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

by

Martha H. Stinson[†]
U.S. Census Bureau

Peter Gottschalk[††]
Boston College

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Abstract

We investigate the question of whether investing in a child's development by having a parent stay at home when the child is young is correlated with the child's adult outcomes. Specifically, do children with stay-at-home mothers have higher adult earnings than children raised in households with a working mother? The major contribution of our study is that, unlike previous studies, we have access to rich longitudinal data that allows us to measure both the parental earnings when the child is very young and the adult earnings of the child. Our findings are consistent with previous studies that show insignificant differences between children raised by stay-at-home mothers during their early years and children with mothers working in the market. We find no impact of maternal employment during the first 5 years of a child's life on earnings, employment, or mobility measures of either sons or daughters. We do find, however, that maternal employment during children's high school years is correlated with a higher probability of employment as adults for daughters and a higher correlation between parent and daughter earnings ranks.

Keyword*:* human capital, child development, female labor supply

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

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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)) while others have found limited or no effect (Blau and Grossberg (1992), Baker and Milligan (2010), Dustmann and Schonberg (2012)). But this evidence on short term impacts, such as test scores in school, is not evidence on adult outcomes, such

[1] BLS statistics 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.

as labor supply or earnings. Our paper fills this gap by using the Survey of Income and Program Participation (SIPP) linked to administrative tax data. Together, the survey and tax records form 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 is challenging since maternal employment is almost certainly endogenous. High ability mothers presumably command a higher wage in the labor market and, hence, are more likely to work. However, they may 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 first estimating models that condition on a rich set of observables, including family income at various ages of the child and summary measures of each parent's lifetime earnings, and then estimating a within-mother fixed-effects model that relies on variation in maternal employment across siblings and an IV model that uses geographic variation in female labor force participation and child care costs to instrument for maternal employment.[2]

While our estimation techniques are traditional, the outcomes we are able to study are unique in the literature on maternal employment. We investigate whether the mother working when her child was young impacts how much the child earned in his early thirties; whether the child was more or less likely to work himself between the time he turned 18 and the year 2012; how mobile the child was in the earnings distribution relative to where his parents were ranked when he was young and when he was a teenager.

We find few significant effects of a mother working on her child's adult earnings and find a positive correlation between mother and child employment status only for daughters and only for maternal employment when the daughter is in her late teens. We find that if both the mother and father contribute to family earnings when the child is young, there is no impact on the correlation between parental earnings rank and child earnings rank and hence mobility is largely unaffected. If both parents work and contribute to family earnings when the child is a teenager, the correlation between a daughter's earnings and the parents' joint earnings increases. Thus for a teenage girl, having an employed mother seems to be associated with lower levels of mobility. We suspect this is at least partly a labor supply issue. Daughters with employed mothers are more likely to be employed themselves and this fact alone will push these daughters higher in the rank ordering of earnings, above the segment of women in their thirties who

[2] We estimate log linear earnings regressions on the parents' earnings histories 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 Searle, Casella, and McCuloch (1992) for details. Appendix A summarizes this literature.

earn nothing because they are engaged in full-time child care, and closer to the same part of the earnings distribution as their parents. Taken together, these results seem to suggest that maternal employment when a child is pre-school age has few long term effects on the child's labor market outcomes. Maternal employment when a child is in high school is correlated with future labor supply decisions of daughters, and this may be mostly through the effect of imitation: daughters who see their mothers working are more likely to make similar labor market choices.

Our results are consistent with several possible interpretations. First, it may indeed be true that mothers who stay at home with young children are no more effective at producing human capital for their children than mothers who work outside the home and use bought inputs to invest in their children's future human capital. An alternative explanation for the lack of a clear relationship between mothers' employment and their children's future earnings is that there is unobserved heterogeneity in the mother's relative productivity in the market and in the home. Mothers who are relatively productive in the home become stay-at-home mothers. Those relatively productive in the market work outside the home and buy market goods that are inputs into their children's human capital production functions. While each mother is making the optimal choice for her child, the expected future income of children with working mothers is no higher than the expected future income of children raised by stay-at-home mothers. Unobserved heterogeneity in child raising skills and market skills may mask the effect of the mother's work choice on the child's future income.

Finally, measurement error in home production may mask the impact of mothers' work decisions on children's outcomes. The implicit assumption in many papers, including this one, is that there is a one-to-one relationship between increases in market work and decreases in home production. However, evidence from time use studies suggests even working mothers continue to invest large amounts of maternal time in their children, compensating for working outside the home by doing less housework and having less personal time (Bianchi (2000), Sandberg and Hofferth (2001)).

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 about whether to invest their own time in the development of their child's human capital 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 and present our results. We conclude with an assessment of what we learn from this work.

2 Literature Review

Our study builds on the vast literature that considers the effects of family circumstances on the outcomes of children. This includes much recent literature that has focused on whether early childhood investment (or negative shocks)

produces effects that can be measured later in life (see Almond and Currie (2010) for a summary). It also includes the literature on intergenerational mobility which focuses on whether children do better or worse than their parents, usually in terms of earnings, and what influences this outcome (see Jantti and Jenkins (2014) for a recent review). We tie these literatures together by considering the impact of maternal employment when a child is very young on long range outcomes that include mobility. If, in fact, as Conti and Heckman (2012) suggest, investments early in life are more effective at producing good long term outcomes for children than investments later in life, we might expect that maternal employment at young ages will be more related to children's adult outcomes than maternal employment at older ages.

