity between the genders is in average earnings at age 30, where the mean is lower for girls than boys. This is true despite boys and girls having similar levels of work experience. We expect this result is probably due in part to unobserved labor supply differences. Girls may work fewer hours per week at age 30 than boys due to child care responsibilities. Unfortunately our administrative data do not contain labor supply measures so we cannot differentiate among various potential causes of lower earnings.

While our data set clearly expands our knowledge by providing the long time series on earnings necessary to observe the mother's labor market attachment when the child is young and to observe the child's labor market outcomes 30 years later, like all data sets this one has limitations. Like many previous studies, we are also not able to control for quality of purchased child care services. Furthermore, we cannot determine what happens to family structure after the end of the survey nor observe any other child outcomes besides labor market participation and earnings. There are currently no published weights for this sample because it is drawn from five different SIPP panels and hence our sample is not nationally representative and our estimates cannot be interpreted as applying broadly. We also have no sub-annual information available on labor supply. However, there is much to recommend these data. Our sample size is relatively large, we have long histories of earnings which are potentially more reliable than self-reports about earnings and work decisions from the far past, and we know a great deal about the history of the family over a time period that covers the important early years of a child's life.

5 Results

We begin in Table 2 with our first results from earnings regressions for sons. This sample of sons includes any who met our original sample restrictions and had positive earnings in at least one year at age 30 or older, which reduces our sample by about 900 boys. This leaves us with 6080 total sons with 16,731 earnings years at age 30 or older. In column one we show results from our most simple regression which, in addition to mother work indicators, includes only the following son characteristics: age, age squared, black, oldest child, youngest child, age at birth of the mother, and year indicators for 2008 through 2011. We specify the mother work variables as indicators for working at age 0, age 1, age 2, age 6, and age 13, and counts of the number of years worked between age 3 and 5, age 7 and 10, age 11 and 12, and age 14 to 18. We chose these grouping after examining an initial regression with separate indicators for each age from 0 to 18 and then grouping together the ages that had similar coefficients. These coefficients on the number of years a mother worked in a certain child-age range can be thought of as the average effect of the mother working one year while the child was between these ages. The only significant effects for the mother working in column 1 are at age 6 and age 14-18, with positive coefficients in both cases. The coefficient on years of work between age 1 and 5 for the dad is also significant and positive. However in column 2 when we add controls for parent education, random parent effects from our parent earnings regressions, and average family income at age 1 to 5, 6 to 10, 11 to 13, and 14 to 18, the work coefficients are no longer significant. This leads us to believe that these initial positive coefficients were indeed bias introduced by unobserved parental ability being correlated with unobserved son characteristics. The coefficients on the mother working at age 1 and 2 are negative in both columns and similar in magnitude but in neither case are they significant. The parental controls behave as expected with higher levels of education and higher random effects for both mothers and fathers being correlated with higher son earnings.

In Table 3 we show results from an IV estimation using the full sample. In order to successfully estimate the first stage equations, we had to combine the mother work variables into four categories: years worked when child age 1-5, when child age 6-10, when child age 11-13, and when child age 14-18. While again there are no mother work coefficients that are statistically significant, we see that the estimated relationship between mother work at child age 14-18 and child earnings is now negative, further strengthening our belief that the initial positive coefficient was the result of unobserved parent heterogeneity. Working when the child is preschool age also has a negative relationship and the point estimate is larger than for the age 1, age 2, and age 3-5 estimates of Table 2, column 2. However the standard errors are large enough that this difference is certainly not statistically significant and we cannot draw any certain conclusions from these IV results. We test for weak instruments using the standard F-test that measures the joint significance of the excluded exogenous variables (in our case female labor force participation, child care centers per capita, and payroll per employee at child care centers by state) in the first stage regression. While they are clearly significant, the F-statistic is not sufficiently high to alleviate all concerns about weak instruments as Stock, Wright, and Yogo (2002) argue that only values of 10 or higher reliably mean there is not a weak instrument problem.

In Table 4, we show results that are equivalent to Column 2 in Table 2 with the sample being split by mother education. Again, we see no significant coefficients on the mother work variables. The indicator for mother working at child age 1 has a positive coefficient for mothers with at least a college degree and a negative coefficient for mothers with only some college or a high school degree or less. However given the size of the standard errors, these differences are almost certainly not statistically significant. Again parental education and ability variables seem to be much more strongly correlated with son earnings than mother work indicators.

In Table 5, we show results from several sub-samples of sons in an effort to group sons who are more similar. In the first column, we restrict ourselves to sons of mothers who were working the year before the son was born and fathers who worked every year from when the son was age 1 to 5. The results are similar to those in Column 2 in Table 2. Although the sign on mother working at child age 1 has changed, there are no statistically significant effects. In Column 2, we further restrict our sample to oldest children whose parents had strong labor force attachment. These coefficients have the same signs and similar magnitudes as Column 1, again with no significant effects. Finally in Column 4, we include oldest children whose mothers worked when the child was age 1, whose fathers worked every year when the child was age 1 to 5, and who had a younger sibling born before the child turned 5. We include an indicator for whether the mother worked all years between age 1 and 5 with the alternative being that the mother quit working at some point before the child reached age 5. Our hope was that the arrival of the second child might provide some exogenous variation in mother employment if different mothers face different costs of going back to work after having a second child. However, while the effect of the mother working when the child was under age 5 is negative and the largest point estimate of any of the specifications thus far, the uncertainty of the estimate is large enough that we cannot say with confidence that it is significantly different from zero.

In Table 6, we present results from our mother fixed-effects model where we use variation in mother work choices across brothers to identify the effect of maternal employment at young ages. The signs on all the coefficients are negative except for age 1 and age 14-18 but again none of them are significant. Finally in Table 7, we show results from regressions where we include years of mother working in various industry and firm size categories when the son was age 1 to 5. This regression seeks to measure whether the effect of a mother working varies depending on what types of jobs she holds. The manufacturing group includes the NAICS sectors of agriculture and fishing, mining, utilities, construction, manufacturing, wholesale trade, and transportation and warehousing; the service 1 group includes information, financial, insurance, real estate, professional, scientific, and technical service; education includes only the education sector; health includes only the health care sector (which also includes day care centers); service 2 includes retail, arts and entertainment, food and accommodations, and other; government includes public administration and actual government jobs; self-employment indicates the presence of earnings filed using IRS 1040 Form, Schedule C, and missing indicates lack of industry information on the LBD. The firm size categories are (1) under 10 employees, (2) 10-25 employees, (3) 26-50 employees, (4) 51-100 employees, (5) 101-200 employees, (6) 201-500 employees, (7) 501-1000 employees, and (8) more than 1000 employees. Generally the industry categories have positive coefficients and the firm size categories have negative coefficients but none of them are statistically significant.

All these results from son earnings equations lead us to believe that there is no significant correlation between mother labor force participation and son earnings outcomes, once we have controlled for parental characteristics correlated with mother, father, and son ability levels. There may be a number of reasons for this result. Perhaps our sample is too small, perhaps we have not been able to adequately identify exogenous variation in mother labor supply, or perhaps our annual measure of labor supply are too coarse to pick up effects. It is also possible that there truly are no effects of mother employment once we account for earnings. If mothers are truly optimizing their hours of work and their hours at home, one would expect that they would work until the benefit from additional income arising from another unit of time spent at a job would be equal to the benefit of additional production of child human capital from another unit of time spent at home and the net effect of working would be zero.

In Table 8 we consider the relationship between maternal employment and child labor force participation. We estimate a logistic regression using presence of W-2 earnings as an annual indicator for the son working between the ages of 18 and age in 2012. In our first simple model, analogous to column 1 in Table 2, we see that the mother working when the son was between the ages of 14 and 18 is correlated with an increased likelihood of the son working after the age of 18. Even when we add parental controls in Table 7, column 2, we see that this correlation persists. To get an idea of the magnitude of this effect, we exponentiate this coefficient to get an odds ratio of 1.06 meaning that boys are about 6% more likely to work if their mother worked when they were in high school. In Table 9, we see that this effect is only significant for sons of mothers with a high school degree or less. Of particular interest in Table 8 is the lack of significance of any parental controls. In Table 9, we see that father education is only significant for the sample of sons with mothers who have college degrees or higher level of education.

