The Intergenerational Transmission of Employers and the Earnings of Young Workers

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Abstract

This paper investigates how the earnings of young workers are affected by the *intergenerational transmission of employers*–which refers to individuals working for the same employer as a parent. My analysis of survey and administrative data from the United States indicates that 7% of young workers find their first stable job at the same employer as a parent. Using an instrumental variables strategy that exploits exogenous variation in the availability of jobs at the parent's employer, I estimate that working for the same employer as a parent increases initial earnings by 31%. The earnings benefits are attributable to parents providing access to higher-paying employers. Individuals with higher-earning parents are more likely to work for the employer of their parent and experience greater earnings benefits conditional on doing so. Thus, the intergenerational transmission of employers amplifies the extent to which earnings persist from one generation to the next. Specifically, the elasticity of the initial earnings of an individual with respect to the earnings of their parents would be 10% lower if no one worked for the employer of a parent.

Keywords: intergenerational mobility, labor market networks, job ladders
JEL Codes: D10, J62

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1 Introduction

In the United States, earnings are highly persistent from one generation to the next.[1] The fact that children born into poverty are likely to remain in poverty as adults runs counter to the ideal of equality of opportunity and may be indicative of untapped human potential. But the justification and design of an effective policy response depends on the mechanisms through which parents shape the earnings of their children. Much of the research within economics attributes differences in earnings by family background to differences in human capital (Black and Devereux 2011). However, earnings depend on factors beyond human capital. Indeed, research has shown that who you know affects where you work (Ioannides and Loury 2004) and where you work affects how much you earn (Manning 2011). An open question is then: Do parents also affect the earnings of their children by using their connections to provide access to higher-paying firms?

I investigate how the earnings of young workers are affected by the *intergenerational transmission of employers*–which refers to individuals working for the same employer as a parent. By working for their parent's employer, individuals may gain access to jobs that offer greater pay and more opportunities for career advancement.[2] The intergenerational transmission of employers will increase the intergenerational persistence in earnings if individuals with higher-earning parents are the largest beneficiaries. However, the benefits–which depend on the likelihood and earnings consequences of working for a parent's employer–could be increasing or decreasing in parental earnings. On the one hand, higher-earning parents may be better able to provide access to high-paying jobs. On the other hand, individuals from disadvantaged backgrounds may be more reliant on their parents to find work. I organize my analysis into three sections that: (1) document descriptive patterns in the intergenerational transmission of employers, (2) estimate the earnings consequences, and (3) investigate the implications for intergenerational mobility.

I begin by showing that it is not uncommon for an individual to work for their par-

[1]Intergenerational mobility in the United States is low both relative to the past (Chetty et al. 2017) and relative to other developed countries (Solon 2002).

[2]Both theoretical (e.g., Jovanovic and Nyarko 1997; Gibbons and Waldman 2006) and empirical (e.g., Von Wachter and Bender 2006; Khan 2010; Oreopoulos et al. 2012; Altonji et al. 2016; Arellano-Bover 2020) evidence suggests that early career experiences can have a large and persistent effect on earnings.

ent's employer, especially for individuals with higher-earning parents. I combine survey data from the 2000 Decennial Census with administrative data from the Longitudinal Employer-Household Dynamics (LEHD) program and study the experiences of 10 recent cohorts. I find that 7% of individuals work for a parent's employer at their first stable job, and 29% do so at some point between the ages of 18 and 30.[3] It is possible that some individuals work for a parent's employer by chance. However, individuals are more than 40 times more likely to work for a parent's employer relative to other similar employers in the same local labor market. Instead, a number of results suggest that parents influence the hiring or job search process to help children with limited labor market options. For example, individuals with less education who are searching for a job in periods of high unemployment are more likely to work for a parent's employer. Higher-earning parents are more likely to be employed and hold a position of authority, and are therefore in a better position to procure a job for their child. Indeed, individuals with parents in the top percentile of the earnings distribution are three times more likely to find their first stable job at their parent's employer, relative to those in the bottom percentile.

Next, I find large earnings benefits of working for a parent's employer. Estimating causal effects is difficult because individuals who work for a parent's employer likely differ from those who do not. I address this concern by using an instrumental variables strategy that exploits exogenous variation in the availability of jobs at the parent's employer. Specifically, I instrument for whether an individual works at their parent's employer using the hiring rate at that employer. My empirical specification, which I estimate via two-stage least squares, includes two-way fixed effects for the parent's employer and the local labor market and thus exploits variation in the hiring rate that is orthogonal to time-invariant characteristics of the parent's employer and time-varying local labor market conditions. The empirical strategy bears some similarities to a difference-in-differences estimator as the identifying variation comes from the difference across employers in the differences in the hiring rate over time. I find that individuals earn 31% more at their first stable job when working for their parent's employer relative to their next best option.

[3]My estimates of the rate of transmission are similar to other estimates from the United States (Stinson and Wignall 2018), Canada (Corak and Piraino 2011), and Sweden (Kramarz and Skans 2014).

Individuals with higher-earning parents experience larger gains.

These earnings gains appear to be explained by parents providing access to higher-paying employers. For example, working for a parent's employer leads individuals to work in higher-paying industries, i.e., the manufacturing and production sectors instead of unskilled services. Furthermore, the effect on the employer pay premium–estimated via a model with worker and employer fixed effects as in Abowd et al. (1999)–is virtually identical to the effect on individual earnings. A wide class of models (e.g., Postel-Vinay and Robin 2002; Card et al. 2018) predict that imperfect competition can produce job ladders, where more productive firms occupy higher rungs of the ladder and offer higher wages. Consistent with these models, I find that working for a parent's employer leads individuals to work for employers that pay more on average, are more productive, and tend to poach workers from other employers. In other words, by taking a job at the employer of their parent, individuals start their careers on a higher rung of the job ladder.

Lastly, I find that the intergenerational transmission of employers leads to a modest increase in the degree to which earnings persist across generations. I develop a methodology that uses the estimates from the first two sections of the paper to quantify the implications for intergenerational mobility–as defined by the intergenerational persistence in earnings. The elasticity of the initial earnings of an individual with respect to the earnings of their parents would be 10% lower if no one worked for the employer of a parent.[4] Given that parents could have contacts at other employers, my results likely understate the importance of parental labor market networks more broadly defined.

Non-Black males with high-earning parents are the largest beneficiaries of the intergenerational transmission of employers. Consistent with Chetty et al. (2020), I find that, conditional on parental earnings, Black males have lower expected earnings than White males. On average, the intergenerational transmission of employers explains 10% of this conditional Black-White gap in initial earnings. The intergenerational transmission of employers disproportionately benefits sons of high-earning parents but daughters of low-

[4]Corak and Piraino (2011) and Stinson and Wignall (2018) explore the relationship between the transmission of employers and intergenerational associations in earnings. However, their results are difficult to interpret, as they do not account for the endogenous nature of employer transmission.

earning parents. On average, daughters benefit more than sons, and the gender pay gap in initial earnings would be 4% larger if no one worked for a parent's employer.

My paper sits at the nexus of three large but distinct literatures on intergenerational mobility, labor market networks, and the contribution of firm-level pay policies to earnings inequality. I make four main contributions to these literatures.

My first contribution is to quantify how the intergenerational transmission of employers affects intergenerational mobility. While there is general agreement that parents shape the economic outcomes of their children through a multitude of channels, there is much less agreement about the relative and quantitative importance of the various channels. A common approach in this literature focuses on estimating the causal relationship between characteristics of parents–such as income (Shea 2000), education (Black et al. 2005), or labor market networks (Magruder 2010)–and outcomes of their children.[5] These causal estimates are informative, but they fall short of quantifying the extent to which different channels shape intergenerational associations. Intuitively, a channel will reduce mobility if children with higher-income parents tend to benefit more from that channel than children with lower-income parents. I develop a methodology that formalizes this intuition. Specifically, I show that the difference between observed measures of intergenerational mobility and measures that correspond to a counterfactual world in which no one worked for the employer of a parent can be expressed as a function of the benefits of working for a parent's employer conditional on parental earnings. These benefits depend on the likelihood of working for a parent's employer and the earnings consequences conditional on doing so, two objects that I estimate in this paper. My methodology helps to bridge the gap between the focus on causal identification and the broader research agenda that seeks to understand why economic outcomes persist across generations.

My second contribution is to show that the difference between the earnings of individuals from high- and low-income families is partly attributable to parents using their connections to provide access to higher-paying employers. Most theoretical models of intergenerational mobility build on Becker and Tomes (1979), who focus on the trans-

[5] Of these papers, Magruder (2010) is most closely related to my paper. Magrduer (2010) finds that parental labor market networks help young unemployed workers find a job in the context of South Africa.

mission of human capital across generations. In this framework, parents influence the human capital of their children by passing on genetic material and shaping childhood experiences. When the children reach adulthood and enter the labor market, differences in earnings are attributable to differences in human capital, which commands the same economic rewards regardless of family background. However, a growing body of evidence suggests that imperfect competition and frictions in the labor market lead earnings to depend on factors beyond human capital, and these other factors have a significant impact on earnings inequality within a generation. My results suggest that these same market imperfections also shape how inequality is transmitted across generations.

My third contribution is to obtain credible causal estimates of the earnings consequences of finding a job through a social contact. Research on labor market networks establishes the widespread use of social contacts in the hiring and job search process, but there is mixed evidence on the magnitude, and even the sign of, the earnings consequences (Topa 2011). Many estimates of the earnings consequences (e.g., Kramarz and Skans 2014; Stinson and Wignall 2018) are based on empirical strategies that lack exogenous variation in the method of job finding.[6] These estimates vary widely across papers, and the disagreement likely stems from an inability to fully account for factors that lead workers to use social contacts (Loury 2006). A number of recent papers convincingly establish that social contacts can improve labor market outcomes by reducing the duration of unemployment (Beaman 2012; Cingano and Rosolia 2012; Glitz 2017), helping workers find jobs at high-paying firms (Schmutte 2015; Eliason et al. 2019), and strengthening workers' bargaining positions (Caldwell and Harmon 2019).[7] Unlike these papers, I estimate the magnitude of the earnings benefits of finding a job through a specific social contact versus some other method and find that the benefits are large. Supplemental

[6]Kramarz and Skans (2014) use data from Sweden and estimate the earnings benefits of working for a parent's employer by controlling for observable differences between children who do and do not work with their parents. Stinson and Wignall (2018) use data from the United States to estimate the earnings consequences of working for a parent's employer using individual fixed effects.

[7]Eliason et al. (2019) study how social networks (including family networks) shape earnings inequality by affecting how workers sort into firms. Two important differences between their paper and my paper include: (1) I focus on how parental connections affect the transmission of inequality across generations, i.e., the intergenerational persistence in earnings, and (2) I estimate the causal effect on earnings by exploiting exogenous variation in the use of connections.

analyses rule out threats to identification that could arise from factors such as time-varying offer wages (results are robust to controlling for time-varying offer wages), local labor demand shocks (earnings are unrelated to hiring at other employers in local labor market), and heterogeneity across households (similar results when comparing siblings).

My fourth contribution is to provide novel empirical evidence that firm-level pay policies are an important determinant of earnings. A substantial portion of earnings inequality is attributable to differences in average pay across firms. But competing explanations emphasize the role of imperfect competition in generating dispersion of firm-level pay policies (e.g., Mortensen 2003; Manning 2003) versus the sorting of workers into firms in a perfectly competitive labor market (e.g., Gibbons and Katz 1992). Prior research finds that moves to higher-paying firms are associated with earnings growth (e.g., Abowd et al. 1999; Haltiwanger et al. 2018). However, the changes in earnings are not necessarily explained by differences in firm-level pay policies since worker mobility is endogenous, and factors that lead workers to change firms could be correlated with factors that have an independent effect on earnings. I provide more direct evidence that moves to higher-paying firms have a causal effect on earnings since my empirical strategy isolates exogenous variation in where individuals are employed.