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 cognitive test scores in early elementary school. Likewise, 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 these studies is that they are only able to follow children for a relatively short amount of time. Only Ruhm (2008) follows children as far as age 11. He finds mixed results of maternal employment depending on the education of the mother. We build on these studies by extending the time horizon many years forward and considering whether these early differences in children's test scores translate into long-run benefits.

Time spent at home by the mother when the child is young provides more opportunity for direct parental involvement in child human capital production but comes at the cost of lower family income. Since the effect of parental income on child development is generally found to be positive (see Blau (1999), Baum (2003), and Dahl and Lochner (2012)), it is not clear a priori what the net effect of maternal employment will be. However, the estimated income effects in these studies are quite small. Thus it is possible that the effects of the loss in income when the mother stays at home are offset by the benefits of the mother's direct input into the child's training. It is also possible 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. Our study is able to consider the combined effects of family income and

maternal time at home by controlling for both types of inputs at every age of the child.

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 to identify the effects of maternal employment. 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 to estimate the casual effect of mothers working on high school outcomes. The results of these studies are mixed, with Rasmussen (2010) and Dustmann and Schonberg (2012) 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 these newer studies by considering outcomes for children after they have entered the labor market and (most likely) finished their schooling.

Intergenerational mobility is another long-run outcome that has generated particular interest recently, partly due to work that 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 their parents. They rank children based on family income at age 30 and rank their parents based on income in the years the child was age 12-16. These data are used to estimate the rank-rank correlation which is the slope coefficient from an OLS regression of child rank on parent rank. Their estimates are in the range of .3, meaning that for every percentile higher the family is located in the earnings distribution, the child will be approximately a third of a percentile higher in his or her adult earnings distribution. Thus compared to a child from a family that was in the 50th percentile of the earnings distribution, a child from a family in the 80th percentile would be predicted to be 9 percentiles higher in the adult earnings distribution. Recent work by Rothbaum (2015), using similar linked Census Survey-SSA-IRS data to ours, shows that in addition to family income, family demographics are also highly correlated with a child's adult earnings rank. In particular parental education and race are significant predictors of child rank, even after controlling for parental earnings. We build on these analyses by estimating a rank-rank correlation model that considers the direct effect of maternal employment and the composition of family earnings on the rank outcome of the child. This model allows us to test whether the same level of family earnings has a different correlation with child earnings if the mother contributes part of the earnings versus if the father earns everything alone.

3 Analytical Framework

3.1 Endogeneity Concerns in a Basic Model

We consider our question about the impact of maternal employment in the framework of a static utility maximization problem where mothers get utility from consumption and child human capital. Mothers make choices about the optimal amounts of these two commodities by choosing 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, their child human capital production abilities, the earnings of their husband, and the husband's inputs into child human capital development.[3] The optimal amount of consumption and child human capital that is the solution to the utility maximization 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 which translate into foregone consumption. 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 mother's 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 the mother's 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, academic enrichment activities, and transportation to these 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 lower.

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

[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 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.

specification that uses covariation in maternal 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, which because of the time period we are considering, we treat as exogenous.[4] 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 wives who stay 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 sorting 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

Consider the following linearized version of the relationship between the child's adult earnings, Y_{it}, and the mother's employment status, 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} is a time specific unobservable characteristic of the child. Given the growing consensus that the effects of parental inputs are largest when the child is young (see Conti and Heckman (2012)), we include a set of interactions between the child's age and the mother's employment status. This allows us to test the effect of working at different points during childhood. We define the age categories to correspond to the common educational periods of a child's life in the United States: pre-school age 0-5, elementary school age 6-10, middle school age 11-13, and high school age 14-18.

The key to obtaining consistent estimates of γ, the effect of a mother working at various ages of her child, on the child's adult outcomes, is to address the potential endogeneity between the mother's work pattern, W_i, and the child's

[4] In a more recent time period, this assumption would likely be unrealistic, but in the late 1970s and early 1980s, we think it unlikely that men significantly reduced their hours or chose to stay home due to childcare responsibilities with any frequency.

unobserved θ_i. In other words, we need to find variation in parent work patterns that is independent of the child's unobservable traits. We describe three methods that we employ, each commonly used in the literature on maternal employment.