In Tables 10-17, we present results for daughters that are very similar to those for sons. An initial positive correlation between mother working when the daughter was age 14-18 and daughter earnings from age 30 onward becomes insignificant when parental controls are included. There is a significant negative correlation between working when the daughter is age 3-5 if the mother has a high school degree or less (Table 12), but for no other education groups are there any statistically significant effects. For the sub-sample of oldest-child daughters with mothers who worked at age 1, fathers who were continuously employed during the years the child was under the age of 5, and siblings who were born before the daughter turned five, the initial estimate of the impact of the mother working all 5 years is positive (Table 13, Column 3) although not significant. Future work will test to determine if the results change when endogeneity is taken account of through IV methods. In terms of labor force participation, the results are again very similar to those for boys. The mother working when the daughter is in high school is positively and significantly correlated with the daughter having positive earnings from age 18 onward (Table 16, Columns 1 and 2). However this effect is only significant for daughters who have a mother with a high school degree or less (Table 17). For mothers with some college education, mothers working at age 1 and age 2 are both significantly correlated with daughter work decisions. The net effect of these two coefficients is .016 but the standard error of the sum is .152 so this net effect is not significantly different from zero.

Finally, we report measures of intergenerational mobility in Table 18. As one might expect, for sons, the correlation with father earnings in higher than the correlation with mother earnings. In fact the correlation between mother average earnings from ages 28 to 32 and average son earnings from 28 to 30 is not significantly different from zero. For both parents, the correlation is higher when earnings are measured at older ages for the parents. Having a working mother between the ages of 1 and 5 does not alter any of these correlations in a statistically significant way. However, when we consider family earnings, we do see differences by mother work status. If the mother worked every year when the son was ages 1 to 5, the correlation between family earnings and son earnings is reduced by between 3.4 and 4.6 percent. However if the mother only worked some years, there is no change in the rank correlation.

For daughters, there is again no significant correlation between their average earnings from age 28 to 30 and mother average earnings for ages 28 to 32, while there is a significant correlation with father average earnings at these ages. Unlike sons, the correlations between mother and father average earnings at ages 43 to 47 and daughter earnings are similar to each other and there is a significant difference in the correlation with father earnings if the mother worked every year during the first 5 years of the daughter's life. For daughters, having a working mother increases the correlation with father earnings, lowering mobility. This is likely the result of increased labor force participation on the part of daughters with working mothers, as we saw in Table 16. For daughters, there is no significant difference in the correlation between child and family earnings by mother work status.

Our rank slope estimates range from .13 to .24, lower estimates than the .3 found by CHKST (2014). There are two likely reasons for this difference. First, we use only W-2 earnings and not total family income to measure our correlations. While income is a better measure of general well-being, we feel it is also important to consider specifically how children fare in the labor market relative to their parents. Second, our families are all intact families with both biological parents present at the time of the interview. Thus our families are likely somewhat advantaged relative to the sample used by CHKST and this may contribute to higher estimates of mobility. Given these differences, our highest correlation estimate of .24 for family earnings and son earnings is closer than might have been expected to the CHKST estimate, which gives us some confidence that our SIPP sample is similar in other characteristics to the IRS tax-return population sample.

6 Conclusion

Given the dramatic increase in labor force participation of mothers with young children, it is not surprising that considerable attention has been paid to possible impacts on later outcomes of the children of these newly working mothers. Did these children benefit or were they harmed when mothers went to work and spent less time with them?

A very standard human capital model predicts that if mothers' work decisions were constrained by social norms or discrimination, then relaxing these constraints would lead some mothers to enter the labor market and substitute bought inputs for own time raising their children. If these mothers place value on better outcomes for their children then their decision to increase their labor force participation would lead to better outcomes for their children. While the argument is straightforward, the evidence is at best mixed. The existing literature spans the spectrum. Some studies show that children of working mothers have better outcomes than children of stay at home moms, while others report no difference or even mild declines.

A major limitation of all these studies has been that they cover relatively short time periods. Children's outcomes can only be measured a few years after their mothers' labor market activity. In this paper are able to overcome this major impediment to measuring children's outcomes when they become adults and their mothers labor force status when they were young. Our ability to link administrative earnings data to the mother and the child's records allows us to measures of the parental input and the child's adult outcomes many years apart.

With this data we were able to estimate standard models that deal explicitly with the endogeneity of the parents' work decision. Before controlling for correlated unobservables across generations, we find statistically significant correlations between the mother's work decisions and the child's labor outcome as an adult. However, once we control for endogeneity, these correlations largely disappear.

This finding is consistent with two possible interpretations. First, mother's inputs into the child's human capital production function when the child is very young may have little impact on the child's outcomes 30 years later. This explanation could be consistent with the large literature by Heckman and others that shows that early childhood investments do have an impact on children's pre-teen outcomes if these effects fade over time. Our ability to look at adult outcomes for the children suggests that these early childhood interventions may not have long term impacts.

An alternative interpretation of our empirical finding is that there is heterogeneity both in production functions and in mother's input. Some children benefit more from having a stay at home mom while others benefit more from having a working mother who buys child rearing goods and services. Mothers may also differ in their child rearing and market skills. This heterogeneity in production functions and in mother's skills may vary not only over children but also across time. For example, a mother may be particularly skilled at raising her children when they are very young but not when they are teenagers. This heterogeneity in production functions and mother's skills would lead to heterogeneity in optimal outcomes. Comparing outcomes of children with stay at home moms with the outcomes of children of working mothers, would show no difference even though mothers' work decisions did have an impact.

Our estimates of intergenerational mobility based on the rank correlation approach used by Chetty et al. tell a mixed story. For a son, it appears that having a working mother when he is young may help increase mobility, perhaps by providing additional resources for human capital production. However for a daughter, her mother's decision to work early in her life is correlated with less mobility. More work is needed to disentangle the sample selection effects of which mothers are working, whether this is different for sons and daughters, and how this relates to mobility measures.

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8 Appendix A

In this Appendix we describe the estimation of the mother and father random effects that we use to control for unobserved heterogeneity of both parents in the child's equations. Our goal in estimating these effects is to exploit the long earnings history from the administrative data for each parent in order to create a measure of unobserved labor market heterogeneity, beyond what could be observed in terms of education and labor force experience of the parent. We treat these as random effects and estimate them using a mixed effects model. While such models are common in the statistics literature, especially biostatistics, they are not as common in the economics literature. Therefore, we begin by briefly presenting the mixed model and then we explain why the mixed effects model does not suffer from some of the same problems that economists typically associate with random effects. We end with a brief description of our estimation method.

8.1 Mixed Model

In their classic text on random and mixed effects models, Searle, Casella, and McCulloch begin by defining factor variables as information that classifies the data into categories. These factor variables have effects on variables of interest to a researcher and these effects can be either fixed or random. The authors define fixed effects as those which are "attributable to a finite set of levels of a factor that occur in the data." Random effects are unobserved factors with an infinite set of levels "of which only a random sample occur in the data." In each case there are multiple observations for each factor. For example, the data may be on housing prices which vary by city, neighborhood and block. Heterogeneity occurs at each level. The heterogeneity within blocks can be treated as random since the quality of homes has infinite support.

Note that the distinction in the statistics literature between random and fixed effects is based on whether the heterogeneity distribution is fully captured by covariates in the data (i.e. fixed effects) or whether the data only provides a sample of the heterogeneity distribution that has infinite support.

In our data, we treat mothers and fathers unobserved personal earnings heterogeneity, θ, as random because there is an infinite number of types of mothers and fathers– the support for the unobserved heterogeneity is infinite. Therefore, the heterogeneity for the group of mothers and fathers present in our data is only a finite sample of all possible values. In contrast, we treat the unobserved heterogeneity associated with different levels of education as fixed since there is a finite and relatively small number of levels of education, each with its own heterogeneity component. If the unobserved heterogeneity distribution is fully captured by the observed education then this form of heterogeneity is fixed. Note that the distinction between the parental heterogeneity, which is random, and the educational heterogeneity, which is fixed, does not require any assumption about the independence of the unobserved heterogeneity.