The remainder of the paper is structured as follows. Section 2 discusses the data. Section 3 documents patterns in the intergenerational transmission of employers. Section 4 estimates the earnings consequences of working for the employer of a parent. Section 5 investigates implications for intergenerational mobility. Section 6 concludes.

2 Data

I rely on two main sources of data: (1) the Hundred Percent Census Edited File (HCEF), which measures the relationship between parents and children who are living together in 2000 and (2) data from the LEHD program to measure labor market outcomes of both parents and their children between 2000 and 2016. The HCEF contains all responses from the 2000 Decennial Census Short Form and, in principle, includes all individuals living in the United States in 2000.[8] The LEHD is an employer-employee linked dataset produced by the U.S. Census Bureau and is constructed from two core administrative datasets: (1) unemployment insurance (UI) records, which provide job-level earnings records and (2) the Quarterly Census of Employment and Wages, which provides establishment-level characteristics. The earnings records in the LEHD capture roughly 96% of private non-farm wage and salary employment in the United States (Abowd et al. 2009). Employers are identified by a state-level employer identification number (SEIN), which typically captures the activity of a firm within a state and industry.[9] The LEHD covers most jobs, but a notable exception is self-employment. While previous work, such as Dunn and Holtz-Eakin (2000), documents strong patterns of intergenerational persistence in self-employment, I focus on more formal employer-employee relationships.

The sample frame is defined based on the HCEF and includes children who are living with their parents in 2000 and who were born after June 30th of 1982 and before July 1st of 1992.[10] The cohorts were chosen so as to focus on a set of individuals who are young enough to likely have lived with their parents in 2000–the oldest individual in the sample was 17 years old when data collection for the 2000 Decennial Census took place–but old enough to have likely entered the labor market by 2016–the youngest individual in the sample was 24 years old by the end of 2016. There are approximately 37 million individuals in the sample frame. See Appendix B.1 for details.

I implement two sets of sample restrictions. First, I require that the individuals and their parents found in the HCEF can be linked to the LEHD. In order to account for non-random attrition from the sample due to issues associated with linking records across the two data sources, I construct sample weights and use them to produce all descriptive results. Second, I drop cases in which the earnings of the children or parents are likely to be affected by coverage issues in the LEHD. Of the 37 million children in the sam-

[8] In practice, some individuals are not surveyed in the 2000 Decennial Census and non-respondents are more likely to be minorities or lower-income households. See Appendix B.1 for a more detailed discussion of the coverage issues.

[9] A worker could have positive earnings at multiple employers in a given quarter. In such cases, I measure the characteristics of the employer providing the majority of earnings in that quarter.

[10] Over 90% of individuals within this age range live with a parent in 2000. Children are individuals whose relationship to the household head is: son/daughter, adopted son/daughter or step son/daughter. I exclude individuals living in U.S. territories in 2000.

ple frame, approximately 21 million (57%) meet the two sets of restrictions. Based on these sample restrictions and the source of earnings data, my analysis should be viewed as representative of working families, a category which excludes very low income households (approximately the bottom 10% of households) and very high income households (approximately the top 1% of households). See Appendix B.2 for details.

2.1 Measuring Entry and Parental Earnings

My paper focuses on initial labor market outcomes and thus I need to define when individuals enter the labor market. Conceptually, I define entry as the first period in which work becomes the primary activity. My empirical definition of entry is the first quarter in which the individual earns at least $3,300 per quarter–which approximately corresponds to working 35 hours per week at the federal minimum wage–in the current and two consecutive quarters, and receives positive earnings from the same employer for those three quarters.[11] I refer to the employment spell at this employer as the first stable job. Approximately 80% of individuals (17 million individuals) that meet all the sample restrictions have entered the labor market by the end of 2016.

There are many possible ways to define entry, but three pieces of evidence suggest that my approach is reasonable.[12] First, individuals experience a dramatic and persistence increase in earnings after the quarter of entry. For example, average quarterly earnings in the three years prior to entry is $1,258 compared to $6,597 in the three years after entry. Figure A.1 provides more detailed evidence by plotting the average quarterly earnings in the three years before and after entry. Second, the age of entry generally lines up with common perceptions of when individuals start their careers. For example, 89% of children enter the labor market between ages 18 and 26. Figure A.3 depicts the distribution of the age at which the children enter the labor market and compares this distribution to results based on an analogous measure constructed from the National Longitudinal Survey of Youth 1997 cohort (NLSY97).[13] The timing of entry is quite similar in the two data

[11] Dollar values are converted to 2016 dollars using the Consumer Price Index for All Urban Consumers.

[12] Kramarz and Skans (2014) use a similar set of criteria to identify the first stable job.

[13] The analogous measure constructed from the NLSY97 is the first time an individual works at least 35 hours for 36 consecutive weeks (or three quarters). An alternative approach is to focus on labor market

sources. Furthermore, 83% of workers in the NLSY97 data are not enrolled in school at the time of labor market entry, which suggests that my measure is not primarily picking up jobs held by students. Third, the first stable job is indeed stable as the average duration of employment at the first stable job exceeds two years.

It is possible that working for a parent's employer could affect when and even whether an individual finds their first stable job. Figure A.2 presents age-earnings profiles between the ages of 17 and 30 for different groups of workers defined by when they enter the labor market. For workers that ever enter the labor market, annual earnings rise dramatically and persistently at the time of entry. For workers that never enter the labor market, earnings remain persistently low (average annual earnings is only $1,814 at age 30). Workers who never enter the labor market simply never participate in work in a meaningful way. Based on this observation, it seems unlikely that an individual would satisfy the earnings restriction for labor market entry only if they had the option to work for their parent's employer. A more likely possibility is that working at a parent's employer could affect the timing of entry. This does not affect interpretation of the analysis in Section 3, which simply aims to document descriptive patterns in the rate at which individuals work for the employers of their parents. This does however pose a potential challenge when estimating causal effects and I address the concern in more detail in Section 4.

Given the intergenerational focus, the measure of parental earnings plays an important role in the analysis. In the context of the intergenerational mobility literature, the goal is to construct a measure of lifetime earnings of the parents. Without data on the full labor market history, a common approach is to calculate parental earnings as the average earnings over a limited number of years. In addition to the measurement issues raised by Solon (1989) and Zimmerman (1992), unique features of the LEHD make this approach particularly problematic. The main issue is that there is no way to distinguish between zero earnings and missing data.[14] To account for this, I construct a measure of lifetime parental earnings by estimating a regression of quarterly earnings on an individual fixed

outcomes after all schooling is completed and I also present results for this definition of entry.

[14]Earnings data could be missing either because a state may not report to the LEHD in a given time period or because the job may not be covered in the LEHD frame.

effect and a third degree polynomial in age within cells defined by the interaction between state of residence in 2000, sex, and race.[15] The measure of the lifetime earnings of each individual parent is the imputed value of earnings between ages 35 and 55. For one-parent households, parental earnings is simply the lifetime earnings of the parent. For two-parent households, parental earnings is the average of the lifetime earnings of both parents. The parental earnings percentile ranks are calculated within each cohort of children using sample weights.[16] See Appendix B.4 for details.

3 Intergenerational Transmission of Employers

I begin the empirical analysis by documenting descriptive patterns related to the intergenerational transmission of employers. Table 1 presents summary statistics. The first column presents results for the entire sample. The second through fifth columns present results for subsamples defined by whether the first stable job is with the employer of neither parent, the secondary earner, both parents, or the primary earner, respectively.[17] The bottom row indicates that 7% of individuals work for the employer of either parent at their first stable job. A comparison across columns indicates that individuals who work for a parent's employer tend to stay at their first stable job longer, are less likely to be employed in the unskilled service sector, are more likely to work in the manufacturing/production sector, and earn slightly less.[18]

One interpretation is that parents directly influence the hiring or job search process. This would be consistent with Loury (2006), who finds that 10% of males found their current job through a parent, as well as with a more general body of evidence that finds ubiquitous use of informal search methods (Ioannides and Loury 2004; Topa 2011)

[15] The data are a panel measured at a quarterly frequency that include all strictly positive earnings records between 2000 and 2016 for the parents in the sample. Quarters with zero earnings are not included in the sample. I further restrict the panel to observations when the individuals are between the ages of 30 and 60 and drop individuals that have fewer than 4 quarters of strictly positive earnings over the entire time period. Parents not included in this sample are assumed to have zero lifetime earnings.

[16] Cohorts consist of individuals born between July 1st of year t and June 30th of year t+1.

[17] The primary earner is defined as the parent with the greatest earnings in the year prior to the quarter in which the child entered the labor market.

[18] I group two-digit North American Industry Classification System (NAICS) industry codes into three sectors: unskilled services, skilled services, and manufacturing/production. See Appendix B.5 for details.

Table 1: Summary Statistics

	Full Sample	First Job at the Employer of: Neither Parent	Secondary Earner	Both Parents	Primary Earner
A. Individual Characteristics					
male	0.50	0.49	0.46	0.61	0.60
White non-Hispanic	0.79	0.78	0.83	0.84	0.79
Black non-Hispanic	0.08	0.08	0.05	0.02	0.07
Asian non-Hispanic	0.02	0.02	0.02	0.04	0.02
Hispanic	0.09	0.09	0.09	0.08	0.09
born in United States	0.97	0.97	0.97	0.96	0.97
B. Household Characteristics					
parents are married	0.78	0.78	0.96	0.98	0.78
parent has unmarried partner	0.03	0.03	0.04	0.02	0.03
primary earner is male	0.57	0.56	0.77	0.80	0.54
parental earnings / 1,000	51.42	51.26	53.15	67.28	51.28
C. First Stable Job					
age at first job	20.94	21.00	20.10	19.81	20.08
tenure at first job (quarters)	10.07	9.77	13.40	18.03	13.67
log of quarterly earnings	8.74	8.74	8.62	8.70	8.72
skilled services	0.37	0.37	0.45	0.31	0.37
unskilled services	0.46	0.47	0.36	0.31	0.28
manufacturing/production	0.18	0.16	0.19	0.39	0.36
employer size $<$ 50	0.28	0.28	0.27	0.62	0.30
50$\leq$employer size$<$500	0.31	0.32	0.26	0.15	0.29
500$\leq$employer size	0.40	0.40	0.47	0.23	0.40
located in urban area	0.77	0.78	0.71	0.71	0.72
Sample Size					
proportion of full sample		0.93	0.02	0.01	0.04
observations	17,010,000	15,830,000	298,000	137,000	746,000

Notes: The table presents the average value of the variable defined in the row. Column 1 presents results for the full sample and columns 2-5 present results for the sample of children who, at their first stable job, worked for the employer of neither parent, the secondary earner, both parents, or the primary earner, respectively.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

and that labor market networks influence where individuals work (Bayer et al. 2008; Hellerstein et al. 2011; Schmutte 2015). However, some individuals may work for a parent's employer simply by chance.

The tendency for young workers to find a job at their parent's employer could reflect the fact that children and parents tend to live and work in the same local labor market. Table 1 indicates that individuals who work for a parent's employer are no more likely to work for large employers and over 70% of these individuals are located in urban areas. Together, this suggests that the tendency to work for a parent's employer is not driven by cases in which a single employer dominates a local labor market. To investigate the issue more rigorously, I calculate the proportion of individuals who work for an employer of the same size category and located in the same census tract as the employer of the primary earner.[19] The results, displayed in Panel A and column 2 of Table A.1, suggest that individuals are about 43 times more likely to work for the employer of their parent compared to another employer in the same census tract. Column 3 presents a similar statistic for an employer that is in the same commuting zone, size category, and industry and shows that individuals are about 70 times more likely to work for the employer of the primary earner.[20] These results suggest that geography, industry, and employer size are poor explanations for the intergenerational transmission of employers.