3.2.1 Control on Observables

First, we control for observable parent characteristics including age of the mother when the child was born, education levels for both parents, and estimates of parental ability created from statistical measures of each parent's work history. We also include average family earnings during the same childhood periods defined above. To create parental ability estimates, we stack annual earnings measures and parental characteristics and use a mixed-effects model of the following form to estimate separate earnings equations for mothers and fathers:

$$Y^m = X^m\beta + Z\theta^m + \eta \quad (2)$$

$$Y^f = X^f\beta + Z\theta^f + \eta \quad (3)$$

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 $\widetilde{\theta^m}$ and $\widetilde{\theta^f}$. We treat the predicted values of the random effects for the parents of each child i, $\widetilde{\theta_i^m}$ and $\widetilde{\theta_i^f}$, as measures of mother and father unobserved ability in the formal labor market. 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 characteristics included in X. Thus using these predicted random effects allows us to control for observed ability (education) and unobserved ability (variation in earnings not explained by education) of the parents when estimating child earnings equations.[5] For fathers we use all earnings years from 1978 to 2012 to estimate these effects. For mothers, we use earnings from years between when the youngest child turned 11 and 2012 in order to try to mitigate the impact of unobserved part-time work which deflates earnings in a way unrelated to ability.[6] We control for parent age, race, education, labor force experience, calendar year, and firm characteristics such as industry and firm size in X^f and X^m. More detail about the firm characteristics data is given in Section

[5] A more traditional fixed effect estimate from a parent's earnings equation would not have allowed us to separate the effect of parent education and unobserved characteristics in the child earnings equation as the fixed effect would have contained the effect of education as well as any unobservable, time-invariant characteristics.

[6] In other words, if a mother earns only 25,000 in a year, it is necessary to know whether she worked full-time or only a few hours a week in order to determine whether she is a low-skill, low-paid worker or a high-skill, high-paid worker. However since we do not have any information about number of hours worked in our administrative data on earnings, we are unable to distinguish between these two alternatives. In order to avoid lumping these different types of workers together, we restrict ourselves to a time period in a woman's life when labor supply is more likely to be uniform in terms of hours worked.

4. For a relatively small group of mothers and fathers we are unable to predict a random effect because they have no history of paid employment during the time period we use to estimate the mixed effects models. This is not a case of missing data but rather structural zeros due to lack of participation in the labor market. We control for this circumstance in our regressions by including indicators for whether each parent has a predicted random effect.

In addition to estimating equation 1 for the full sample, we also address the issue of the endogeneity of parental work by limiting the sample to sub-groups that are more homogeneous and less likely to differ in unobservable ways. In our first sub-sample, we compare parents with the same labor force attachment before their first child was born by restricting our sample to children whose fathers worked continuously when the child was between the ages of 1 and 5 and mothers who were working before the child was born. This sub-sample is potentially more homogenous than our original sample because it contains women who were all working the year before their child was born and hence had at least some labor market attachment. The effect of working after the child is born is then identified from variation in the mothers' decisions about whether to quit work or stay employed. Since all the fathers were steadily employed as well, the women's alternatives are similar to each other.

We also restrict our original sample to oldest children whose fathers worked continuously when the children were between the ages of 1 and 5 and whose mothers were working before the children were born. As with the previous sub-sample, these mothers were already employed and faced the decision of whether to stay home or work after having a baby, but in this case they were making the decision for the first time, with no other children to take into consideration. This sub-sample is again a more homogenous group because the mothers all had only one child after the baby was born.

We also use the birth of a younger sibling to identify potentially exogenous variation in the mother's labor supply. For example, consider a mother who works when she has only one child. If she has an additional child, she may leave the labor market since time at home now serves two children. Hence, working parents may decrease their labor market activity when the second child is born. To take advantage of this potential change in mothers' work decisions, we limit the sample to oldest children whose mothers and fathers had strong labor force attachment when they had only one child. Among these, we then further restrict the sample to oldest children who had younger siblings born before the oldest turned 5. These are the families who may limit their labor market activity when the second child is born, potentially impacting the older child.

Variation due to changes in family size, however, is only exogenous if parental decisions about spacing between births is unrelated to unobserved maternal ability. If high ability mothers who are more likely to work tend to space their children differently in order to diminish child care costs for preschoolers, the birth of the second child does not represent an exogenous impact on mother labor supply. For example, a mother may delay a second birth because she cannot afford the child care for two children at once or she may shorten the interval between births in order to have fewer total years when she needs full-

day child care and to take advantage of a lower marginal child care cost for the second child in the form of sibling discounts. Since controlling for endogenous fertility decisions is beyond the scope of our paper, we interpret these results with caution.

Finally, we consider whether characteristics of the mother's employer have any impact on child outcomes. Our hypothesis is that some types of work are more flexible and family-friendly than others and hence mothers working at some types of firms might be better able to balance work and home responsibilities. Thus two mothers earning the same amount of money but working in two different industries or firm size-classes might have different amounts of time to spend with their children. For example, if larger firms provide more family-oriented benefits and higher earnings for the same number of hours worked, we might expect to see an impact on children. Likewise, if the mother works in the retail industry and has few benefits and low earnings per hour worked, this may also have an impact. We investigate this possibility by including indicators for maternal employment by broad industry and firm size categories (see Section 4 for details on how these categories are created).

3.2.2 Instrumental Variables

We also use IV methods to control for endogeneity of mother's work attachment when the child is young. State-year-level female labor force participation rates, per capita counts of child care facilities, and averages of payroll per employee at child care facilities are used as instruments. These variables represent local conditions faced by mothers that may cause exogenous shifts in their labor supply if geographic location is not correlated with mother and child unobserved ability. Our hypothesis is that if child care is more plentiful or cheaper or working is more common among a woman's peers, then she will be more likely to work.