One particularly appealing characteristic of mixed effects models is that both fixed and random effects can be included. For example when estimating an earnings equation, one can include a set of dummies for a particular characteristic such as education that capture the mean of the heterogeneity distribution across time for individuals. These fixed effects control for time invariant attributes of the individual. A person random effect can also be included that captures the dispersion around this conditional means. This is in contrast with the standard fixed effects models where a person-level effect will soak up the effect of all time-invariant person characteristics.

8.2 Estimation

The models we first estimate are a set of parental earnings models with parental characteristics such as age, labor force experience, race, education, and year time dummies included as explanatory variables. We estimate separate models for mothers and fathers but they are not qualitatively different. To aid the flow of our description, we use mothers as our example in what follows. Everything can be equivalently applied to fathers. First let I be the total number of mothers in the sample with T observations each for a total of $N = I * T$ observations. Let Y_i be a $Tx1$ vector of annual earnings measures for mother i and let X_i be a Txk matrix of explanatory variables with coefficient vector β with dimensions $kx1$. Let d_i be a $1 \times I$ design matrix of the effects associated with mother i and θ be the $Ix1$ matrix of person effects such that $d_i\theta = \theta_i$. Finally let η_i be the $Tx1$ vector of residuals. The linear model for mother i is given by

$$Y_i = X_i\beta + d_i\theta + \eta_i$$

and then stacked across all mothers to become

$$Y = X\beta + Z\theta + \eta \tag{5}$$

$$Z = \begin{bmatrix} d_1 \\ ... \\ d_I \end{bmatrix} = \begin{bmatrix} 1 & 0 & ... & 0 \\ 0 & 1 & ... & 0 \\ ... & ... & ... & ... \\ 0 & 0 & ... & 1 \end{bmatrix}, \theta = \begin{bmatrix} \theta_1 \\ ... \\ \theta_I \end{bmatrix}, Y = \begin{bmatrix} Y_1 \\ ... \\ Y_I \end{bmatrix}, X = \begin{bmatrix} X_1 \\ ... \\ X_I \end{bmatrix}, \eta = \begin{bmatrix} \eta_1 \\ ... \\ \eta_I \end{bmatrix}$$

Statisticians call Z the design matrix of the effects θ. It is merely a set of dummies that assign θ_i from the θ vector to the i^{th} mother.

This model described by 5can be treated as what Greene calls the least squares dummy variable (LSDV) model (page 466) with the following commonly made assumptions:[9]

$$\begin{aligned} \eta &\sim N(0, R) \\ R &= \sigma_\eta^2 I \end{aligned}$$

[9] In all our descriptions here we will assume that the variance structure of the model error, η, is defined as $R = \sigma_\eta^2$ but this assumption can be changed to a more complicated variance structure without substantially changing the model descriptions presented here.

where β and θ are called fixed effects in the statistics literature if the unobservable and observable factors in the population ($\theta_1 ... \theta_l$ and $X_1 ... X_l$) are finite and cover all possible values in the population.

The standard normal equations for the OLS estimator are

$$\begin{bmatrix} Z'Z & Z'X \\ X'Z & X'X \end{bmatrix} \begin{bmatrix} \theta \\ \beta \end{bmatrix} = \begin{bmatrix} Z'Y \\ X'Y \end{bmatrix} \tag{6}$$

which are familiar to most economists. These can be solved to yield

$$\begin{aligned} \beta &= \left[X' \left(I - Z(Z'Z)^{-1}Z'\right) X\right]^{-1} \left[X' \left(I - Z(Z'Z)^{-1}Z'\right) Y\right] \\ \theta &= \left[Z' \left(I - X(X'X)^{-1}X'\right) Z\right]^{-1} \left[Z' \left(I - X(X'X)^{-1}X'\right) Y\right] \end{aligned}$$

using the general rules for obtaining solutions for partitioned regressions (Greene page 179). One characteristics of the LSDV method is that the solutions for (θ, β) do not impose orthogonality between Z and X. In the terms used in the econometrics literature, one does not need to assume that the time invariant unobservables are independent of the $X's$.

The term "random effects" has a different meaning in the econometrics literature where unobserved heterogeneity is treated as a random effect in the following sense:

$$\begin{aligned} Y &= X\beta + \theta + \eta \\ \eta &\sim N(0, \sigma_\eta^2 I) \\ \theta &\sim N(0, \sigma_\theta^2 I) \\ cov(\theta, \eta) &= 0, cov(X, \eta) = 0, cov(X, \theta) = 0 \\ \Omega &= var(y_i) = \begin{bmatrix} \sigma_\eta^2 + \sigma_\theta^2 & \sigma_\theta^2 & ... & \sigma_\theta^2 \\ \sigma_\theta^2 & \sigma_\eta^2 + \sigma_\theta^2 & ... & \sigma_\theta^2 \\ ... & ... & ... & ... \\ \sigma_\theta^2 & \sigma_\theta^2 & ... & \sigma_\eta^2 + \sigma_\theta^2 \end{bmatrix} \\ R &= \begin{bmatrix} \Omega & 0 & ... & 0 \\ 0 & \Omega & ... & 0 \\ ... & ... & ... & ... \\ 0 & 0 & ... & \Omega \end{bmatrix} \end{aligned}$$

In this model, the random effect is merely treated as a portion of the error term. The identity of the mother imposes additional structure on the variance/covariance matrix of the error term. This type of model does not estimate θ directly but rather estimates σ_θ^2. The solution for fixed effects, β, is

$$\beta = (X'R^{-1}X)^{-1}X'R^{-1}Y$$

which is the standard GLS estimator. There is no $X'Z$ term in this model because of the assumption of orthogonality between the random effects and the observed characteristics in the X vector.[10]

[10] The widely-used Hausman test is in fact a test of whether $X'Z = 0$ and the frequent rejection of this hypothesis has left most economists skeptical of using random effects.

In contrast to these two methods, mixed effects models allow θ to be treated as a random effect but also allow $\widehat{\theta_i}$ to be estimated for each mother in the sample. These methods were pioneered by Henderson, a biostatistician interested in estimating genetic models that predicted milk production of cows as a function of the identity of their sires and dames. The goal of his models was to be able to predict parent effects for the milk production of a child cow, with the intent of identifying which bulls sired the best milk-producing daughters. He began with the same model as above, namely,

$$Y = X\beta + Z\theta + \eta$$

along with the assumptions (Searle, Casella, McCulloch page 275)

$$\begin{bmatrix} \theta \\ Y \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ X\beta \end{bmatrix}, \begin{bmatrix} G & GZ' \\ ZG & V \end{bmatrix} \right)$$

$$\begin{aligned} var(Y) &= V = ZGZ' + R \\ R &= \sigma^2_\eta I \\ var(\theta) &= G = \sigma^2_\theta I \\ cov(Y, \theta) &= ZG \end{aligned}$$

Henderson shows that the pdf of the joint distribution is given by

$$\begin{aligned} f(y, \theta) &= f(y \mid \theta) f(\theta) \qquad (7) \\ &= \frac{\exp\left\{-\frac{1}{2}\left[(y - X\beta - Z\theta)' R^{-1} (y - X\beta - Z\theta) + \theta' G^{-1} \theta\right]\right\}}{(2\pi)^{1/2(N+I)} |R|^{1/2} |G|^{1/2}} \end{aligned}$$

By taking partial derivatives of 7 with respect to β and θ, Henderson arrived at what are now known as the mixed model equations (MME) (Searle, Casella, McCulloch page 276).

$$\begin{bmatrix} X'R^{-1}X & X'R^{-1}Z \\ Z'R^{-1}X & Z'R^{-1}Z + G^{-1} \end{bmatrix} \begin{bmatrix} \widehat{\beta} \\ \widehat{\theta} \end{bmatrix} = \begin{bmatrix} X'R^{-1}Y \\ Z'R^{-1}Y \end{bmatrix} \qquad (8)$$

The important thing to notice in these equations is that $X'R^{-1}Z \neq 0$, and hence the standard economist concern about imposing orthogonality between the characteristics in X and the design of the random effects matrix is no longer an issue.