Alternatively, parents may pass on human capital that is particularly well-suited for a specific employer. To test this hypothesis I identify past employers (the employer of the primary earner when the child was 10 years old) and future employers (the employer of the primary earner in 2016). Separately for past and future employers, I limit the sample to cases where the past or future employer existed in the quarter in which the child entered the labor market but the current employer of the primary earner differed. Within these two samples, I find individuals are 6 and 4 times more likely to work for their parent's current employer relative to the past and future employers, respectively. If the transmission of employers were driven by the transmission of human capital, then we would expect these rates to be more similar. The fact that the child is more likely to work for a past or future employer of the parent relative to other employers in the same local labor market could be explained by the presence of other social contacts.

Taken together, the results suggest that the intergenerational transmission of employers is not driven by the tendency for children and parents to be similar in terms of characteristics such as human capital, preferences, or residential location. Rather, the evidence suggests that individuals work for their parent's employer primarily because parents directly influence the hiring or job search process.[21] For example, parents may reduce

[19]Employer size categories are: small (employees$<$ 50), medium (50 $\leq$employees$<$ 500), and large (500 $\leq$employees).

[20]Industry is defined as the three-digit NAICS industry code.

[21]It is also possible that non-monetary benefits could make it more likely for individuals to want to work for their parent's employer. Although, this explanation seems less likely, in light of the large earnings benefits found in the next section.

information asymmetries between the child and the firm. Alternatively, these patterns may simply reflect nepotism, by which the parent's employer grants favors regardless of merit. While distinguishing between the latter two explanations is difficult, there is some evidence that employer transmission tends to benefit children with more limited labor market opportunities. Table A.2 links responses to the American Community Survey to a subset of records and shows that, conditional on parental earnings, individuals with lower levels of educational attainment are more likely to work for a parent's employer. Table A.3 shows that, conditional on the age of entry, the transmission of employers is more likely to occur when unemployment is high.[22] Figures A.4 and A.5 illustrate that the industries in which employer transmission is more common tend to offer higher wages (conditional on observable worker characteristics) and exhibit higher rates of unionization. These results provide suggestive evidence that the benefits of employer transmission accrue to the worker (in the form of higher wages) as opposed to the employer.

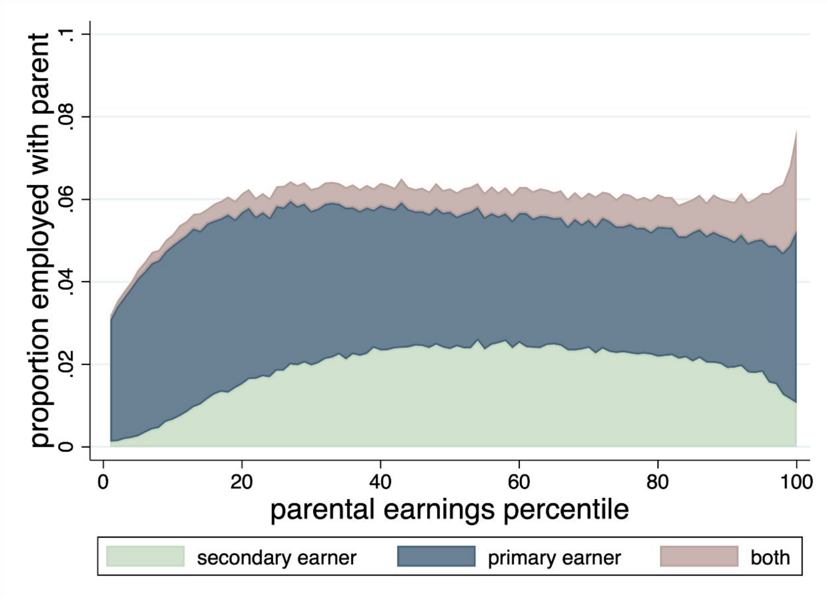
The rates of employer transmission differ across subgroups. To start, sons are more likely to work for the employer of a parent at their first stable job relative to daughters, with 7.8% of sons doing so compared to 6.0% of daughters. Both sons and daughters are more likely to work with the primary earner relative to the secondary earner, but the difference is larger for sons. Table A.4 presents rates of transmission by the sex of the parent and child and illustrates that individuals are at least twice as likely to work with the parent of the same sex. There is also substantial variation across the parental earnings distribution. Figure 1 plots the share of individuals working for the employer of a parent by the parental earnings percentile separately for sons and daughter. For both sons and daughters there is a strong positive association between transmission of employers and parental earnings in the bottom quintile and top decile of the parental earnings distribution and a weak (slightly negative for sons) relationship elsewhere.

A likely explanation for the relationship between parental earnings and the intergenerational transmission of employers is that higher-earning parents are more likely to be employed and hold a position of authority within their employer. The percent of primary

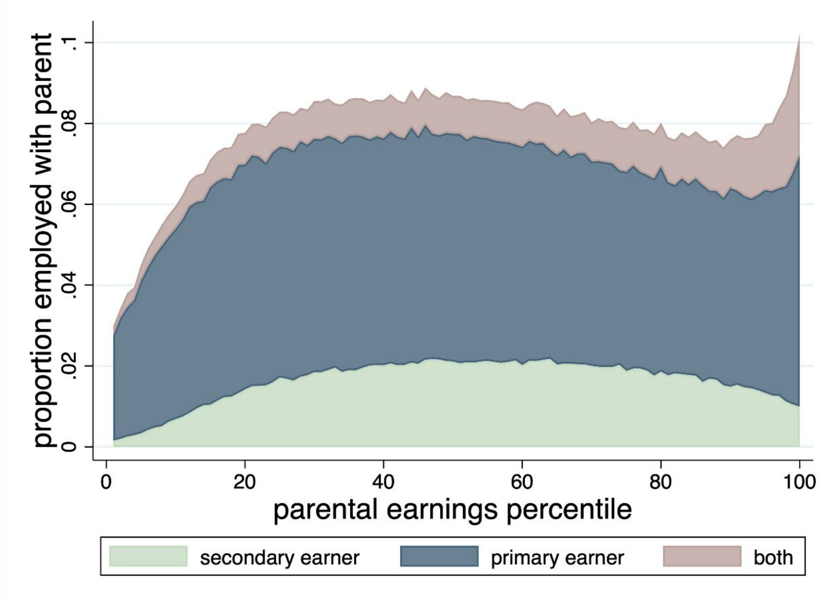
[22] I condition on the age of entry because older individuals are less likely to work for the employer of a parent and average age of entry is older later in the sample period (when unemployment is higher).

Figure 1: Intergenerational Transmission of Employers by Parental Earnings

(A) Daughters



(B) Sons

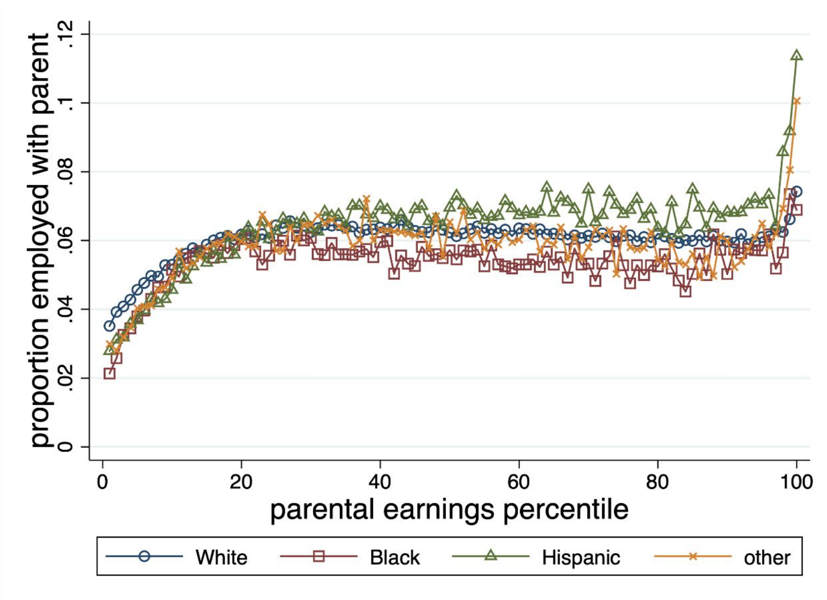


Notes: The figures plot the proportion of children whose first stable job is at the same employer as the secondary earner only, primary earner only, or both parents, respectively. Each statistic is reported separately by the percentile of the parental earnings distribution. Panels A and B present results for the sample of daughters and sons, respectively. All statistics are calculated using sample weights.

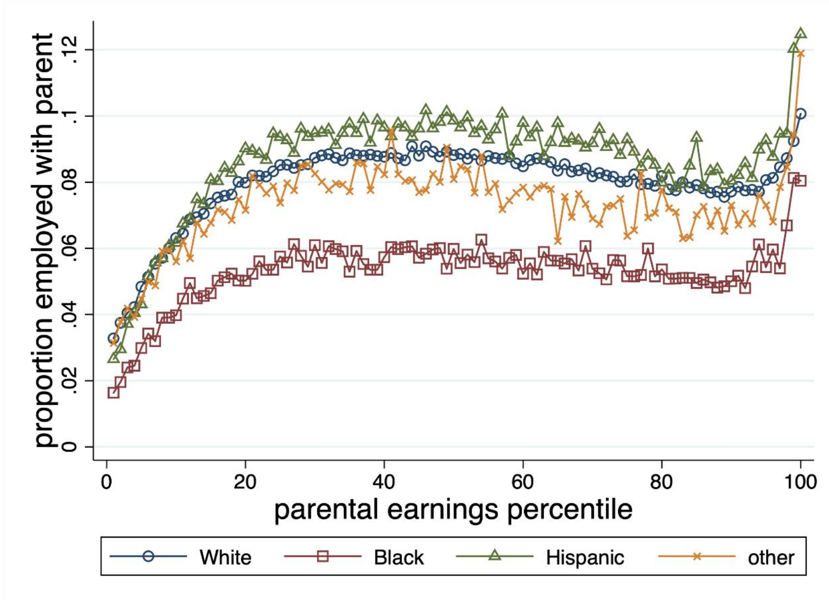
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Figure 2: Intergenerational Transmission of Employers by Parental Earnings and Race/Ethnicity

(A) Daughters



(B) Sons



Notes: The figures plot the proportion of children whose first stable job is at the same employer of either parent. Each statistic is reported separately by the percentile of the parental earnings distribution and by mutually exclusive and exhaustive categories of race/ethnicity. Panels A and B present results for the sample of daughters and sons, respectively. All statistics are calculated using sample weights.

Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

earners that are employed when their child enters the labor market rises steeply from 55% to 84% for parents in the bottom quintile of the parental earnings distribution and eventually plateaus at 94%. Parents who are top earners within their employer are likely to be in positions of authority and in a better position to get their child a job. The percent of primary earners that are top earners (top percentile) within their employer when the child enters the labor market rises gradually from 4% to 14% in the bottom nine deciles of the parental earnings distribution and then rises steeply from 14% to 41% in the top decile. Thus, the nonlinear relationship between the probability of working for a parent's employer and parental earnings closely tracks the probability that the parent is employed and is a top earner within their employer.[23]

Figure 2 presents the proportion of children working for the employer of either parent at their first stable job by parental earnings, sex, and race/ethnicity. For daughters, the patterns look fairly similar across the four race/ethnicity categories. In contrast, Black sons are substantially less likely to work for the employer of a parent relative to other groups throughout the parental earnings distribution. The average gap between Black and White sons conditional on parental earnings is 2.7 percentage points.