3.2.3 Variation in maternal employment across siblings

Following the literature, we also control for time invariant attributes of the family. These mother fixed effects models use only within-mother variation in maternal work to see if siblings' adult outcomes depend on whether the mother worked during their formative years.[7] To estimate this model, we use the sub-sample of children with same gender siblings who were also born between 1978 and 1982 and hence have earnings histories stretching to their thirties. Thus we use sons who had brothers born in this 5 year window and daughters who had sisters born in the same time period (see Section 4 for details). The advantage of these restrictions is that family conditions besides the mother's employment status are likely to be quite similar for both siblings. The disadvantage is that these restrictions produce a small sample both for sons and daughters and

[7] In this model, mother and father fixed-effects are not separately identifiable because both siblings had the same mother and father. This is due to our method of linking parents and children that requires us to observe children living with both their biological parents.

hence this estimation strategy may suffer from lack of precision. It is also the case that if unobservable child characteristics that impact child outcomes are associated with maternal labor supply, this model will produce biased results. Thus if a mother is less likely to work when the second child is young because that child has a disability, the effect of her employment on the earnings of her children will be biased upward.

3.2.4 Logistic Models

We use logistic regressions to estimate models predicting the probability of annual employment from the child's 18th year to calendar year 2012. For these models, we use the same parental controls and family earnings measures as in the previous models and continue to interact maternal employment with age categories for the child.

3.2.5 Rank-Rank Correlation Models

Our final analysis uses our longitudinal earnings history to estimate rank-rank correlations between parent and child earnings in the same manner as CHKST (2014). We first calculate average couple earnings when the child is age 1-5 and age 14-18. Next we rank fathers of children in our sample on the basis of couple earnings relative to all SIPP 1984, 1990-1993, and 1996 panel male respondents born in the same birth cohort and ever having children. We divide men into cohorts born between 1923-1929, 1930-1935, 1936-1940, 1941-1947, 1948-1950, 1951-1955, and 1956-1966. A man without a spouse in the comparison group has couple earnings equal to his own earnings. The same is true for men in our sample and men in the comparison group who have non-employed spouses. For sons and daughters, we calculate average earnings between age 28 and 30 and rank them relative to other same-gender SIPP respondents born between 1978 and 1982.

After obtaining each child's and each couple's rank, we regress the child's rank on the couple's rank. We do this analysis both for couple average earnings when the child is age 1-5 and for average earnings when the child is age 14-18 in order to compare the impact of the mother working at different ages of the child. We include an indicator for whether the couple earnings include maternal earnings or not and also interact this maternal employment indicator with the couple's rank. The maternal employment indicator provides an estimate of the direct effect of the mother working on the child's rank. The interaction tests whether the correlation with child rank changes if some of the family rank was determined by money earned by the mother. Thus we are able to compare two families of the same rank where one family obtained all its earnings through the father and the other obtained its earnings through both the father and the mother. A significant main effect indicates that maternal employment is associated with different levels of child rank, regardless of couple earnings. A significant interaction term indicates that the earnings source matters for the child's mobility outcome. A positive sign on the interaction means children of

working mothers have earnings that are more closely correlated to their parents' earnings, i.e. the children are less mobile. This would indicate that maternal employment provided an advantage for children in high-ranked families. A negative sign on the interaction means that children of working mothers are more mobile, which would indicate that maternal employment provided an advantage for children from low-ranked families. The relative magnitudes of the main effect and the interaction provide evidence on whether additional family earnings, added by the mother and leading to a higher couple rank, overcome any deficit in expected child rank due to the direct impact of maternal employment.

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.[8] 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, 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. Finally we require that average family earnings when the child was age 1-5 be positive. This restricts our sample to families where at least one parent worked at least 1 year when the child was preschool age.

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 and construct annual measures of maternal and paternal employment and total fam-

[8] 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.

ily earnings to age 18 of the child. Children are also followed forward through 2012, and we construct 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, 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.[9] 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 by running the regression models separately on each implicate and then combining the results using the standard Rubin formulae.[10] Quite simply, the reported regression coefficients are the average calculated across all four implicates and the standard errors are a weighted average of the average variance of each coefficient and the between-implicate variance. Exact formulae are given in Appendix B. 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.[11] From the survey, we also make use of the

[9] 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.

[10] The more common practice is to drop observations containing missing data. This has the disadvantage of reducing sample size and potentially introducing bias into the sample if individuals who are missing their SSNs are different from those who are not. Due to these concerns, we prefer to handle the missing data problem by imputation. We have also run all the regression models in the paper dropping observations with missing SSNs. The results are noisier but otherwise give the same conclusions in terms of sign and significance of the coefficients.

[11] 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

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 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 1976-2012. 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. 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. For the parent earnings equations, we assign earnings-year observations without firm size or industry information to a "missing" category.

Second, we divide mothers who were employed when their children were age 1-5 into groups based on industry classification and firm size. Mothers who worked in the agriculture, forestry, fishing, mining, utilities, construction, manufacturing, wholesale trade, or transportation sectors are assigned to the production industry group. Mothers who worked in the information, finance, insurance, real estate, professional, scientific, technical, management, administration, education, health care, public service, or government sectors are assigned to the skilled services group. Mothers who worked in the retail, arts, accommodations, food services, or other services sectors are assigned to the unskilled services group. Mothers with self-employment are assigned to the self-employment group. We assign firm size groups in a similar manner. Mothers who worked at firms with 1-50 employees are assigned to the small firm group; 51-500 employees is the medium firm group; and over 500 employees is the large firm group. Mothers may be assigned to more than one industry and firm size group if they have jobs in different industries and/or at different firms during the period their child was age 1-5.[12] We include these indicators in one child

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.