It is also informative to compare equation 8 to equation 6, the normal equations for the LSDV model. Without G^{-1} in the bottom right cell, the MME are simply the maximum likelihood versions of the normal equations for the LSDV model. As $|G| \to \infty$, the MME converge to the normal equations. Thus the LSDV model is a special case of the mixed effect model.

In estimating our mixed effect model we use Restricted Maximum Likelihood (REML). The basic concept of REML estimation is to maximize a marginal likelihood. A set of linear error contrast equations are created that do not include β and these are used to create a likelihood function that contains only σ_η^2 and σ_θ^2 from the variance matrices G and R (Searle, Casella, and McCulloch (1992)). These parameters are called variance components and are estimated by maximizing this marginal likelihood. Using these estimates of G and R, the mixed model equations are solved to give estimates for the fixed effects, $\widehat{\beta}$, and then the predicted random effects, $\widehat{\theta}$. For samples of our size and earnings equations with simple random effects, the Stata version of REML for mixed effects models (xtmixed) is sufficient to generate $\widehat{\theta_i}$ for each parent in our sample in a computationally feasible amount of time.

Table 1A: Summary Statistics for Sons

	Overall		Mother - no work	Mother - some work		Mother - all work	
	mean	**st. dev.**	**mean**	**diff**	**st. err.**	**diff**	**st. err.**
N	**6986**		**1885**	**2869**	**for diff**	**2232**	**for diff**
black	0.08	0.27	0.05	0.03***	(0.01)	0.07***	(-6.10)
oldest child	0.38	0.48	0.30	0.08***	(0.01)	0.15***	(-7.82)
youngest child	0.29	0.45	0.27	0.02	(0.01)	0.03*	(-1.39)
age of mother at birth	26.93	5.08	27.30	-0.82***	(0.15)	-0.08	(-3.30)
age of father at birth	29.44	5.80	30.13	-1.16***	(0.18)	-0.68***	(-0.13)
younger sibling before age 5	0.51	0.50	0.55	-0.05***	(0.01)	-0.08***	(4.11)
Mother Educ. Indicators							
high school or less	0.51	0.50	0.61	-0.08***	(0.01)	-0.21***	(13.22)
some college	0.28	0.45	0.23	0.04***	(0.01)	0.10***	(-5.94)
college or more	0.21	0.41	0.16	0.03**	(0.01)	0.12***	(-8.83)
Father Educ. Indicators							
high school or less	0.45	0.50	0.48	-0.00	(0.01)	-0.08***	(6.04)
some college	0.27	0.44	0.24	0.03*	(0.01)	0.05***	(-3.09)
college or more	0.28	0.45	0.28	-0.02	(0.01)	0.03	(-3.48)
Mother no random effect	0.06	0.24	0.14	-0.10***	(0.01)	-0.13***	(13.44)
Mother random effect	-0.01	0.77	0.06	-0.14***	(0.03)	-0.02	(-3.42)
Father no random effect	0.01	0.08	0.01	-0.01***	(0.00)	-0.01***	(4.24)
Father random effect	-0.00	0.61	0.06	-0.05**	(0.02)	-0.12***	(5.96)
Avg. family earnings							
Son ages 1-5	61,307	72,204	54,322	1075.37	(1985.99)	19764.09***	(-10.73)
Son ages 6-10	74,165	113,522	63,131	10021.01**	(3360.42)	21179.62***	(-6.69)
Son ages 11-13	82,858	114,293	71,851	10050.20**	(3241.47)	21015.04***	(-5.41)
Son ages 14-18	90,520	113,584	80,797	8905.27*	(3804.89)	18643.20***	(-5.27)
Age 30+ positive earnings	0.87	0.34	0.87	0.00	(0.01)	-0.00	(0.42)
Age 30 earnings ($2012)	45,463	50,319	44,675	-8.49	(1483.44)	2462.44	(-1.67)
Age 30 yrs labor force exp.	12.70	3.16	12.45	0.20*	(0.10)	0.51***	(-4.94)
Mom work year before birth	0.61	0.49	0.29	0.32***	(0.01)	0.59***	(-39.61)
Mom work age 0	0.54	0.50	0.16	0.35***	(0.01)	0.73***	(-53.22)
Mom work age 1	0.49	0.50					
Mom work age 2	0.51	0.50					
yrs mom work age 3-5	1.63	1.32					
yrs mom work age 6-10	3.27	1.96	1.59	1.73***	(0.05)	3.02***	(-55.42)
yrs mom work age 11-13	2.19	1.20	1.55	0.65***	(0.04)	1.16***	(-31.43)
yrs mom work age 14-18	3.87	1.81	3.05	0.85***	(0.06)	1.48***	(-25.27)
yrs dad work age 1-5	4.48	1.27	4.04	0.55***	(0.05)	0.69***	(-13.08)

Sample is boys who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993, 1996); Means for overall sample; Means for sons with NO WORK mothers; Diff=Mean(SOME WORK mothers) - Mean(NO WORK mothers); Diff=Mean(ALL WORK mothers) - Mean(NO WORK mothers); Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 1B: Summary Statistics for Daughters

	Overall		Mother - no work	Mother - some work		Mother - all work	
	mean	**st. dev.**	**mean**	**diff**	**st. err. for diff**	**diff**	**st. err. for diff**
N	**6662**		**1844**	**2695**		**2123**	
black	0.08	0.28	0.05	0.03***	(0.01)	0.08***	(0.01)
oldest child	0.38	0.49	0.32	0.06***	(0.01)	0.11***	(0.02)
youngest child	0.28	0.45	0.26	0.03	(0.01)	0.03*	(0.01)
age of mother at birth	26.82	5.05	27.39	-1.17***	(0.16)	-0.30	(0.16)
age of father at birth	29.35	5.89	30.30	-1.65***	(0.18)	-0.89***	(0.19)
younger sibling before age 5	0.50	0.50	0.57	-0.06***	(0.02)	-0.11***	(0.02)
Mother Educ. Indicators							
high school or less	0.52	0.50	0.63	-0.11***	(0.01)	-0.22***	(0.02)
some college	0.28	0.45	0.23	0.05***	(0.01)	0.08***	(0.01)
college or more	0.21	0.41	0.14	0.06***	(0.01)	0.14***	(0.01)
Father Educ. Indicators							
high school or less	0.46	0.50	0.50	-0.04*	(0.02)	-0.08***	(0.02)
some college	0.27	0.44	0.23	0.05***	(0.01)	0.06***	(0.01)
college or more	0.27	0.44	0.27	-0.02	(0.01)	0.02	(0.01)
Mother no random effect	0.07	0.25	0.15	-0.10***	(0.01)	-0.14***	(0.01)
Mother random effect	-0.00	0.76	0.08	-0.16***	(0.03)	-0.05	(0.03)
Father no random effect	0.01	0.08	0.01	-0.01**	(0.00)	-0.01***	(0.00)
Father random effect	0.01	0.61	0.08	-0.08***	(0.02)	-0.13***	(0.02)
Avg. family earnings							
Daughter ages 1-5	60,554	48,250	56,596	-4311.76**	(1522.92)	17445.91***	(1769.63)
Daughter ages 6-10	72,981	67,090	65,052	2824.75	(2045.38)	20897.96***	(2395.07)
Daughter ages 11-13	81,304	84,414	73,223	4095.46	(2461.79)	19795.28***	(2777.14)
Daughter ages 14-18	88,582	93,190	81,647	2459.78	(3015.57)	18424.50***	(3179.48)
Age 30+ positive earnings	0.84	0.37	0.82	0.02	(0.01)	0.04**	(0.01)
Age 30 earnings ($2012)	34,899	32,090	35,428	-2299.87*	(1104.93)	1214.34	(1218.26)
Age 30 yrs labor force exp.	12.58	3.10	12.08	0.53***	(0.10)	0.89***	(0.10)
Mom work year before birth	0.61	0.49	0.29	0.31***	(0.01)	0.62***	(0.01)
Mom work age 0	0.53	0.50	0.17	0.32***	(0.01)	0.74***	(0.01)
Mom work age 1	0.49	0.50	0.00	0.42***	(0.01)	1.00	(0.00)
Mom work age 2	0.51	0.50	0.00	0.47***	(0.01)	1.00	(0.00)
yrs mom work age 3-5	1.62	1.32	0.00	1.63***	(0.02)	3.00	(0.00)
yrs mom work age 6-10	3.23	1.98	1.46	1.88***	(0.05)	3.17***	(0.04)
yrs mom work age 11-13	2.17	1.21	1.44	0.76***	(0.04)	1.30***	(0.04)
yrs mom work age 14-18	3.83	1.82	2.92	0.97***	(0.06)	1.60***	(0.06)
yrs dad work age 1-5	4.48	1.28	4.05	0.54***	(0.05)	0.67***	(0.05)