Lastly, the nonlinear relationship between the intergenerational transmission employers and parental earnings is also present in longer-run measures. Within the sample of children who turn 30 by the end of 2016, 28% of daughters and 29% of sons work for the employer of a parent between the ages of 18 and 30. These estimates are consistent with Stinson and Wignall (2018), who find that 22% of sons have shared an employer with their father by the time they are 30 years old.[24] Figure A.8 presents how these estimates vary across the parental earnings distribution and illustrates that the nonlinear patterns observed at the first stable job are replicated in these longer-run measures.

[23]Figure A.6 presents these results in detail by plotting the proportion of parents that are employed and top earners within their employer against the percentile of parental earnings. Furthermore, Figure A.7 examines this point by regressing an indicator for working for the primary earner's employer against the percentile of parental earnings. I then sequentially add groups of covariates that control for (1) demographic characteristics of the individuals and households, (2) labor force participation by the primary earner, (3) tenure and earnings rank within the employer of the primary earner, and (4) characteristics of the primary earner's employer. Together, the controls for employment and earnings rank within the employer explain much of the differences across the parental earnings distribution.

[24]Similar estimates for other countries include 40% in Canada (Corak and Piraino 2011) and 28% in Denmark (Bingley et al. 2011).

4 Earnings Consequences

This section estimates the earnings consequences of working for a parent's employer. I begin by considering a structural earnings equation in order to define the treatment effect of interest, highlight potential mechanisms, and illustrate the challenges associated with estimating causal parameters. Let the log earnings at the first stable job (y_{ijt}) be additive in an individual component (α_i), an employer component (ψ_j), an individual-employer component (ζ_{ij}), and an error term (ϵ_{it}), where i denotes the individual, t denotes the quarter in which they begin their first stable job, and j denotes the employer. Working at a parent's employer affects where an individual works and thus may affect earnings through ψ_j or ζ_{ij}. Using notation from the potential outcomes framework, let $j(1)$ denote parent's employer and let $j(0)$ denote the employer that is the next best option. Thus,

$$y_{ijt} = D_{it}\big[\underbrace{\beta_{it}^{\psi} + \beta_{it}^{\zeta}}_{\text{treatment effect}}\big] + \big[\alpha_i + \psi_{j(0)} + \zeta_{ij(0)} + \epsilon_{it}\big] \tag{1}$$

where D_{it} is an indicator equal to one if the first stable job is at the parent's employer. The treatment effect of working for a parent's employer consists of an employer component ($\beta_{it}^{\psi} = \psi_{j(1)} - \psi_{j(0)}$) and an individual-employer component ($\beta_{it}^{\zeta} = \zeta_{ij(1)} - \zeta_{ij(0)}$).[25]

In equation 1 the term, $\beta_{it}^{\psi} + \beta_{it}^{\zeta}$, highlights two potential mechanisms through which working for the parent's employer could affect earnings. β_{it}^{ψ} illustrates that if pay policies vary across employers, working for a parent's employer could affect earnings by simply affecting where the individual is employed. This mechanism is consistent with the model of labor market networks developed in Mortensen and Vishwanath (1995) as well as models which show how imperfect competition in the labor market leads to dispersion in employer-level pay policies. β_{it}^{ζ} illustrates that employers might offer different wages to children of current employees relative to otherwise similar workers. This could happen if parents reduce information asymmetries between workers and employers (e.g., Montgomery 1991; Dustmann et al. 2016) or if working with a parent affects worker

[25]The treatment effect has a time subscript because the employer an individual matches to could depend on when they enter the labor market.

productivity (e.g., Heath 2018). The key difference between these two explanations is whether the earnings benefits are common to all workers at the employer (as in the former explanation) or specific to the presence of an idiosyncratic parent-child relationship (as in the latter explanation). I return to this distinction when investigating mechanisms.

Equation 1 also highlights the empirical challenges associated with estimating causal parameters. In the previous section I found that individuals were more likely to work at a parent's employer if they were less educated–this could be modeled as a negative correlation between α_i and D_{it}–and if they were searching for a job in labor markets with higher levels of unemployment–this could be modeled as a negative correlation between $\psi_{j(0)}$ or $\zeta_{ij(0)}$ and D_{it}. These patterns suggest that a naive comparison between individuals who do and do not work for their parent's employer would understate the earnings benefits. More generally, an empirical strategy that identifies causal parameters must account for the possibility that the characteristics and outside options of individuals are related to the probability that they take a job at their parent's employer.

4.1 Instrumental Variables Strategy

I use an instrumental variables strategy that exploits exogenous variation in the availability of jobs at the parent's employer. In order to explain the empirical strategy, consider estimating the following equation via two-stage least squares,[26]

$$\begin{aligned} D_{it} &= \tilde{\pi}^1 + \gamma Z_{j(1)t-1} + \tilde{u}_{it} \\ y_{ijt} &= \tilde{\pi}^2 + \beta_{it} D_{it} + \tilde{v}_{it} \end{aligned} \tag{2}$$

where $Z_{j(1)t-1}$ is the average quarterly hiring rate at the parent's employer in the four quarters prior to the quarter in which the child begins their fist stable job.[27] Intuitively, the parent's employer will be more likely to make a job offer to the child of a current

[26]The precise relationship between the structural earnings equation, presented in equation 1, and the empirical equation, presented in equation 2, is as follows: $\beta_{it} = \beta^{\psi}_{it} + \beta^{\zeta}_{it}$, $\tilde{\pi}^2 = \alpha_i + \psi_{j(0)} + \zeta_{ij(0)} + \epsilon_{it}$ and $\tilde{v}_{it} = [\alpha_i + \psi_{j(0)} + \zeta_{ij(0)} + \epsilon_{it}] - \tilde{\pi}^2$.

[27]I follow the methodology used to produce the Quarterly Workforce Indicators and calculate the End-of-Quarter Hiring Rate, which is the number of new hires that remain with the employer for at least one additional quarter divided by the average of the total employment at the employer at the beginning and end of the quarter.

employee when they are hiring more intensively. The choice to use the average hiring rate in the preceding four quarters has the dual advantage not being affected by the actions of the child or seasonal variation.

The stylized model highlights two main reasons why the independence assumption–which is a key assumption needed to interpret the estimates as causal–is unlikely to hold.[28] First, the hiring rate at the parent's employer could be correlated with local labor market conditions that directly effect the earnings of the child–this could be modeled as a positive correlation between $\psi_{j(0)}$ or $\zeta_{ij(0)}$ and $Z_{j(1)t-1}$. Second, employers that hire more intensively may tend to employ more highly educated workers who have more highly educated children–this could be modeled as a positive correlation between α_i and $Z_{j(1)t-1}$.

I include covariates in the empirical model to address the concern that the hiring rate at the parent's employer could be related to time-varying local labor market conditions as well as time-invariant characteristics of the parent's employer. Specifically, I estimate the following equation via two-stage least squares,[29]

$$\begin{aligned} D_{it} &= \pi^1 + \gamma Z_{j(1)t-1} + X_{it}\Gamma^1 + \psi^1_{j(1)} + \phi^1_{l(j(1),t)} + u_{it} \\ y_{ijt} &= \pi^2 + \beta_{it} D_{it} + X_{it}\Gamma^2 + \psi^2_{j(1)} + \phi^2_{l(j(1),t)} + v_{it} \end{aligned} \tag{3}$$

where $\psi_{j(1)}$ is a fixed effect for the parent's employer; $\phi_{l(j(1),t)}$ is a fixed effect for the local labor market in which the parent's employer is located, which is defined by the interaction between the state, industry (two-digit NAICS code) and calendar year; X_{it} is a vector of demographic characteristics; and u_{it} and v_{it} are regression residuals, which are clustered at the level of the parent's employer.[30]

I implement two sample selection criteria when estimating the specification. First, since I exploit variation in the hiring rate at the parents' employer, I require that the

[28]The independence assumption requires that $\{\alpha_i, \psi_{j(1)}, \psi_{j(0)}, \zeta_{ij(1)}, \zeta_{ij(0)}, \epsilon_{it}\} \perp\!\!\!\perp Z_{j(1)t-1}$.

[29]I estimate all regressions without sample weights since the empirical strategy explicitly accounts for the reasons weights should be used when estimating causal effects (Solon et al. 2015). In practice, I find that the using sample weights makes little difference for the results.

[30]The vector of demographic characteristics includes: the log of the annual earnings of the parent in the year prior to entry; a fixed effect for the cohort of the child; and an interaction between the sex of the child and their race, ethnicity, and an indicator equal to one if born in the United States. The race categories include White, Black, Native American, Asian, Pacific Islander, and other. Ethnicity is defined as Hispanic and non-Hispanic.

parent is employed at the time the child enters the labor market. For much of the analysis I focus on estimating the effect of working for the employer of the parent who is the primary earner and require that the primary earner has at least one year of tenure in the quarter in which the child enters the labor market. The tenure restriction helps address concerns that children and parents might be responding to common economic shocks affecting firms in the local labor market. Second, I drop all singleton observations because these observations do not contribute to the identification of any parameters in the model and retaining them would bias estimates of the standard errors.[31]

The estimates from equation 3 have a causal interpretation under three assumptions. First, the hiring rate must affect the probability of working for a parent's employer. This assumption is testable and I present the relevant empirical evidence in Section 4.2. Second, the hiring rate must have a monotonic affect on the probability of working for a parent's employer. With the two sets of fixed effects in the model, this assumption implies that for any two employers and any two periods, the employer that experiences a larger increase in the hiring rate also experiences a larger increase in the propensity to hire a child of a current employee.[32] While not directly testable, Section 4.3 presents some empirical evidence to support the plausibility of this assumption.

Third, the independence assumption requires that the hiring rate is only related to the earnings of the individual through the effect on working at the parent's employer.[33] The covariates directly address two main concerns. First, the state-by-industry-by-year fixed effects address the possibility that the hiring rate at the parent's employer might be

[31] A singleton refers to an observation which has a unique value of a fixed effect. For example, if there only existed one observation for a given parent's employer, then the outcome would be perfectly predicted by the employer fixed effect and this observation would not contribute to the identification of any other coefficients.

[32] The hiring rate may be correlated with the composition of new hires if some types of workers are relatively more likely to be hired than others when the employer is hiring more intensively. However, this is not a violation of the monotonicity assumption as long as the absolute probability–as opposed to the probability relative to other workers–of a given worker being hired is weakly increasing in the hiring rate. Consider the following example. The parent's employer only makes job offers to the high ability individuals when hiring is relatively low. The parent's employer makes job offers to both high and low ability individuals when hiring is relatively high. While this affects the interpretation of the estimates (the estimates identify the average effect for low ability individuals in this case), it does not necessarily affect the validity of the instrument. I make this point formally in the context of the stylized model presented in Appendix D.

[33] Independence requires that $\{\alpha_i, \psi_{j(1)}, \psi_{j(0)}, \zeta_{ij(1)}, \zeta_{ij(0)}, \epsilon_{it}\} \perp\!\!\!\perp Z_{j(1)t-1} \mid \{\psi_{j(1)}, \phi_{l(j(1),t)}\}$.

correlated with local labor market conditions. Second, the fixed effects for the parent's employer address the concern that the hiring rate may be correlated with time-invariant characteristics of the employer that are correlated with the characteristics of the parents and their children. The vector of demographic variables accounts for additional individual-level heterogeneity not captured by the employer fixed effect; although, the demographic controls do not play a major role in identification.[34] In Section 4.3 I present evidence to suggest that the covariates achieve their stated objective and I also explore other possible violations of the independence assumption.