[12] This includes assigning the mother to the missing firm characteristics group if any of her

earnings regression in order to capture the effect of the mother being employed and allow that effect to vary by type of employment. We drop children whose mothers do not have valid SSNs since the LBD variables have not been imputed.

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 2012. 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 Tables 1A and 1B we show summary statistics for our sample of sons and daughters. 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 26.8 years.[13] Among these sons, 51% had mothers with no more than a high school degree, and 46% had fathers with a high school degree or less. Average total mother and father earnings when the son was age five or under was approximately $62,000 in 2012 dollars.[14] At age 30, average total earnings of these sons was around $46,000 in 2012 dollars, and average labor force experience was 12.6 years.

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 sons 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

employers did not match to the LBD. Being in the missing group means that a mother had an SSN and matched to the W-2 records but the employer identifier from the tax records could not be found in the Census firm data. Therefore, being in the missing group does not preclude the mother from also being in other industry and firm size groups if she had other W-2 employers who do match to the LBD.

[13] Each completed implicate has a slightly different sample size. In all tables, we report the sample size of the smallest of the four implicates.

[14] 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.

between different types of maternal working patterns. For most characteristics, 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 less likely to have siblings under the age of 5. Their mothers are more educated but have lower random effects. However the No Work mothers are more likely to have no random effects because they never worked for pay after the youngest child reached age 11 and hence have no W-2 work history in the years we used to predict a random effect. Mothers who work when their sons are preschool-age continue to work more years throughout the son's school-age years. Sons in the All Work group have higher family earnings on average 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 significant. However fathers of sons in both the All Work and Some Work groups have lower random effects on average and are more likely to have no W-2 employment history and hence no predicted random effect. There are no statistically 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 daughters. The most noteworthy thing about this table is that our sample of approximately 6700 daughters looks remarkably similar to the sample of sons 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 under 5, and percentages of mothers who worked are very similar on average to sons. Similar trends hold that daughters in the All Work and Some Work groups also have more highly educated mothers compared to the No Work group; slightly more educated fathers; and fathers with lower unobserved ability. The only major dissimilarity between the genders is in average earnings at age 30, where the mean is lower for daughters than sons. This is true despite sons and daughters having similar levels of work experience. We expect this result is probably due in part to unobserved labor supply differences. Daughters may work fewer hours per week at age 30 than sons 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 was 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 on labor supply and no occupation information because the W-2 records we receive do not contain dates worked, hours, or occupation. 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

5.1 Child earnings at age 30 and older

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 6035 sons with 16,598 years of positive earnings at age 30 or older.[15] In column one we show results from our most restrictive regression which, in addition to mother work indicators, includes only the following son characteristics: age, age squared, black, oldest child, youngest child, mother's age at birth of the son, and year indicators for 2008 through 2011. We specify the mother work variables as indicators for working when the son was age 0 and between the ages of 1-5, 6-10, 11-13, and 14-18. The maternal work indicator for age 0 helps to account for mother heterogeneity as it also captures the mother's work decisions in the part of the year before the child was born. Given the differences we observed in Table 1A and 1B between mothers who work some years and mothers who work all years when their children are age 1-5, we split the work indicator for this age range into two components: work some years (i.e. between 1 and 4 years) and work all years (i.e. work 5 years).[16] None of the coefficients on the mother's work indicators are significant. However, the coefficient on years of father working when the son was 1 to 5 is significant and positive. In column 2 we add controls for parent education, random parent effects from our parent earnings regressions, indicators for parents who had no random effects because they had no work histories during the time period used to estimate the mixed model equations, and average family earnings when the son was 1 to 5, 6 to 10, 11 to 13, and 14 to 18. With these controls, neither mother's or father's work coefficients are significant. This implies that the initial positive coefficient on father's work when the son was 1-5 was capturing correlated unobservables.

The next two columns show slightly different results for daughters. In

[15] These sample sizes are from the smallest completed data implicate.

[16] For example the All Work mothers are more likely to have a college degree.

neither column 3 or 4 is the father's years of work when the daughter was 1 to 5 statistically significant. In column 3, the indicator on mother working some years when her daughter is age 1-5 is negative and significant. Even when we add parental controls, the coefficient on mother working some when the child was a preschooler remains negative and significant although it has decreased in magnitude. We also stratified by mother's education in order to allow all coefficients to vary by education, and find that the negative effects of the mother working are confined to mothers with a high school degree or less. We conclude from this that these regressions may not adequately control for differences in father and mother underlying ability.[17]

To further highlight the impact of the parent observable characteristics, we also report the coefficients on the parent education indicators and random parent effects in Table 2. As we expected, parental education is a very strong predictor of future child earnings, as are both parent random effects. Interestingly, father education has the strongest impact on both sons and daughters. Having a father with a graduate degree predicts the son's earnings to be 40% higher than the earnings of a son with a father without a high school degree. The equivalent comparison is 32% for daughters. In comparison, the magnitude of the impact of the parental random effect is much smaller. A one standard deviation increase above the mean in the mother's random effect predicts a 4.8% increase in the son's earnings and a 6.3% increase in the daughter's earnings. The impact of the father's random effect is larger, with a one standard deviation increase above the mean predicting 11% higher earnings for the son and 5.4% higher for daughters. These coefficients lead us to conclude that much of the correlation in parent-child earnings is due to correlation in education levels and unobserved factors such as ability.