Sample is girls who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993, 1996); Means for overall sample; Means for daughters with NO WORK mothers; Diff=Mean(SOME WORK mothers) - Mean(NO WORK mothers); Diff=Mean(ALL WORK mothers) - Mean(NO WORK mothers); Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 2: Annual W-2 Earnings of Sons at age 30 and older

Parental Work Variables		(1) Simple	(2) Full Controls
mom work age 0		0.028	0.016
		(0.037)	(0.036)
mom work age 1		-0.034	-0.022
		(0.042)	(0.041)
mom work age 2		-0.019	-0.019
		(0.041)	(0.040)
years mom work age 3-5		0.009	0.015
		(0.017)	(0.016)
mom work age 6		0.080*	0.063
		(0.040)	(0.039)
years mom work age 7 - 10		-0.026	-0.020
		(0.014)	(0.014)
years mom work age 11-12		0.010	0.011
		(0.028)	(0.028)
mom work age 13		-0.067	-0.051
		(0.051)	(0.050)
mom work age 14-18		0.027*	0.016
		(0.012)	(0.013)
years dad work age 1-5		0.047***	0.019
		(0.012)	(0.021)
Parental Controls			
Mother	HS degree		0.110*
			(0.052)
Mother	Some College		0.049
			(0.058)
Mother	College degree		0.115
			(0.068)
Mother	Graduate degree		0.205*
			(0.081)
Father	HS degree		0.126*
			(0.052)
Father	Some College		0.255***
			(0.055)
Father	College degree		0.284***
			(0.065)
Father	Graduate degree		0.443***
			(0.074)
Mother	no RE		0.168*
			(0.071)
Mother	Random Effect		0.058**
			(0.020)
Father	no RE		0.026
			(0.194)
Father	Random Effect		0.178***
			(0.031)
N sons-years		16731	16731
N sons		6080	6080

Sample is boys who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993,1996); 3. had positive earnings at age 30 or older; Other regression controls not reported in both columns: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year); in column 2: log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 3: IV Estimate: Annual W-2 Earnings of Sons at age 30 and older

Parental Work Variables		(1) Full Controls	F-stat
mom work age 0		0.545	5.55
		(0.394)	
mom work age 1 - 5		-0.061	7.39
		(0.104)	
years mom work age 6 - 10		0.047	10.00
		(0.124)	
years mom work age 11-13		-0.229	6.86
		(0.333)	
mom work age 14-18		-0.022	4.27
		(0.204)	
years dad work age 1-5		0.017	
		(0.038)	
Parental Controls			
Mother	HS degree	0.163*	
		(0.077)	
Mother	Some College	0.125	
		(0.088)	
Mother	College degree	0.190	
		(0.107)	
Mother	Graduate degree	0.303*	
		(0.130)	
Father	HS degree	0.101	
		(0.063)	
Father	Some College	0.228**	
		(0.073)	
Father	College degree	0.243*	
		(0.097)	
Father	Graduate degree	0.342**	
		(0.128)	
Mother	Random Effect	0.112*	
		(0.048)	
Father	Random Effect	0.125	
		(0.092)	
N sons-years		16731	
N sons		6080	

Sample is boys who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993,1996); 3. had positive earnings at age 30 or older; Other regression controls not reported in both columns: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Excluded instruments are: presence of sibling under age 5 at child age 1, 5, 10, 14; female labor force participation rate in state of employment at child age 1, 5, 10, 14; number of child care establishments per capita in state of employment at child age 1, 2, 3; average total payroll/employment at child care estabs. in state of employment at child age 1, 2, 3. F-stat reports significance of excluded instruments in each first-stage regression; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 4: Annual W-2 Earnings of Sons at age 30 and older, By Mother Education Category

Parental Work Variables		(1) College or more	(2) Some College	(3) High School or less
mom work age 0		-0.060	0.058	0.015
		(0.085)	(0.066)	(0.049)
mom work age 1		0.052	-0.030	-0.040
		(0.101)	(0.074)	(0.057)
mom work age 2		-0.021	-0.001	-0.034
		(0.099)	(0.077)	(0.054)
years mom work age 3-5		0.021	0.012	0.012
		(0.038)	(0.031)	(0.022)
mom work age 6		0.079	0.027	0.078
		(0.099)	(0.077)	(0.051)
years mom work age 7 - 10		-0.014	-0.038	-0.012
		(0.037)	(0.027)	(0.019)
years mom work age 11-12		0.031	0.031	0.006
		(0.077)	(0.051)	(0.037)
mom work age 13		-0.222	0.061	-0.051
		(0.140)	(0.102)	(0.065)
mom work age 14-18		0.043	-0.012	0.021
		(0.031)	(0.024)	(0.017)
years dad work age 1-5		0.049	-0.014	0.008
		(0.049)	(0.044)	(0.029)
Parental Controls				
Mother	HS degree	-	-	0.141**
				(0.055)
Mother	Graduate degree	0.064	-	-
		(0.065)		
Father	HS degree	-0.563*	0.408*	0.104
		(0.221)	(0.161)	(0.056)
Father	Some College	-0.427*	0.494**	0.249***
		(0.214)	(0.158)	(0.063)
Father	College degree	-0.482*	0.660***	0.190*
		(0.214)	(0.170)	(0.092)
Father	Graduate degree	-0.264	0.692***	0.386**
		(0.220)	(0.182)	(0.133)
Mother	no RE	0.158	-0.003	0.219*
		(0.203)	(0.170)	(0.087)
Mother	Random Effect	0.049	0.031	0.067*
		(0.045)	(0.037)	(0.028)
Father	no RE	-0.091	-0.209	0.121
		(0.862)	(0.237)	(0.241)
Father	Random Effect	0.126	0.213***	0.191***
		(0.071)	(0.059)	(0.043)
N sons-years		3499	4691	8541
N sons		1289	1733	3058

Sample is boys who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993,1996); 3. had positive earnings at age 30 or older; Other regression controls not reported in both columns: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 5: Annual W-2 Earnings of Sons at age 30 and older, Restricted Samples

Parental Work Variables		Sample 1	Sample 2	Sample 3
mom work all years age 1-5				-0.070
				(0.080)
mom work age 0		0.010	0.078	
		(0.051)	(0.083)	
mom work age 1		0.026	0.045	
		(0.049)	(0.068)	
mom work age 2		-0.038	-0.038	
		(0.053)	(0.072)	
years mom work age 3-5		-0.003	-0.022	
		(0.022)	(0.031)	
mom work age 6		0.026	0.050	0.019
		(0.055)	(0.078)	(0.104)
years mom work age 7 - 10		-0.006	0.013	0.011
		(0.020)	(0.028)	(0.036)
years mom work age 11-12		0.075	0.007	-0.053
		(0.041)	(0.059)	(0.088)
mom work age 13		-0.108	-0.075	-0.042
		(0.071)	(0.102)	(0.155)
mom work age 14-18		0.023	0.011	0.012
		(0.018)	(0.027)	(0.032)
Parental Controls				
Mother	HS degree	0.091	0.198	0.169
		(0.097)	(0.191)	(0.214)
Mother	Some College	0.030	0.214	0.275
		(0.103)	(0.193)	(0.223)
Mother	College degree	0.127	0.316	0.396
		(0.111)	(0.201)	(0.238)
Mother	Graduate degree	0.185	0.400	0.371
		(0.125)	(0.211)	(0.248)
Father	HS degree	0.187*	0.247	0.142
		(0.088)	(0.144)	(0.165)
Father	Some College	0.294**	0.323*	0.041
		(0.092)	(0.152)	(0.178)
Father	College degree	0.434***	0.471**	0.248
		(0.102)	(0.165)	(0.191)
Father	Graduate degree	0.581***	0.673***	0.482*
		(0.118)	(0.188)	(0.212)
Mother	no RE	0.331**	0.327*	0.262
		(0.104)	(0.146)	(0.189)
Mother	Random Effect	0.049	0.094*	0.139**
		(0.027)	(0.039)	(0.049)
Father	Random Effect	0.228***	0.214**	0.141
		(0.050)	(0.080)	(0.099)
N sons-years		8897	4375	2347
N sons		3252	1589	855