With two-way fixed effects, the identification strategy bears some similarities to a difference-in-differences estimator.[35] Intuitively, the first-stage compares individuals whose parents work for the same employer but who enter the labor market at different times. I ask if the individual is more likely to work with their parent if they enter the labor market when their parent's employer is hiring more intensively, and whether this difference is larger relative to individuals who enter the same local labor market in the same periods but whose parent's employer experiences a relatively smaller growth in the hiring rate. In this way, the empirical strategy exploits variation in the hiring rate that is orthogonal to both time-invariant characteristics of the parent's employers and time-varying conditions of the local labor market.

If the three identifying assumptions are met, the two-stage least squares estimator identifies a *local average treatment effect* (LATE), which is the average effect for the *compliers*–the population whose treatment status depends on the value of the instrument (Imbens and Angrist 1994). I first focus on understanding the consequences of working for a parent's employer for this population. After presenting the main results, Section 4.5 explores the relationship between the LATE and other causal parameters of interest.

[34]The main estimates are qualitatively similar when including no demographic controls.

[35]Goodman-Bacon (2019) shows that differences-in-differences estimators will be biased when treatment effects evolve over time. This is not a concern in my setting, as I focus on the initial outcomes of the young workers and do not use information on how their outcomes evolve before and after working for the employer of a parent.

4.2 Estimates of the Effect on Initial Earnings

Table 2 presents estimates from equation 3 of the earnings consequences of working for the employer of a parent (the primary earner) at the first stable job. Column 1 presents the estimates from the first-stage and demonstrates that the hiring rate at the parent's employer is highly predictive of whether or not the child works there, with an associated F-statistic of 1,434.[36] Column 2 presents the reduced form estimates, illustrating that there is a positive and statistically significant relationship between the hiring rate and initial earnings, which are measured during the first full-quarter of employment at the first stable job.[37] Column 4 presents the second stage estimates, which indicate that working for a parent's employer leads to a 31% increase in initial earnings. Column 3 presents Ordinary Least Squares (OLS) estimates for comparison, which are positive but significantly smaller than the two-stage least squares estimates. The OLS estimates could be negatively biased if, for example, low-ability children with limited labor market opportunities are most likely to accept job offers from their parents' employers. It is plausible that the OLS estimates would suffer severely from bias since the data lack meaningful measures of human capital.

The estimated earnings benefits of working for the employer of a parent are large but not inconsistent with other evidence of the importance of place of work in determining earnings. For example, the estimated effect is about twice as large as the union wage premium (Farber et al. 2018) and about two standard deviations of the inter-industry wage premium (Katz and Summers 1989). Another way to assess the magnitude of my estimates is to compare them to the college premium–the relative wage of college versus high school educated workers–which is about 68 log points (Acemoglu and Autor 2011). In the context of the United States, Stinson and Wignall (2018) estimate specifications

[36]To assess whether the first stage is also economically significant, I estimate placebo regressions in which I replace all variables related to the employer of the parent with variables that correspond to the placebo employers considered in Section 3, including employers in the same census tract or local labor market and past or future employers. Both the point estimates and F-statistics associated with the true employers are an order of magnitude larger (see Panel B of Table A.1).

[37]A full-quarter employment spell occurs when a worker receives strictly positive earnings from the same employer in the current, previous and subsequent quarter and variation in earnings is less likely to be driven by differences in the duration of an employment spell within a quarter. The definition of the first stable job implies that every worker experiences a full-quarter employment spell in the second quarter at their first stable job.

Table 2: Effect on Initial Earnings

	parent's employer	log of quarterly earnings		
	(1)	(2)	(3)	(4)
hiring rate	0.119***	0.036***		
	(0.003)	(0.003)		
work for parent's employer			0.032***	0.307***
			(0.002)	(0.029)
estimator	OLS	OLS	OLS	2SLS
F-statistic	1,434			
mean	0.056			
control mean		8.737	8.737	8.737
control s.d.		0.427	0.427	0.427
observations	11,460,000	11,460,000	11,460,000	11,460,000

Notes: Each column presents results from a separate regression. The outcome variable in column 1 is an indicator equal to one if the individual works for their parent's employer (primary earner) at the first stable job. The outcome variable in columns 2-4 is the log of the first full-quarter earnings at the first stable job. The main independent variable in column 1 is the average quarterly hiring rate at the parent's employer and the main independent variable in columns 2-4 is an indicator equal to one if the individual works for their parent's employer. The results in columns 1-3 are estimated by Ordinary Least Squares (OLS) and the results in column 4 are estimated by two-stage least squares (2SLS), where the instrument is the average quarterly hiring rate at the parent's employer in the four quarters prior to entry. All specifications include a fixed effect for the parent's employer; a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and the standard vector of demographic characteristics. Standard errors are clustered at the level of parent's employer and are presented in parentheses.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.
*** p≤0.001, ** p≤0.01, * p≤0.05

with individual fixed effects and find that sons and daughters who work for the employer of their father experience an increase in earnings by 22% and 8%, respectively. My results differ more dramatically relative to Kramarz and Skans (2014), who study the school-to-work transition in Sweden and find small wage losses in the short run, which appear to be offset by stronger wage growth in the medium run; this finding is supported by Eliason et al. (2019), who use more recent data from Sweden.

4.3 Validity of the Empirical Strategy

One potential issue is that employers might offer higher wages when hiring more intensively. I assess this concern by controlling for the log of average earnings of all new hires at the parent's employer in the preceding year. This only reduces the main estimates from 0.307 to 0.299 (see column 2 of Table A.5). However, changes in the earnings of new hires might partially reflect a change in the composition of workers being hired. Columns 3 and 4 of Table A.5 take an alternative approach and control for the earnings growth of the parents and all workers at the employer, respectively, in the year prior to entry. The idea is that changes in offer wages are likely to be correlated with earnings growth for current workers. Again, the estimated earnings benefits are largely unaffected. Lastly, column 5 of Table A.5 shows that the results are also robust to controlling for the growth in employment in the year prior to entry (point estimate is 0.307). In general hiring and employment growth are positively correlated, but, conditional on the covariates in the model, the hiring rate captures variation in job opportunities that is orthogonal to more general measures of firm health. Thus, it is unlikely that within-employer intertemporal variation in offer wages is driving the results. This find is consistent with Lachowska et al. (2019) who find that employer pay premiums are highly persistent.

The empirical specification might not adequately control for changes in local labor market conditions. I investigate this by estimating the reduced form regression for the placebo employers drawn from the same census tract or local labor market. In these placebo specifications I replace all variables related to the employer of the parent with variables related to the placebo employer. Columns 2 and 3 of Panel C in Table A.1 indicate that, conditional on the covariates, the hiring rate at the placebo employers is unrelated to the initial earnings of the child.[38] The point estimates (standard errors) for the placebo reduced form specification are -0.0016 (0.0015) and 0.0018 (0.0015) compared to 0.0364 (0.0033) for the main specification. Thus, the positive relationship between initial earnings and the instrument is unlikely to be driven by local labor market conditions.

Local labor market conditions could also lead to a violation of the monotonicity assumption. If there tends to be more job opportunities at all firms when the parent's employer is hiring, an increase in the hiring rate at the parent's employer could actually

[38]Columns 5 and 7 of Table A.1 indicate that the reduced form is positive for past and future employers. The magnitudes are substantially smaller than those associated with the true employer of the parent (see columns 4 and 6) and only the estimates for future employers are statistically significant. I do not view these results as problematic since it is possible that young workers have access to these employers through other connections. This interpretation is consistent with the results from Panels A and B.

accompany a reduction in the probability that the individual works there. I measure the aggregate hiring rate in the three sectors–unskilled services, skilled services, and manufacturing/production–in the commuting zone in which the parent's employer is located and include a vector of controls that interacts these aggregate hiring rates with the sector of the parent's employer. This modified specification directly controls for hiring conditions at all employers in the local labor market. The point estimate (standard error) from the first and second stage are 0.118 (0.003) and 0.297 (0.029), respectively. Thus, these controls have little impact on the results, which provides additional evidence that local labor market conditions are not biasing the estimates.

Where the parent works is not randomly assigned, which raises two concerns. First, the sample excludes parents that are not employed and some parents may lose their jobs when the hiring rate is lower and the employer is not doing well. It is likely that this would produce negative bias, since lower-earning parents would be be more likely to appear at employers with high hiring rates. Second, parents may anticipate that their child will struggle to find a job and move to employers that have more job opportunities when their child is starting their career. If parents are more likely to do this for children with lower earnings potential, then this would also lead to negative bias. The sample selection criteria requiring parents to have at least one year of tenure likely helps to address these concerns, as the concerns are more applicable to parents that are less attached to their employer. To further assess this point I estimate the main specification on a sample of parents with at least five years of tenure and continue to find large positive earnings benefits for this sample with a point estimate (standard error) of 0.23 (0.048).

I use comparisons between siblings to further investigate potential issues that could arise from parents sorting into employers. Specifically, I estimate one specification that includes a fixed effect for the parent's employer and another that includes a fixed effect for the parent's employer by household, which limits the identifying variation to comparisons between siblings. Both regressions are estimated on the same subsample, which retains cases for which at least two siblings entered the labor market when the primary earner was at the same employer. The estimates (standard errors) from the specification with the employer fixed effect and the household by employer fixed effect are 0.199 (0.040) and 0.155 (0.045), respectively (see Table A.6). The two estimates are qualitatively and quantitatively similar, which suggests that the results are not driven by unobserved differences across households. These estimates are smaller than the main estimates but this does not necessarily indicate any issues related to the validity of the empirical strategy.

The hiring rate at the parent's employer could be related to earnings through some other channel. First, the option to work for the parent's employer might raise individuals' reservation wages, leading them to match with better employers even if they do not end up working with their parent. Second, if the hiring rate is correlated with other measures of parental financial well-being, individuals might stay in school longer absent financial constraints. Both mechanisms ought to delay entry into the labor market; however, the results presented in Table A.7 indicate that employer transmission actually leads to earlier entry. In particular, children find their first stable job almost a year earlier and are slightly less likely to be employed in the three years prior to entry, which might indicate a smoother transition between school and work. Thus, there is no evidence that the earnings gains are driven by an increase in educational attainment or in the time spent searching for a job. This is not surprising in light of evidence from Hilger (2016) and Fradkin et al. (2018) who find that parental job loss during adolescence does not meaningfully impact educational attainment or job quality through extended search.

It is potentially problematic that working for a parent's employer affects the timing of entry. There are two stories for why the hiring rate at the parent's employer could affect the timing of entry. First, if there are job opportunities in the current period, the individual may start their career earlier if they anticipate not being able to find a better option in future periods. Second, if the parent's employer is not hiring when the individual decides to start looking for work, they may not find their first stable job until the parent's employer is hiring at later date. Both stories are more relevant for individuals who have more limited labor market options. This would then likely bias the estimates downward because individuals with low-earnings potential would be disproportionately likely to work for a parent's employer when the hiring rate is high. My main empirical specification measures the hiring rate at the parent's employer in the four quarters prior to when the individual enters the labor market. I assess the sensitivity of the estimates to shifting this window of measurement four quarters earlier and four quarters later. Shifting this window of measurement backwards one quarter or forwards three quarters yields qualitatively similar results with point estimates (standard errors) that range from 0.25 (0.013) to 0.42 (0.054). Outside of this range the point estimates grow larger (point estimates between 0.49 and 0.72), but first stage grows weaker.[39] See Table A.8 for all estimates. Taken together, these results suggest that is unlikely that issues related to timing of entry are driving the positive earnings benefits.