In Table 3, we re-estimate the model on several sub-samples in an effort to further control for unobserved differences in families that may be correlated with both maternal work decisions and children's outcomes. In the first column, we restrict the sample to children of parents with strong labor force attachment prior to having a child in order to compare families where the mother was working the year before the child was born.[18] The only coefficient that is significantly different from zero is the coefficient on mothers who work when the son is 14-18. Interestingly, the coefficient is positive, indicating that sons have higher expected earnings when they become adults if their mothers worked when they were teenagers.

In Column 2, we further restrict the sample to oldest child so the mother is facing the decision for the first time of whether to use her own time or bought inputs in the child's human capital production function. Imposing this restriction has little impact on the coefficients but the smaller sample size increases the standard errors. As a result, none of the coefficients on parental work are statistically significant.

[17] Results available from the authors.

[18] We restrict the sample to sons and daughters of mothers who were working the year before the child was born and fathers who worked every year from when the child was age born to 5.

Finally in Column 3, we further restrict the sample to children who had a younger sibling born before the oldest child turned 5. If different mothers face different costs of going back to work after having a second child then the arrival of the second child could provide some exogenous variation in mother employment. However, in this sample also, none of the work variables are statistically different from zero.

In Table 4, we present results from models where we control for mother employer characteristics. Here we find that the indicators for maternal employment by industry category are all positive for sons but not statistically significant. In contrast maternal employment in the service sectors groups is negative for daughters while production and self-employment are positive. However these results are also not significant. We do see a significant negative correlation between the mother working at a large firm when her son is age 1-5 and son earnings as an adult. More research is needed to see what factors might be driving this result. One possibility is that the large firms that tended to employee mothers of young children in the late 1970s and early 1980s were often large retail chains which did not provide the benefits and higher pay often associated with large firms.

In Table 5 we show results from an IV estimation in which state-year-level female labor force participation, child care centers per capita, and average payroll per employee at child care centers are the excluded instruments. Again there are no mother work coefficients that are statistically significant for sons or for daughters. We test for weak instruments using the standard F-test that measures the joint significance of the excluded exogenous variables in the first stage regression. While these variables are often significant, the F-statistic is not sufficiently high to alleviate concerns about weak instruments.[19]

Thus far we have controlled for observables, implicitly assuming that the remaining unobservables were random. We now relax this assumption by presenting estimates of models that include fixed effects. In Table 6 we use variation in mother work choices across brothers and across sisters to identify the effect of maternal employment at young ages. Again none of coefficients are significantly different from zero.

In summary, these results lead us to believe that there is no significant correlation between mothers' labor force participation and their children's adult earnings, once we have controlled for parental characteristics correlated with mother, father, and child 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 is too coarse to pick up effects. It is also possible that there truly are no effects of mother employment once we account for family earnings. If mothers are truly optimizing when choosing their hours of employment 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

[19] Stock, Wright, and Yogo (2002) argue that only values of 10 or higher reliably mean there is not a weak instrument problem.

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.

5.2 Child employment from age 18 till early 30s

While mothers' decisions about whether to work when their children are young may not have an impact on the quality of the jobs the children get when they are adults, mothers may nonetheless be role models, influencing their children so that they are more likely to work if their mothers do. To explore this possibility, we re-estimate some of our models with adult labor force participation as the dependent variable.

To explore this intergenerational link, Table 7 shows the results of a logistic regression using presence of W-2 earnings as an annual indicator for employment between the age of 18 and age in 2012, which ranged from 30-34. 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. However when we add parental controls in column 2, we see that this correlation is no longer statistically significant. Here we see different results for sons and daughters. In columns 3 and 4, the mother and father working when the daughter is high school age are both correlated with an increased probability of the daughter working as an adult. We conclude that the influence of the mother (or father) as a role model is stronger with daughters than sons in regards to employment. This is not surprising given the relatively high percentages of men in their twenties and thirties who work and the still common phenomenon of women taking time off from the formal labor market when raising young children. Sons are a more homogeneous group than daughters in terms of their expectations about whether they will work and hence are less influenced by maternal example. Daughters, on the other hand, may shape their beliefs about the feasibility and desirability of employment outside the home by watching their mothers.

5.3 Intergenerational Mobility

Finally, we report measures of intergenerational mobility in Tables 8 and 9. In these tables the outcome of interest is the child's rank in his or her adult earnings distribution. The focus is on the role of family earnings rank in predicting the child's rank and whether parental composition of family earnings matters. We consider family earnings rank in two different time periods in order to determine whether family rank from early childhood or from the high school years is a better predictor of adult rank.