Column 1: Original sample restricted to mother worked year before birth, father worked every year child age 1-5; Column 2: column 1 sample restricted to oldest children; Column 3: original sample restricted to mother worked at child age 1, father worked every year child age 1-5, oldest child, younger sibling born before child is 5; Other regression controls not reported: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 6: Annual W-2 Earnings of Sons at age 30, Mother Fixed Effects

Parental Work Variables	
mom work age 0	-0.262
	(0.135)
mom work age 1	0.015
	(0.134)
mom work age 2	-0.183
	(0.141)
years mom work age 3-5	-0.109
	(0.089)
mom work age 6	-0.117
	(0.141)
years mom work age 7 - 10	-0.050
	(0.082)
years mom work age 11-12	-0.202
	(0.108)
mom work age 13	-0.187
	(0.175)
mom work age 14-18	0.004
	(0.086)
years dad work age 1-5	0.237
	(0.151)
N sons	1170
N mothers	571

Original sample restricted to sons with brothers also born between 1978 and 1982; mother fixed effects included; Other regression controls not reported: oldest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 7: Annual W-2 Earnings of Sons at age 30 and older, Mother Firm Characteristic Variables

Parental Work Variables	Full Controls
years mom work when child is age 1-5 at jobs in:	
manufacturing	0.024
	(0.031)
service 1	0.036
	(0.032)
education	0.040
	(0.035)
health	0.027
	(0.031)
service 2	0.045
	(0.031)
government	0.009
	(0.019)
self-employment	0.021
	(0.024)
missing	-0.006
	(0.039)
firm size 1	-0.003
	(0.033)
firm size 2	-0.026
	(0.036)
firm size 3	-0.038
	(0.037)
firm size 4	-0.018
	(0.036)
firm size 5	-0.041
	(0.039)
firm size 6	-0.021
	(0.034)
firm size 7	-0.051
	(0.044)
firm size 8	-0.041
	(0.028)
mom work age 6	0.045
	(0.041)
years mom work age 7 - 10	-0.014
	(0.015)
years mom work age 11-12	0.015
	(0.030)
mom work age 13	-0.082
	(0.053)
mom work age 14-18	0.021
	(0.014)
years dad work age 1-5	0.003
	(0.024)
N sons-years	15373
N sons	5511

Original sample restricted to sons with mothers with valid SSNs. Other regression controls not reported: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 8: Annual Labor Force Participation of Sons at age 18 and older

Parental Work Variables		(1) Simple	(2) Full Controls
mom work age 0		0.030	-0.005
		(0.058)	(0.058)
mom work age 1		-0.024	-0.016
		(0.071)	(0.069)
mom work age 2		0.066	0.040
		(0.067)	(0.066)
years mom work age 3-5		-0.015	-0.012
		(0.027)	(0.027)
mom work age 6		0.037	0.020
		(0.065)	(0.064)
years mom work age 7 - 10		-0.016	-0.019
		(0.021)	(0.021)
years mom work age 11-12		0.067	0.042
		(0.043)	(0.044)
mom work age 13		-0.098	-0.060
		(0.083)	(0.083)
mom work age 14-18		0.096***	0.059**
		(0.017)	(0.019)
years dad work age 1-5		0.134***	0.050
		(0.016)	(0.031)
Parental Controls			
Mother	HS degree		0.157*
			(0.077)
Mother	Some College		0.139
			(0.087)
Mother	College degree		0.114
			(0.104)
Mother	Graduate degree		0.085
			(0.129)
Father	HS degree		0.122
			(0.075)
Father	Some College		0.141
			(0.085)
Father	College degree		0.004
			(0.098)
Father	Graduate degree		0.135
			(0.115)
Mother	no RE		-0.052
			(0.101)
Mother	Random Effect		-0.019
			(0.030)
Father	no RE		-0.146
			(0.251)
Father	Random Effect		0.040
			(0.047)
N sons-years		104715	104715
N sons		6986	6986

Sample is boys who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993,1996); Other regression controls not reported in both columns: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (1996-2011; 2012 excluded year); in column 2: log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 9: Annual Labor Force Participation of Sons at ages 18 and older, By Mother Education Category

Parental Work Variables		(1) College or more	(2) Some College	(3) High School or less
mom work age 0		-0.026	0.097	-0.064
		(0.133)	(0.100)	(0.083)
mom work age 1		-0.033	-0.067	0.017
		(0.155)	(0.122)	(0.100)
mom work age 2		-0.050	0.176	-0.002
		(0.167)	(0.117)	(0.092)
years mom work age 3-5		-0.035	-0.001	-0.008
		(0.071)	(0.050)	(0.036)
mom work age 6		0.105	-0.081	0.031
		(0.187)	(0.129)	(0.080)
years mom work age 7 - 10		0.038	-0.043	-0.024
		(0.056)	(0.044)	(0.027)
years mom work age 11-12		0.054	0.096	0.028
		(0.101)	(0.080)	(0.062)
mom work age 13		-0.104	0.259	-0.170
		(0.213)	(0.161)	(0.112)
mom work age 14-18		0.053	-0.018	0.084***
		(0.048)	(0.040)	(0.025)
years dad work age 1-5		0.040	-0.044	0.066
		(0.080)	(0.066)	(0.041)
Parental Controls				
Mother	HS degree	-	-	0.157
		-	-	(0.082)
Mother	Graduate degree	-0.038	-	-
		(0.108)	-	-
Father	HS degree	-1.431**	-0.003	0.150
		(0.483)	(0.196)	(0.084)
Father	Some College	-1.333**	-0.020	0.195
		(0.470)	(0.192)	(0.106)
Father	College degree	-1.523**	-0.016	-0.035
		(0.469)	(0.208)	(0.149)
Father	Graduate degree	-1.222*	-0.253	0.120
		(0.476)	(0.233)	(0.232)
Mother	no RE	-0.027	-0.389	0.005
		(0.237)	(0.228)	(0.127)
Mother	Random Effect	-0.066	-0.116*	0.035
		(0.061)	(0.056)	(0.042)
Father	no RE	1.094	-0.792	-0.114
		(0.739)	(0.421)	(0.300)
Father	Random Effect	-0.039	0.062	0.042
		(0.110)	(0.087)	(0.067)
N sons-years		21619	29337	53759
N sons		1453	1964	3569

Sample is boys who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993,1996); Other regression controls not reported: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (1996-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 10: Annual W-2 Earnings of Daughters at age 30 and older

Parental Work Variables		(1) Simple	(2) Full Controls
mom work age 0		0.058	0.042
		(0.041)	(0.041)
mom work age 1		-0.066	-0.047
		(0.050)	(0.049)
mom work age 2		0.047	0.041
		(0.049)	(0.048)
years mom work age 3-5		-0.031	-0.037
		(0.020)	(0.020)
mom work age 6		0.016	0.018
		(0.049)	(0.048)
years mom work age 7 - 10		-0.010	-0.005
		(0.016)	(0.016)
years mom work age 11-12		0.024	0.032
		(0.034)	(0.034)
mom work age 13		0.004	0.026
		(0.062)	(0.061)
mom work age 14-18		0.040**	0.010
		(0.013)	(0.014)
years dad work age 1-5		0.009	-0.025
		(0.013)	(0.025)
Parental Controls			
Mother	HS degree		0.044
			(0.053)
Mother	Some College		0.185**
			(0.058)
Mother	College degree		0.126
			(0.070)
Mother	Graduate degree		0.240**
			(0.087)
Father	HS degree		0.115*
			(0.054)
Father	Some College		0.140*
			(0.059)
Father	College degree		0.311***
			(0.067)
Father	Graduate degree		0.323***
			(0.080)
Mother	no RE		0.015
			(0.073)
Mother	Random Effect		0.095***
			(0.022)
Father	no RE		0.010
			(0.282)
Father	Random Effect		0.116***
			(0.035)
N daughters-years		15076	15076
N daughters		5570	5570