4.4 Mechanisms and Other Results

One possible channel through which working for a parent's employer could affect earnings is by matching individuals to firms that offer higher pay to all workers. I investigate this in column 1 of Table 3, where the outcome is the employer-level pay premium estimated via a model with worker and employer fixed effects as in Abowd et al. (1999); hereafter referred to as AKM.[40] Working for the parent's employer increases the employer pay premium by 0.304 (the employer pay premium is measured in log quarterly earnings). A comparison to the main results in Table 2 reveals that virtually the entire impact on individual earnings is explained by an improvement in the employer pay premium.[41]

A wide class of models predict that imperfect competition, which could arise from search frictions (Burdett and Mortensen 1998; Postel-Vinay and Robin 2002) or heterogeneous preferences over a firm's non-wage characteristics (Card et al. 2018), can lead to job ladders, where more productive firms poach workers from less productive firms by offering higher wages. Consistent with this class of models, columns 2-4 of Table 3 illustrate that working for the employer of a parent leads individuals to work at employers that are more productive (measured by revenue per worker), that are more likely poach

[39]The F-statistic from the first stage falls to 177 when the hiring is measured between eight and four quarters prior to the quarter of entry.

[40]See Appendix B.6 for details on how the employer pay premium is estimated.

[41]I estimate a specification where the outcome variable is individual log earnings minus the employer pay premium and the estimated effect falls to 0.004 with a standard error of 0.03. This provides additional evidence that the earnings benefits are driven by access to higher-paying employers.

Table 3: Effect on Employer Characteristics

		National Rank					Sector		
	employer pay premium (1)	revenue per worker (2)	poaching hires (3)	average earnings (4)	log firm size (5)	firm age in years (6)	unskilled services (7)	skilled services (8)	manufacturing/ production (9)
works for parent's employer	0.304***	4.767*	9.217***	25.060***	-1.979***	-3.040***	-0.433***	0.062*	0.372***
	(0.024)	(2.118)	(1.638)	(1.741)	(0.244)	(0.866)	(0.036)	(0.031)	(0.028)
control mean	0.359	51.75	55.33	42.47	6.72	22.78	0.375	0.460	0.165
control s.d.	0.366	27.52	23.38	26.53	3.40	12.28	0.484	0.498	0.371
observations	11,460,000	9,391,000	11,460,000	11,460,000	11,460,000	11,460,000	11,460,000	11,460,000	11,460,000

Notes: Each column presents estimates from a separate regression estimated by two-stage least squares. All outcomes measure characteristics of the employer of the individual at the first stable job. The outcome in column 9 is the employer-level pay premium estimated via a model with worker and employer fixed effects. The outcomes in columns 2-4 correspond to the rank of the employer based on time-invariant measures of log revenue per worker, the proportion of hires made through poaching workers from other employers and the average log earnings of all workers at the employer, respectively. The outcomes in column 5 and 6 are log firm size and firm age, respectively. The outcome in columns 7-9 are indicator variables equal to one if the employer of the child is in the unskilled service sector, skilled service sector, or the manufacturing/production sector, respectively. The endogenous variable is an indicator equal to one if the individual works for their parent's employer (primary earner) at the first stable job, which is instrumented for using the average quarterly hiring rate at the parent's employer in the four quarters prior to entry. All specifications include a fixed effect for the parent's employer, a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer, and the standard vector of demographic characteristics. Missing data in the outcome varaibles in columns 3 and 4 are imputed with the mean of the control group and the specifications include an indicator equal to one if the outcome is imputed. Standard errors are clustered at the level of parent's employer and are presented in parentheses.

Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

workers from other employers when hiring and whose employees are paid more on average.[42] These results suggest that employer transmission increases earnings by allowing individuals to start their careers on a higher rung of the job ladder. Column 4 suggests that individuals who work for their parent's employer end up at smaller firms. While job ladder models typically predict that larger firms will occupy higher rungs of the job ladder, Haltiwanger et al. (2018) argue that firm age complicates this prediction because there are productive young firms that have not had ample time to grow into large firms. Consistent with this explanation, column 6 indicates that employer transmission leads individuals to work for younger firms.

A number of papers find a systematic relationship between individual earnings and the identity of the employer. For example, workers tend to experience earnings growth when they move up the firm job ladder defined by productivity (Haltiwanger et al. 2017), poaching flows (Bagger and Lentz 2019) and average pay (Haltiwanger et al. 2018). Furthermore, evidence from the AKM empirical model suggests that different workers who move between the same employers experience similar changes in earnings. One interpretation of this evidence is that some firms pay higher wages than others. However, this interpretation is complicated by the fact that worker mobility is endogenous: workers on an upwards (or downwards) career trajectory, might tend to move to certain firms.

My empirical strategy isolates exogenous variation in where individuals are employed and thus I provide more direct evidence that the firm-level pay policies are an important determinant of earnings. My results indicate that, for the complier population, the parent's employer occupies a higher rung of the job ladder than the employer that is the next best option and individuals earn more at their parent's employer. The fact that the estimated effect on individual earnings is virtually identical to the estimated effect on the employer pay premium is most easily explained by the following three statements being true: (1) my instrumental variables estimator identifies a causal parameter, (2) the AKM empirical model identifies an employer pay premium and (3) the earnings

[42] The outcomes in columns 2-4 correspond to the rank of time-invariant characteristics of the first stable employer relative to the national distribution of employers. See Appendix B.7 for a description of how the employer- and firm-level variables are constructed.

benefits of working for a parent's employer are driven by parents providing access to higher paying employers. In this way, my results offer novel support of the plausibility of the assumptions underlying the AKM empirical model. Importantly, the identifying assumptions imposed by AKM are entirely distinct from the assumptions required to interpret my two-stage least squares estimates as causal.[43]

Part of the effect on the employer pay premium is explained by parents providing access to employers in higher-paying industries. Columns 7-9 of Table 3 present estimates in which the outcome is an indicator equal to one if the child works in one of three broad sectors. Working for a parent's employer reduces the probability of working in the unskilled service sector by 43 percentage points and increases the probability of working in the manufacturing/production sector by 37 percentage points. The effect on the industry of employment has large predicted earnings consequences. Table A.9 presents estimates in which the outcome variable is the industry-level earnings premium (estimated analogously to the employer-level pay premium). Working for a parent's employer increases the two- and six-digit industry pay premium by 0.167 and 0.230, respectively. Thus, 75% of of the effect on individual earnings is explained by individuals working in different six-digit industries. To the extent that young workers are aware of pay differences across industries, these results cast doubt on the possibility that parents simply provide information to their children about where to look for high-paying jobs.

The effect of working for a parent's employer on subsequent job mobility provides further support for the hypothesis that the earnings gains are driven by employer-level pay policies. Column 1 of Table 4 indicates that working for a parent's employer increases the probability of remaining at the first employer for at least three years by 17.4 percentage points. Columns 2 and 3 illustrate that this effect is entirely driven by a reduction in the probability of making a job-to-job transition. These results are consistent with predictions from a job ladder model: individuals who do not work for their parent's employer start on

[43]Using the notation from equation 1, AKM assumes that $\{\alpha_i, \psi_j\} \perp\!\!\!\perp \{\epsilon_{it}, \zeta_{ij}\}$. In contrast, the independence assumption requires that, $\{\alpha_i, \psi_{j(1)}, \psi_{j(0)}, \zeta_{ij(1)}, \zeta_{ij(0)}, \epsilon_{it}\} \perp\!\!\!\perp Z_{j(1)t-1} \mid \{X_{it}, \psi_{j(1)}, \phi_{l(j(1),t)}\}$. Importantly, AKM makes assumption about the relationship between unobserved error terms $(\epsilon_{it}, \zeta_{ij})$ and the individual- and employer-level components of earnings (α_i, ψ_j), whereas my empirical strategy makes no assumptions about the relationship between these variables.

Table 4: Effect on Post Entry Earnings and Job Mobility

	First Move in 3 Years			Annual Earnings After Entry		
	stay	j2j	j2n	year one	year two	year three
	(1)	(2)	(3)	(4)	(5)	(6)
works for parent's employer	0.174***	-0.182***	0.008	7,363***	7,226***	4,790***
	(0.034)	(0.033)	(0.038)	(1,002)	(1,284)	(1,440)
control mean	0.286	0.279	0.435	26,460	25,660	26,740
control s.d.	0.452	0.449	0.496	15,660	19,290	21,130
observations	10,200,000	10,200,000	10,200,000	10,200,000	10,200,000	10,200,000

Notes: Each column presents estimates from a separate regression estimated by two-stage least squares. The outcome in column 1 is an indicator variable equal to one if the child stayed at their first employer for three years. The outcomes in columns 2 and 3 are indicator variables equal to one if the child left their first employer within three years and made a job-to-job (j2j) or a job-to-nonemployment (j2n) transition for their first move, respectively. Note that the outcome variables in columns 1-3 are mutually exclusive and exhaustive. The outcome varaibles in columns 4-6 are the annual earnings one, two and three years after the quarter of entry, respectively. The endogenous variable is an indicator equal to one if the individual works for their parent's employer (primary earner) at the first stable job, which is instrumented for using the average quarterly hiring rate at the parent's employer in the four quarters prior to entry. The sample includes individuals who found their first stable job prior in 2014:Q1 or earlier. The F-statistic from the first stage for this sample is 1,532. All specifications include a fixed effect for the parent's employer, a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer, and the standard vector of demographic characteristics. Standard errors are clustered at the level of parent's employer and are presented in parentheses.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

a lower run of the job ladder and are thus more likely to be poached (make a job-to-job transition) by an employer on a higher rung of the ladder. Furthermore, if the outcomes in columns 2 and 3 are viewed as proxies for quits and fires, respectively, then these results provide some indication that the benefits of employer transmission accrue to the children as opposed to the employers: working for a parent's employer allows individuals to gain access to employers that pay more than their outside option and so they choose to remain at those employers, whereas the employers are not gaining access to better workers and so they are no less likely to fire these workers.[44]

Columns 4-6 of Table 4 illustrate that the earnings benefits of working for a parent's employer are quite persistent. Working for the parent's employer leads to an increase of $7,363 in the first year after entry into the labor market. The effects are persistent but by the third year the magnitude of the effect falls to $4,790. Figure A.9 presents estimates of the effect on annual earnings one to six years after entry for a group of individuals for whom I am able to observe these outcomes. There is less statistical precision in the later years but the point estimates suggest that the earnings benefits are quite persistent, with annual earnings benefits that exceed $5,000 even six years after entry.

I investigate heterogeneous effects by estimating the main specification on different subgroups of workers defined by the quintile of parental earnings and the sex of the individual. The results are presented in Table 5. In the full sample the point estimates for daughters (0.424) is larger than for sons (0.312). While earnings benefits for daughters are also larger within each parental earnings quintile, large standard errors prevent me from concluding whether or not there are meaningful differences between the earnings effects by sex. A comparison of estimates across columns 1-5 in Panel A indicates that children with parents higher up in the parental earnings distribution experience greater earnings benefits from working for a parent's employer. For example, the estimated effect for individuals whose parents are in the fifth quintile (highest earnings) is 0.328 compared to 0.189 for individuals whose parents are in the first quintile (lowest earnings). The estimates in Panels B and C illustrate that the positive association between the effects

[44]Fallick et al. (2019) find a strong association between transitions into nonemployment and earnings losses, which lends credibility to this interpretation of job-to-job and job-to-nonemployment transitions.