We show several different specifications of the rank-rank correlation model. In the first column of both Table 8 and 9, we show the simplest model which includes only family average earnings rank, either from the child's preschool years or the child's high school years, and indicators for maternal employment.

In the second column we add controls for demographic characteristics: an indicator for whether the child is black, age of the mother at birth of the child, age of the father at birth of the child, and mother and father three-category education indicators.[20] In the third column, we again include the demographic characteristics but drop the maternal employment indicators and instead interact maternal employment with family earnings rank in order to test whether the presence of maternal earnings changes the correlation between family and child rank. Finally in the fourth column we include all the controls from the previous columns: demographic characteristics, indicators for maternal employment, and interactions between maternal employment and family earnings rank.

In the first row of Table 8 we report the correlation between family earnings rank when the child was preschool age and child rank as an adult across all our different specifications. For sons, the correlation ranges from .19 to .12, meaning that for every percentile higher in the earnings distribution the family ranks, the son is predicted to rank between 12% and 19% of a percentile higher.[21]

In the second row, the indicator for maternal employment in all 5 preschool years is negative and significant for sons but only before we include the interaction term with family earnings rank (columns 1 and 2 versus 4). The interaction term between maternal employment in all five preschool years and family rank, reported in row 3, is negative and significant when it is included by itself (column 3) but is not significant when the indicator for maternal employment is included (column 4). Looking at the full specification in column 4, the point estimates indicate that maternal employment in all preschool years has a direct negative impact on the son's adult earnings rank but no effect on the correlation between family earnings rank and child earnings rank. However, the standard errors are high enough that we cannot say the main effect of maternal employment is significantly different from zero. If the mother worked only some years when the son was age 5 and under, there is no evidence of an impact on the son's earnings rank as an adult, either through a main effect or through an interaction with family earnings rank. Considered together, these results lead us to conclude that maternal employment does not have an impact on the mobility of sons relative to their parents' place in the family earnings distribution when

[20] Education indicators are mother high school degree of less, mother some college, mother college degree or higher, father high degree or less, father some college, father college degree or higher.

[21] The range of our rank-rank correlation estimates is lower than the .3 found by CHKST (2014). There are two likely reasons for this difference - one having to do with the type of data we use and one having to do with our sample of individuals. First, we use only earnings and not total family income to measure our correlations because we have only W-2 records filed by employers with SSA/IRS on behalf of individual employees and not Form 1040 records filed by couples with IRS to report income for tax purposes. 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 .22 for family earnings during high school and son earnings as an adult is closer than might have been expected to the CHKST estimate.

the son was very young.

Columns 5-8 show the results for daughters. Here we see that family earnings rank is the only statistically significant predictor of daughter earnings rank as an adult. Maternal employment when the daughter is age 1-5, both all years and some years, is never significant nor is the interaction of maternal employment and family earnings rank. As with sons, it appears that maternal employment at very young ages does not impact the mobility of daughters.

In general daughters have a much lower correlation between family earnings rank and their own rank as adults, beginning at .14 and dropping to .04 after all controls are included. We hypothesize that this may again be a labor supply issue. Unlike the earnings regressions from section 5.1, the rank-rank correlation model does not drop individuals with zero earnings but rather assigns them to the bottom of the distribution. Daughters may be choosing not to participate in the formal labor market when they are in their thirties and hence will rank very low in the earnings distribution. Because of this possibility, for daughters, we would ideally like to have family earnings at age 30 in order to see how their adult family earnings rank is correlated with their parents' earnings rank. However since we have no data on marital status or the identity of a spouse for individuals after the end of the SIPP data collection period, we are unable to test this hypothesis at this time.

In Table 9, we show results from models that estimate the correlation between the family earnings rank when the child is in high school and the child's earnings rank as an adult. Here we find different results than in Table 8. While we find no significant effects of maternal employment during high school on sons, we do see a positive correlation for daughters. If the mother worked during this time period of the daughter's life, the correlation between family and daughter earnings rank increased. We again hypothesize that this is the result of different labor supply decisions, on average, by daughters of employed mothers. If the mother worked when the daughter was in high school, the daughter is more likely to work and will consequently be ranked higher in the earnings distribution. This causes the correlation between her rank and her parents' rank to more than double (baseline correlation is .062 (row 1, column 8) and the interaction term is .078 (row 3, column 8)). This result substantiates what we found in Section 5.2 and leads us to conclude that maternal employment when the daughter is too young to remember or to understand any alternative has less impact than maternal employment at a point when the daughter is beginning to make her own educational and career choices. However these results do not provide us with a complete picture of the effects of maternal employment on daughter intergenerational mobility. Since we cannot rank daughters on the basis of family income, we do not know how they truly compare to their parents in the overall family earnings distribution.

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 at best through high school. In this paper, we are able to overcome this major impediment through the use of linked survey-administrative data. Our ability to link W-2 earnings data to mothers and children allows us to measure parental input and adult outcomes of children many years apart.