Sample is girls who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993,1996); 3. had positive earnings at age 30 or older; Other regression controls not reported in both columns: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year); in column 2: log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 11: IV Estimate: Annual W-2 Earnings of Daughters at age 30 and older

Parental Work Variables		(1) Full Controls	F-stat
mom work age 0		-0.331	4.38
		(0.516)	
mom work age 1 - 5		-0.132	5.21
		(0.148)	
years mom work age 6 - 10		-0.060	7.23
		(0.173)	
years mom work age 11-13		0.514	6.98
		(0.501)	
mom work age 14-18		-0.191	4.16
		(0.315)	
years dad work age 1-5		-0.105	
		(0.055)	
Parental Controls			
Mother	HS degree	0.130	
		(0.111)	
Mother	Some College	0.320*	
		(0.135)	
Mother	College degree	0.301	
		(0.189)	
Mother	Graduate degree	0.446	
		(0.229)	
Father	HS degree	0.025	
		(0.090)	
Father	Some College	0.019	
		(0.112)	
Father	College degree	0.131	
		(0.168)	
Father	Graduate degree	0.085	
		(0.236)	
Mother	Random Effect	0.093	
		(0.069)	
Father	Random Effect	0.002	
		(0.137)	
N daughter-years		15076	
N daughters		5570	

Sample is girls who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993,1996); 3. had positive earnings at age 30 or older; Other regression controls not reported in both columns: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Excluded instruments are: presence of sibling under age 5 at child age 1, 5, 10, 14; female labor force participation rate in state of employment at child age 1, 5, 10, 14; number of child care establishments per capita in state of employment at child age 1, 2, 3; average total payroll/employment at child care estabs. in state of employment at child age 1, 2, 3. F-stat reports significance of excluded instruments in each first-stage regression; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 12: Annual W-2 Earnings of Daughters at age 30 and older, By Mother Education Category

Parental Work Variables		(1) College or more	(2) Some College	(3) High School or less
mom work age 0		-0.030	-0.023	0.094
		(0.092)	(0.082)	(0.055)
mom work age 1		0.121	-0.142	-0.073
		(0.106)	(0.090)	(0.069)
mom work age 2		-0.124	0.125	0.079
		(0.106)	(0.087)	(0.070)
years mom work age 3-5		-0.053	0.022	-0.071**
		(0.043)	(0.040)	(0.026)
mom work age 6		0.137	-0.049	0.033
		(0.118)	(0.100)	(0.059)
years mom work age 7 - 10		0.008	-0.020	0.002
		(0.039)	(0.031)	(0.022)
years mom work age 11-12		-0.040	-0.044	0.096*
		(0.084)	(0.070)	(0.042)
mom work age 13		0.261	0.034	-0.060
		(0.168)	(0.123)	(0.074)
mom work age 14-18		-0.018	0.036	0.006
		(0.038)	(0.029)	(0.018)
years dad work age 1-5		-0.101*	0.063	-0.028
		(0.043)	(0.051)	(0.034)
Parental Controls				
Mother	HS degree	-	-	0.027
				(0.055)
Mother	Graduate degree	0.125	-	-
		(0.077)		
Father	HS degree	-0.194	0.141	0.138*
		(0.203)	(0.137)	(0.061)
Father	Some College	-0.104	0.113	0.162*
		(0.178)	(0.139)	(0.071)
Father	College degree	0.057	0.168	0.452***
		(0.178)	(0.147)	(0.098)
Father	Graduate degree	0.069	0.304	0.278
		(0.186)	(0.168)	(0.157)
Mother	no RE	-0.231	-0.015	0.063
		(0.211)	(0.146)	(0.091)
Mother	Random Effect	0.042	0.104*	0.105***
		(0.047)	(0.044)	(0.031)
Father	no RE	-0.233	-1.568	0.173
		(0.405)	(0.924)	(0.219)
Father	Random Effect	0.058	0.105	0.155**
		(0.073)	(0.061)	(0.054)
N daughters-years		3265	4142	7669
N daughters		1195	1534	2841

Sample is girls who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993,1996); 3. had positive earnings at age 30 or older; Other regression controls not reported in both columns: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 13: Annual W-2 Earnings of Daughters at age 30 and older, Restricted Samples

Parental Work Variables		Sample 1	Sample 2	Sample 3
mom work all years age 1-5				0.140
				(0.101)
mom work age 0		0.087	0.073	
		(0.057)	(0.085)	
mom work age 1		0.032	0.016	
		(0.065)	(0.089)	
mom work age 2		0.007	0.119	
		(0.063)	(0.086)	
years mom work age 3-5		-0.036	-0.029	
		(0.028)	(0.040)	
mom work age 6		-0.004	-0.031	-0.030
		(0.072)	(0.105)	(0.128)
years mom work age 7 - 10		-0.010	-0.017	-0.056
		(0.024)	(0.032)	(0.043)
years mom work age 11-12		0.002	-0.004	0.122
		(0.055)	(0.075)	(0.086)
mom work age 13		0.022	0.113	0.276
		(0.091)	(0.125)	(0.178)
mom work age 14-18		0.025	-0.003	-0.026
		(0.022)	(0.030)	(0.045)
Parental Controls				
Mother	HS degree	0.069	0.119	-0.137
		(0.086)	(0.152)	(0.171)
Mother	Some College	0.220*	0.243	0.019
		(0.091)	(0.157)	(0.174)
Mother	College degree	0.186	0.207	0.016
		(0.108)	(0.173)	(0.203)
Mother	Graduate degree	0.343**	0.277	-0.204
		(0.127)	(0.202)	(0.251)
Father	HS degree	0.145	0.332*	0.446*
		(0.082)	(0.146)	(0.203)
Father	Some College	0.187*	0.429**	0.598**
		(0.085)	(0.149)	(0.210)
Father	College degree	0.295**	0.503**	0.736**
		(0.099)	(0.165)	(0.234)
Father	Graduate degree	0.241*	0.553**	0.744**
		(0.117)	(0.180)	(0.256)
Mother	no RE	0.036	-0.100	0.047
		(0.134)	(0.187)	(0.260)
Mother	Random Effect	0.071*	0.112*	0.075
		(0.033)	(0.049)	(0.066)
Father	Random Effect	0.048	0.066	0.263*
		(0.056)	(0.085)	(0.104)
N daughters-years		7903	3844	2014
N daughters		2975	1423	738

Column 1: Original sample restricted to mother worked year before birth, father worked every year child age 1-5; Column 2: column 1 sample restricted to oldest children; Column 3: original sample restricted to mother worked at child age 1, father worked every year child age 1-5, oldest child, younger sibling born before child is 5; Other regression controls not reported: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 14: Annual W-2 Earnings of Daughters at age 30, Mother Fixed Effects

Parental Work Variables	
mom work age 0	0.056
	(0.152)
mom work age 1	-0.147
	(0.165)
mom work age 2	0.099
	(0.186)
years mom work age 3-5	0.032
	(0.104)
mom work age 6	-0.002
	(0.160)
years mom work age 7 - 10	0.150
	(0.099)
years mom work age 11-12	0.102
	(0.128)
mom work age 13	0.089
	(0.217)
mom work age 14-18	0.073
	(0.093)
years dad work age 1-5	-0.024
	(0.213)
N daughters	945
N mothers	459

Original sample restricted to daughters with brothers also born between 1978 and 1982; mother fixed effects included; Other regression controls not reported: oldest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 15: Annual W-2 Earnings of Daughters at age 30 and older, Mother Firm Characteristic Variables