Table 5: Heterogeneous Effects by Sex and Parental Earnings

	log of quarterly earnings					
	(1)	(2)	(3)	(4)	(5)	(6)
A. All						
works for parent's employer	0.189*	0.273***	0.234***	0.362***	0.328***	0.307***
	(0.082)	(0.063)	(0.062)	(0.077)	(0.085)	(0.029)
F-statistic	238.9	358.0	446.8	330.4	316.2	1,434
observations	1,350,000	1,987,000	2,297,000	2,462,000	2,487,000	11,460,000
B. Daughters						
works for parent's employer	0.384*	0.413***	0.356*	0.443*	0.417*	0.424***
	(0.176)	(0.129)	(0.164)	(0.176)	(0.188)	(0.057)
F-statistic	64.3	131.2	100.0	106.1	130.9	679.8
observations	586,000	876,000	1,029,000	1,128,000	1,152,000	5,387,000
C. Sons						
works for parent's employer	0.075	0.222**	0.316***	0.405***	0.398***	0.312***
	(0.115)	(0.083)	(0.077)	(0.099)	(0.116)	(0.036)
F-statistic	97.7	198.3	245.7	176.9	161.3	854.2
observations	600,000	909,000	1,067,000	1,149,000	1,148,000	5,501,000
Sample Description						
parental earnings quintile	first	second	third	fourth	fifth	all

Notes: This table presents estimates on subsamples defined by the interaction between the quintile of parental earnings (defined by the column) and sex (defined by the panel). The outcome in all columns is the log of the first full quarter of earnings at the first stable job. The endogenous variable is an indicator equal to one if the individual works for their parent's employer (primary earner) at the first stable job, which is instrumented for using the average quarterly hiring rate at the parent's employer in the four quarters prior to entry. All specifications include a fixed effect for the parent's employer, a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer, and the standard vector of demographic characteristics. Standard errors are clustered at the level of parent's employer and are presented in parentheses.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

on earnings and parental earnings is entirely driven by sons.

Lastly, I use the same empirical strategy to investigate the earnings consequences of working for the employer of the parent who is the secondary earner.[45] The results, presented in Table A.10, indicate that working for the employer of the secondary earner leads to an increase in initial earnings by 29%. Thus, there is no evidence that working for the employer of the secondary earner produces different earnings benefits compared to working for the employer of the primary earner. In addition, Table A.11 presents the estimated effect of working for the father's and mother's employer. Working at the father's employer leads to a 52% and 33% increase in initial earnings for daughters and sons, respectively. Working at the mother's employer leads to a 28% and 34% increase in initial earnings for daughters and sons, respectively. All results suggest that there are substantial earnings benefits associated with working for a parent's employer, particularly for daughters who are able to get a job at their father's employer.

4.5 Interpreting the Local Average Treatment Effect

If the three identifying assumptions are satisfied, the two-stage least squares estimator identifies a LATE, which is the average treatment effect for the compliers. This section explores the relationship between the LATE and other causal parameters of interest. First, I provide a theoretical argument for why in my context, in which working for a parent's employer is determined by the decisions of multiple agents, the LATE may be a reasonable approximation of the *average treatment effect on the treated* (ATT)–which is the average treatment effect for individuals who work for their parent's employer. Second, I present empirical evidence to assess the plausibility of this interpretation.

Let $Y_i(d, z)$ denote the potential outcome of individual i with treatment status $D_i = d \in \{0, 1\}$ and instrument value $Z_i = z \in \{\underline{z}, \bar{z}\}$ where $\underline{z} < \bar{z}$. Let D_{zi} denote the treatment status of i when $Z_i = z$. Furthermore, assume the following: (Independence) $\{Y_i(D_{\bar{z}i}, \bar{z}), Y_i(D_{\underline{z}i}, \underline{z}), D_{\bar{z}i}, D_{\underline{z}i}\} \perp\!\!\!\perp Z_i$, (Exclusion) $Y_i(d, \underline{z}) = Y_i(d, \bar{z}) \equiv Y_{di}$ for $d = \{0, 1\}$,

[45] In order to avoid estimating effects of working with the primary earner, I limit the sample to cases in which the secondary earner is employed with a year of tenure in the quarter of entry and does work work at the same employer as the primary earner.

(First Stage) $\mathbb{E}[D_{\bar{z}i} - D_{\underline{z}i}] \neq 0$, and (Monotonicity) $D_{\bar{z}i} \leq D_{\underline{z}i} \; \forall \; i$. Under these assumptions, the instrumental variables estimator identifies a LATE, which is the average treatment effect for the compliers (i.e., the population for which $D_{\bar{z}i} < D_{\underline{z}i}$).

In the standard selection framework of Roy (1951), the LATE will likely depend on the specific values of the instruments, since selection into treatment is determined by a single agent who weighs the benefits (treatment effects) against the costs (instruments). To see this more formally, consider the selection model in which $D_{zi} = \mathbb{1}\{\beta_i > z\}$, where $\beta_i = Y_{1i} - Y_{0i}$ is the individual-level treatment effect. It immediately follows that the LATE, which is $\mathbb{E}[\beta_i | \underline{z} < \beta_i < \bar{z}]$, will generally depend on the values of the instruments.

In my context, selection is determined by the choices of more than one agent–the young worker and their parent's employer–and this potentially breaks the link between the instruments and the treatment effects. To see why, consider an alternative selection model in which the individual works for their parent's employer if and only if the employer makes them a job offer and they choose to accept the offer. The employer's decision to make an offer depends on the instruments and is defined as, $O_{zi} = \mathbb{1}\{\eta_i^O > z\}$. The child's decision to accept the offer depends on the benefits and is defined as, $A_{zi} = \mathbb{1}\{\beta_i > \eta_i^A\}$. Where η_i^O and η_i^A are unobserved error terms whose values are defined independent of D_i and Z_i.[46] Treatment status is then defined as, $D_{zi} = O_{zi} \times A_{zi}$.

The LATE and ATT are equal if the employer's decision to make an offer is unrelated to the child's decision to accept. Formally, if $\{\eta_i^O, \eta_i^A\} \perp\!\!\!\perp Z_i$ and $\{\beta_i, \eta_i^A\} \perp\!\!\!\perp \eta_i^O$, then

$$\underbrace{\mathbb{E}\big[\beta_i | \{\eta_i^A < \beta_i\}, \{\underline{z} < \eta_i^O < \bar{z}\}\big]}_{\text{LATE}} = \underbrace{\mathbb{E}\big[\beta_i | \{\eta_i^A < \beta_i\}, \{Z_i < \eta_i^O\}\big]}_{\text{ATT}} \tag{4}$$

Under these conditions, both the compliers and the individuals working for their parent's employer are a random sample of individuals who would accept an offer from their parent's employer if made one. Importantly, because of the multi-agent nature of the selection problem, the LATE and ATT may be equivalent even in the presence of selection on gains and selection bias. Appendix D develops a stylized behavioral model and provides

[46] More formally, let $\eta_i^x(d, z)$ denote the potential outcome with treatment status $D_i = d$ and instrument value $Z_i = z$. Then I assume that $\eta_i^x = \eta_i^x(d, z)$ for $x \in \{O, A\}$.

a more detailed discussion of the intuition by focusing on a specific case of equation 4.

The assumptions that imply the equality of the LATE and ATT also imply that the estimated treatment effects should not be sensitive to the variation exploited in the instrument; I test that implication here. To do so, I regress the instrument on the covariates from equation 3 and compute the residualized value, which is the source of identifying variation.[47] I then compute terciles based on the residualized instrument, partitioning the sample into periods in which employers have a relatively low, medium and high rate of hiring. I estimate equation 3 on samples defined by different combinations of the three terciles. The point estimate (standard error) is 0.44 (0.05), 0.31 (0.029) and 0.23 (0.11) when excluding observations from third, second and first terciles, respectively (see Table A.12). While there is some variation across the samples, the two-stage least square estimates are not excessively sensitive to range of variation exploited in the instrument.

An alternative approach to assessing the representativeness of the two-stage least squares estimates is to characterize the compliers. My data lack variables that strongly predict individual earnings benefits, but I can estimate the size of the complier population. The methodology developed by Abadie (2003) applies to binary instruments, so I construct three binary instruments which are equal to one when the residualized hiring rate exceeds the 25th, 50th and 75th percentiles. The estimated effect on log earnings when using these three binary instruments is 0.44, 0.42 and 0.25, respectively. The fact that these estimates are qualitatively similar to those obtained using the continuous instrument provides some evidence that the complier population for these instruments is not fundamentally different. For the three instruments, I find that 3.6%, 2.8% and 16% of the population is in the complier population, respectively.[48] Given that 5.6% of individuals in the estimation sample work for their parent's employer, the compliers represent a meaningful percentage the treated population.[49] Thus, the results provide additional evidence that the instrumental variables estimates are informative of the ATT.

[47]The distribution of the residualized hiring rate is both symmetric and smooth (see Figure A.10). The standard deviation is 0.35, which implies that a one standard deviation increase in the residualized hiring rate leads to an 8% increase in the probability of working for the parent's employer.

[48]Table A.13 presents estimates of the size and characteristics of the complier population.

[49]These estimates suggest that 49%, 25% and 69% of the individuals who work for their parent's employer are in the complier population, respectively.

5 Implications for Intergenerational Mobility

The results from Sections 3 and 4 show that individuals with higher-earning parents are both more likely to work for the employer of a parent and benefit more conditional on doing so. This suggests that the intergenerational transmission of employers reduces intergenerational mobility. Section 5 quantifies this effect by estimating the difference between observed measures of intergenerational mobility and those that correspond to a counterfactual world in which no one works for the employer of a parent. I maintain my focus on the initial labor market outcomes of young workers. As discussed in more detail below, this is an important difference compared to the intergenerational mobility literature, which typically focuses on longer-run measures of earnings.

5.1 Methodology

Consistent with the notation from Section 4.2, let $y_{ij(1)t}$ be the earnings of the individual when they work for their parent's employer and let $y_{ij(0)t}$ denote their earnings at the employer that is their next best option. The individual-level treatment effect is the difference between potential outcomes and is denoted $\beta_{it} = y_{ij(1)t} - y_{ij(0)t}$. Thus,

$$y_{ijt} = y_{ij(0)t} + D_{it}\beta_{it} \tag{5}$$

where D_{it} is equal to one if the individual works for a parent's employer and zero otherwise. Let y_p denote the log earnings of i's parents. My goal is to compare the observed joint distribution of y_{ijt} and y_p to the unobserved joint distribution of $y_{ij(0)t}$ and y_p.

The intergenerational transmission of employers will reduce intergenerational mobility if individuals with higher-earning parents tend to benefit more from working for a parent's employer. The expected earnings benefits depend on the likelihood of working for a parent's employer and the earnings consequences conditional on doing so. Formally,

$$\mathbb{E}\big[y_{ijt} \mid r_p\big] - \mathbb{E}\big[y_{ij(0)t} \mid r_p\big] = \mathbb{E}\big[D_{it}\beta_{it} \mid r_p\big] = \mathbb{E}\big[D_{it} \mid r_p\big] \times \mathbb{E}\big[\beta_{it} \mid r_p, D_{it} = 1\big] \tag{6}$$

where r_p is the percentile rank of parental earnings and the second equality follows from iterated expectations. Equation 6 is important in illustrating how I use the empirical estimates from the previous sections to estimate counterfactual measures of intergenerational mobility. Estimates of $\mathbb{E}\big[D_{it} \mid r_p\big]$ were discussed in Section 3 and estimates of $\mathbb{E}\big[\beta_{it} \mid r_p, D_{it} = 1\big]$ were discussed in Section 4–Section 4.5 argues why the ATT is a reasonable interpretation the parameter identified by the instrumental variables estimator.