With these data we were able to estimate standard models that deal explicitly with the endogeneity of the parents' work decisions. 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 preteen outcomes. 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 mothers 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. (2014) tell a mixed story. The mobility of a son relative to his parents does not appear to be affected by maternal employment at any age. For a daughter, we see that her earnings are more closely tied to her parents earnings (i.e. she is less mobile) if her mother worked during the daughter's high school years. We hypothesize that this is a labor supply issue, with daughters more likely to work in general if their mothers did. The impact of changing from not employed to employed on the daughter's place in the rank ordering of earnings is large enough to significantly impact the mobility estimate. More work is needed to disentangle the sample selection effects of which mothers and children are working, whether this is different for sons and daughters, and how this relates to mobility measures.

It is also important to remember that the birth years we study, 1978-1982, are early enough that the majority of women with young children in the United States were not employed. According to the Bureau of Labor Statistics, only 44% of women with children under age 6 were part of the labor force in 1978 and only 39% were actually employed. Working mothers from that time period may have been a different group than working mothers today and hence we caution that our results may not generalize to the current generation of mothers with young children. Certainly larger U.S. economic trends such as stagnant male wages and declining marriage rates play a role in the reasons women work today and advances in technology that allow more flexible working hours and job locations make working a different experience for today's mothers. All of these may also alter the impact of working on children. Whether maternal employment continues to have minimal impact on children's labor market outcomes will be a topic of continuing research interest as the children of the 1980s, 1990s and 2000s age and their work histories become available.

7 Bibliography

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8 Appendix A

In this Appendix we describe the prediction 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 predicting 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 and prediction 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 mean. 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

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

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

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 ith mother.

This model described by 4can be treated as what Greene calls the least squares dummy variable (LSDV) model (page 466) with the following commonly made assumptions:[22]

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

where β and θ are called fixed effects in the statistics literature if the unobservable and observable factors in the population (θ_1 ...θ_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

$$\begin{bmatrix} Z'Z & Z'X \\ X'Z & X'X \end{bmatrix} \begin{bmatrix} \theta \\ \beta \end{bmatrix} = \begin{bmatrix} Z'Y \\ X'Y \end{bmatrix} \tag{5}$$

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

[22] 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.

(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.[23]

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 predicted 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)

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

[23] 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.

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

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

$$
\begin{aligned}
f(y, \theta) &= f(y \mid \theta) f(\theta) \\
&= \frac{\exp\left\{-\frac{1}{2}\left[(y - X\beta - Z\theta)' R^{-1} (y - X\beta - Z\theta) + \theta' G^{-1} \theta\right]\right\}}{(2\pi)^{1/2(N+I)} |R|^{1/2} |G|^{1/2}}
\end{aligned} \tag{6}
$$

By taking partial derivatives of 6 with respect to β and θ, Henderson arrived at what are now known as the mixed model equations (MME) (Searle, Casella, McCulloch page 276).

$$
\begin{bmatrix} X'R^{-1}X & X'R^{-1}Z \\ Z'R^{-1}X & Z'R^{-1}Z + G^{-1} \end{bmatrix} \begin{bmatrix} \widehat{\beta} \\ \widehat{\theta} \end{bmatrix} = \begin{bmatrix} X'R^{-1}Y \\ Z'R^{-1}Y \end{bmatrix} \tag{7}
$$

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 7 to equation 5, 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 to predict the 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.

9 Appendix B

Rubin first proposed multiple imputation as a way to handle missing data problems. In his seminal book (Rubin 1987), he advocates applying any given

imputation method multiple times to create many replacement values for missing data. This approach produces multiple copies of the data set, each copy having its missing values replaced with one of the sets of imputed values. The need for this arises from the fact that extra variability is introduced by the missing data. This variability needs to be taken into account or else the confidence intervals generated for statistics produced using the data will be too small, i.e. parameters will be determined to be significant too often. By generating multiple data sets or implicates, the user can run a standard analysis on each one and then calculate the within-implicate variance (standard variance measure) and the between-implicate variance (variance across the implicates). The total variance formula then has two components which take account of the standard measure of variance and the variance introduced by the imputation. Formally, for a set of statistics $q^{(\ell)}$ (such as a mean or regression coefficient), calculated for each $\ell = 1, .., m$ completed data implicate, a single point estimate, $\bar{q}_m$, and variance measure, T_m, are created using the following formula:

$$\text{average across implicates:} \quad \bar{q}_m = \sum_{\ell=1}^{m} \frac{q^{(\ell)}}{m}.$$

$$\text{variance across implicates:} \quad b_m = \sum_{\ell=1}^{m} \frac{\left(q^{(\ell)} - \bar{q}_m\right)\left(q^{(\ell)} - \bar{q}_m\right)'}{m-1}$$

$$\text{variance on each implicate file:} \quad u^{(\ell)} = u\left(D^{(\ell)}\right)$$

$$\text{average variance across implicates:} \quad \bar{u}_m = \sum_{\ell=1}^{m} \frac{u^{(\ell)}}{m}.$$

$$\text{total variance:} \quad T_m = \bar{u}_m + \left(1 + \frac{1}{m}\right) b_m$$

$$\text{degrees of freedom:} \quad \nu_m = (m-1)\left(1 + \frac{\bar{u}_m}{\left(1 + \frac{1}{m}\right) b_m}\right)^2$$

For details on the imputation models used to create the implicates used in this paper, see Abowd, Benedetto, and Stinson (2013).