Parental Work Variables	Full Controls
years mom work when child is age 1-5 at jobs in:	
manufacturing	0.009
	(0.035)
service 1	0.036
	(0.032)
education	0.006
	(0.040)
health	0.015
	(0.036)
service 2	-0.005
	(0.035)
government	0.003
	(0.021)
self-employment	0.018
	(0.028)
missing	0.016
	(0.042)
firm size 1	-0.050
	(0.039)
firm size 2	-0.032
	(0.041)
firm size 3	-0.014
	(0.042)
firm size 4	-0.034
	(0.043)
firm size 5	-0.057
	(0.046)
firm size 6	-0.018
	(0.038)
firm size 7	-0.071
	(0.044)
firm size 8	-0.023
	(0.032)
mom work age 6	0.009
	(0.050)
years mom work age 7 - 10	-0.004
	(0.017)
years mom work age 11-12	0.040
	(0.036)
mom work age 13	0.005
	(0.064)
mom work age 14-18	0.015
	(0.015)
years dad work age 1-5	-0.028
	(0.027)
N daughters-years	13809
N daughters	5028

Original sample restricted to daughters with mothers with valid SSNs. Other regression controls not reported: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (2008-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 16: Annual Labor Force Participation of Daughters at age 18 and older

Parental Work Variables		(1) Simple	(2) Full Controls
mom work age 0		0.137*	0.106*
		(0.054)	(0.053)
mom work age 1		-0.125	-0.123
		(0.066)	(0.065)
mom work age 2		0.120	0.108
		(0.071)	(0.071)
years mom work age 3-5		-0.024	-0.028
		(0.027)	(0.027)
mom work age 6		0.090	0.108
		(0.064)	(0.063)
years mom work age 7 - 10		0.009	0.002
		(0.022)	(0.022)
years mom work age 11-12		0.037	0.019
		(0.043)	(0.042)
mom work age 13		-0.043	-0.043
		(0.079)	(0.076)
mom work age 14-18		0.093***	0.061***
		(0.016)	(0.018)
years dad work age 1-5		0.085***	0.034
		(0.015)	(0.030)
Parental Controls			
Mother	HS degree		0.095
			(0.074)
Mother	Some College		0.154
			(0.085)
Mother	College degree		0.053
			(0.099)
Mother	Graduate degree		0.054
			(0.131)
Father	HS degree		0.143*
			(0.072)
Father	Some College		0.107
			(0.080)
Father	College degree		0.246**
			(0.095)
Father	Graduate degree		0.170
			(0.112)
Mother	no RE		-0.118
			(0.094)
Mother	Random Effect		0.018
			(0.028)
Father	no RE		0.386
			(0.279)
Father	Random Effect		-0.022
			(0.046)
N daughters-years		99909	99909
N daughters		6662	6662

Sample is girls who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993,1996); Other regression controls not reported in both columns: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (1996-2011; 2012 excluded year); in column 2: log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 17: Annual Labor Force Participation of Daughters at ages 18 and older, By Mother Education Category

Parental Work Variables		(1) College or more	(2) Some College	(3) High School or less
mom work age 0		0.004	0.156	0.086
		(0.113)	(0.114)	(0.071)
mom work age 1		-0.002	-0.284*	-0.081
		(0.133)	(0.134)	(0.090)
mom work age 2		0.079	0.300*	0.046
		(0.155)	(0.141)	(0.095)
years mom work age 3-5		-0.093	-0.045	0.002
		(0.063)	(0.060)	(0.035)
mom work age 6		0.028	0.065	0.144
		(0.145)	(0.137)	(0.084)
years mom work age 7 - 10		0.016	-0.025	0.001
		(0.051)	(0.046)	(0.029)
years mom work age 11-12		-0.102	-0.024	0.071
		(0.111)	(0.093)	(0.054)
mom work age 13		0.176	-0.015	-0.090
		(0.214)	(0.171)	(0.094)
mom work age 14-18		0.061	0.058	0.055*
		(0.048)	(0.041)	(0.023)
years dad work age 1-5		0.017	0.131*	0.011
		(0.065)	(0.067)	(0.038)
Parental Controls				
Mother	HS degree	-	-	0.101
		-	-	(0.076)
Mother	Graduate degree	-0.001	-	-
		(0.112)	-	-
Father	HS degree	0.623	0.157	0.158*
		(0.528)	(0.203)	(0.079)
Father	Some College	0.625	0.064	0.119
		(0.512)	(0.198)	(0.095)
Father	College degree	0.502	0.287	0.384**
		(0.511)	(0.215)	(0.148)
Father	Graduate degree	0.458	0.237	0.105
		(0.517)	(0.254)	(0.226)
Mother	no RE	0.102	-0.583*	-0.053
		(0.256)	(0.240)	(0.110)
Mother	Random Effect	-0.007	0.059	0.013
		(0.061)	(0.062)	(0.038)
Father	no RE	1.260*	0.656	0.279
		(0.589)	(0.590)	(0.330)
Father	Random Effect	-0.198*	0.047	0.008
		(0.092)	(0.103)	(0.062)
N daughters-years		20757	27385	51767
N daughters		1387	1834	3441

Sample is girls who were: 1. born between 1978 and 1982; 2. living with both biological parents at time of SIPP panel (1984, 1990-1993,1996); Other regression controls not reported: age, age squared, black, oldest child, youngest child, mother age at child birth, year indicators (1996-2011; 2012 excluded year), log of average family earnings child age 1-5, age 6-10, age 11-13, age 14-18 in levels, squared, cubed; Standard errors in parentheses; * p<.05 ** p<0.01 ***p<0.001

Table 18 Rank Slope for Sons and Daughters

slope coefficient on rank for:	Sons: Main Effect	Sons: Mom Work All Interaction	Sons: Mom Work Some Interaction	Daughters: Main Effect	Daughters: Mom Work All Interaction	Daughters: Mom Work Some Interaction
dad average earn age 28-32 or	0.186***	0.012	0.004	0.119***	0.060***	0.006
1978-1982	0.014	0.015	0.014	0.016	0.016	0.015
dad average earn age 43-47 or	0.202***	0.005	0.007	0.163***	0.037*	-0.001
1978-1982	0.015	0.014	0.014	0.016	0.015	0.015
mother average earn age 28-32	0.045	-0.006	0.009	0.050	0.028	-0.008
or 1978-1982	0.036	0.033	0.033	0.039	0.035	0.035
mother average earn age 43-47	0.125***	-0.034	-0.019	0.165***	-0.023	-0.039
or 1978-1982	0.021	0.019	0.019	0.022	0.020	0.020
family average earn father age	0.224***	-0.046**	-0.011	0.155***	0.015	-0.001
28-32 or 1978-1982	0.017	0.015	0.016	0.018	0.016	0.017
family average earn father age 43-	0.241***	-0.034*	-0.010	0.225***	0.001	-0.017
47 or 1978-1982	0.016	0.015	0.015	0.017	0.015	0.015

Father, mother, son, and daughter average earnings ranks calculated relative to group of same gender, same age range individuals from the SIPP (1984, 1990-1993, 1996 panels). Reported coefficients are from a regression of adult (father, mother, family) earnings rank on child (son, daughter) earnings rank. Son and daughter earnings averaged over age 28-30.

Sons: Distribution of parent ages in 1978

	Father	%	Mothers	%
age 12-22	1435	79.72	2384	132.44
age 23-27	2314	128.56	2530	140.56
age 28-30	1352	75.11	1084	60.22
age 31-37	1473	81.83	863	47.94
age 38-42	278	15.44	104	5.78
age 43-48	116	6.44	21	1.17
age 49-55	18	1.00	-	-
total	6986		6986	

Daughters: Distribution of parent ages in 1978

	Father	%	Mothers	%
age 12-22	1445	51.61	2327	83.11
age 23-27	2164	77.29	2366	84.50
age 28-30	1231	43.96	1043	37.25
age 31-37	1444	51.57	833	29.75
age 38-42	257	9.18	80	2.86
age 43-48	93	3.32	13	0.46
age 49-55	28	1.00	-	-
total	6662		6662	