I begin by focusing on the intergenerational elasticity of earnings (IGE), which is a commonly used measure of intergenerational mobility. The IGE, which is defined as $\rho(y_{ijt}, y_p) = \frac{\text{cov}(y_{ijt}, y_p)}{\text{var}(y_p)}$, is the estimated coefficient of a regression of the log earnings of the child on the log earnings of the parents. From equation 5, it follows that:

$$\rho(y_{ijt}, y_p) - \rho(y_{ij(0)t}, y_p) = \frac{\text{cov}(D_{it}\beta_{it}, y_p)}{\text{var}(y_p)} \tag{7}$$

The expression illustrates that the transmission of employers will increase the IGE when individuals with higher-earning parents tend to benefit more.

I quantify the degree to which the intergenerational transmission of employers shapes the IGE by estimating the the difference between $\rho(y_{ijt}, y_p)$ and $\rho(y_{ij(0)t}, y_p)$. While $\text{var}(y_p)$ is directly observable in the data, $\text{cov}(D_{it}\beta_{it}, y_p)$ is not. To estimate the latter term I develop and use the following approximation:

$$\text{cov}(D_{it}\beta_{it}, y_p) \approx \mathbb{E}\Big[\mathbb{E}[y_p|r_p] \times \mathbb{E}\big[D_{it}|r_p\big] \times E\big[\beta_{it}|D_{it} = 1, r_p\big]\Big] - \mathbb{E}[y_p] \times \mathbb{E}[D_{it}] \times \mathbb{E}[\beta_{it}|D_{it} = 1] \tag{8}$$

In addition to using the identity from equation 6, equation 8 relies on the following approximation, $\mathbb{E}[y_p D_{it}\beta_{it}|r_p] \approx \mathbb{E}[y_p|r_p] \times \mathbb{E}[D_{it}\beta_{it}|r_p]$, which is based on the insight that the expected value of the product of two random variables is approximately equal to the product of their expected value if there is sufficiently little variation in one of the random variables. I derive the approximation in Appendix C and present empirical evidence that it performs well by showing that the IGE estimates derived from the micro data are virtually identical to approximations based on this same methodology.

My methodology represents a significant improvement over the descriptive analysis in Corak and Piraino (2011) and Stinson and Wignall (2018). These two papers estimate a standard intergenerational earnings regression as well as a modified specification in which they control for whether an individual works for their parent's employer.[50] They then attempt to determine how the transmission of employers shapes intergenerational mobility by comparing the estimated coefficients on parental earnings between the two specifications and by examining sign of the coefficient on the interaction between parental earnings and employer transmission. As both papers acknowledge, the modified intergenerational earnings regression is likely to deliver biased estimates of the earnings benefits of employer transmission, which makes their estimates difficult to interpret. In contrast, I use causal estimates of the earnings benefits of working for a parent's employer in order to quantify how the IGE would change if no one worked for their parent's employer.

5.2 Counterfactual Estimates of Intergenerational Mobility

Panel A of Table 6 presents estimates of the IGE. Columns 1-3 present estimates for daughters, sons, and the full sample. The elasticities, which range from 0.13 to 0.16, are substantially lower than typical estimates of IGE from the literature (Black and Devereux 2011). To investigate this discrepancy, I produce alternative estimates of the IGE, which measure the earnings of the children in 2016 (when the children are between the ages of 24 and 35). In Table A.14, columns 1-3 of Panel A present estimates based on samples that include children with zero earnings in 2016 (by taking the hyperbolic sine of earnings) and the estimates of the IGE are closer to 0.4, which is comparable to other estimates from the United States. Thus, the low estimates of the IGE appear to be an artifact of focusing on labor market outcomes at the time of entry. Panel B of Table A.14 presents estimates of the IGE for a subsample of the children with strictly positive earnings in 2016. Within this sample, the estimated IGE for the full sample is 0.235, which is much closer to the estimates based on initial labor market outcomes. These results highlight the fact that the IGE is sensitive to how observations with zero earnings are dealt with

[50]The practice of including covariates in the intergenerational earnings regression has been commonly employed to investigate the role of other factors–such as education (Edie and Showalter 1999)–in shaping rates of mobility. In general, results from these regressions will produce biased estimates of the effect of the factor on earnings–and will therefore be difficult to interpret–when the factor is correlated with unobserved characteristics that have an independent effect on earnings.

Table 6: Intergenerational Elasticity of Earnings

	sample		
	daughters (1)	sons (2)	all (3)
A. Observed			
IGE	0.1565	0.1298	0.1430
	(0.0002)	(0.0003)	(0.0002)
B. No Transmission with Primary Earner			
percent change in IGE	-2.04%	-10.79%	-4.73%
	(6.52)	(5.02)	(3.30)
C. No Transmission with Either Parent			
percent change in IGE	-3.87%	-23.09%	-9.68%
	(12.25)	(9.39)	(6.16)
observations	8,416,000	8,591,000	17,010,000

Notes: The results in columns 1-3 corresopnd to daughters, sons, and all children, respectively. Panel A presents the observed intergenerational elasticity of earnings (IGE), which is denoted $\rho(y_{ijt}, y_p)$ and is estimated with sample weights via weighted least squares. Panels B and C present the percent by which the IGE estimates in Panel A would change if no children were to work for the employer of the parent who is the primary earner or either parent, respectively. The percent change is defined as, $\frac{\rho(y_{ijt},y_p)-\rho(y_{i(j0)t},y_p)}{\rho(y_{ijt},y_p)} \times 100$. The treatment effects used to construct the counterfactual estimates are estimated via two-stage least squares and are estimated separately by the quintile of the parental earnings distrbution for the results in column 3 and are estimated separately by quintile of the parental eanrings distribution and the sex of the child for columns 1 and 2. Standard errors are presented in parentheses and are calculated using the delta method and take into account the uncertainty in the estimated earnings consequences.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

and suggests that my estimates of the IGE based on initial labor market outcomes are lower primarily because they condition on positive earnings.[51]

The intergenerational transmission of employer leads to a modest reduction in the IGE. Panel B of Table 6 indicates that the IGE would be about 5% lower if no one worked for the employer of the parent who is the primary earner.[52] The effect of employer transmission on IGE is larger for sons because, relative to daughters, both the probability of working for a parent's employer and the earnings benefits conditional on doings so

[51]To further investigate these patterns, Figure A.11 plots the avergae log earnings of children against the average log earnings of the parents and illustrates that the strength of the intergenerational relationship in earnings is dampened in the lower parts of the distribution. This may be explained by the fact that I focus on the earnings at the first stable job, when many workers are earning the minimum wage.

[52]Estimates of the ATT can be found in Table 5. I allow all estimates of the ATT to vary by parental earnings quintile. For the counterfactual estimates presented in column 3 of Table 6 I use the pooled estimates of the ATT presented in Panel A of Table 5. For the counterfactual estimates presented in columns 1 and 2, I use the appropriate sex-specific estimates.

are more strongly related to parental earnings. Specifically, Columns 1 and 2 indicate that the counterfactual IGE for daughters and sons would be about 2% and 11% lower, respectively. Panel C of Table 6 indicates that the IGE would be about 10% lower if no one worked for the employer of either parent. For the case of working for the employer of the secondary earner, I am unable to estimate heterogeneous effects by both parental earnings and sex. Thus, I assume that the effect of working for the employer of the secondary earner is the same as working for the employer of the primary earner. As previously discussed, this appears to be true, at least in the full sample.

The standard errors, presented below in parentheses, indicate that uncertainty in the estimates of the ATT create some uncertainty regarding the magnitude of these effects. Table A.15 replicates the analysis but uses the point estimates and standard errors from Table 2, which assumes no heterogeneity in effects by parental earnings or sex. Here the magnitudes are smaller, suggesting a 2% decline in IGE if no one worked for a the employer of either parent, but they are also much more precisely estimated (standard error is 0.20). While there is some uncertainty around the exact magnitude, both sets of results suggest that the transmission of employers leads to a modest decrease in the IGE.

In addition to the IGE, I consider an alternative measure of intergenerational mobility: the conditional expected rank (CER). The CER is defined as, $E[r_{ijt}|r_p]$, where r_{ijt} is the percentile rank of the earnings of the child, calculated within cohorts and using sample weights. Rearranging terms in equation 6 reveals how to estimate, $E[r_{ij(0)t}|r_p]$.[53] The CER provides a more detailed picture of how the expected earnings benefits, defined as $\mathbb{E}[D_{it}\beta_{it}]$, differ across subgroups. Figure A.12 presents the expected earnings benefits by sex, race/ethnicity and parental earnings. The benefits are largest for non-Black males whose parents are in the top two quintiles of the earnings distribution. The results by race/ethnicity should be interpreted with some caution since I do not have sufficient power to estimate effects by parental earnings, sex and race/ethnicity and instead assume that,

[53]Specifically, $\mathbb{E}\big[r_{ij(0)t}|r_p\big] = \mathbb{E}\big[r_{ijt}|r_p\big] - \mathbb{E}\big[D_{it}|r_p\big] \times \mathbb{E}\big[\beta_{it}|D_{it} = 1, r_p\big]$. The two-stage least squares estimates of the effect of working for the employer of the primary on the earnings rank of the children are presented in table A.16. When constructing the counterfactual estimates for the case in which no individual worker for the employer of either parent, I assume that the effect of working for a parent's employer, conditional on parental earnings quintile, is the same for both primary and secondary earners.

within groups defined by sex and the parental earnings quintile, average treatment effects do not differ by race/ethnicity.

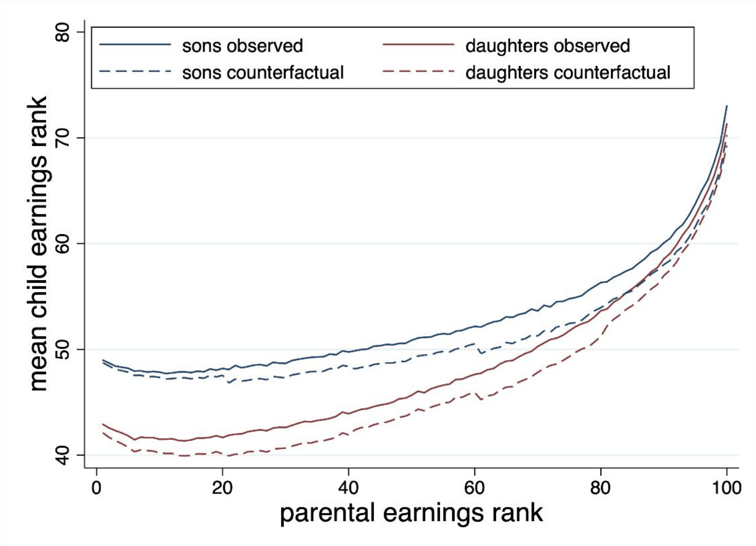
The intergenerational transmission of employers has varying implications for the gender pay gap within different parts of the parental earnings distribution. Panel A of Figure 3 plots the observed and counterfactual CER.[54] Estimating a linear regression using the results in Panel A reveals that, if no one worked for the employer of either parent, the slope of the rank-rank relationship would be 13% and 3% lower for sons and daughters, respectively. While sons are more likely to work for the employer of either parent, daughters experience larger earnings benefits conditional on doing so. Panel B illustrates how this plays out across the parental earnings distribution and plots the expected benefits of working for a parent's employer–which corresponds to the difference between the observed CER and the counterfactual CER–against the parental earnings percentile. Daughters benefit more from working for a parent's employer in the bottom two quintiles of the parental earnings distribution while sons benefits more in the top quintile and both benefit equally elsewhere. Averaging these effects across the parental earnings distribution indicates that the earnings gap between sons and daughters would be 4% larger if no one worked for the employer of either parent.

Section 3 found that Black sons are less likely to work for the employer of a parent relative to other sons in the same parental earnings percentile. This result is interesting in light of recent work by Chetty et al. (2020), who find that, conditional on parental income, Black males have lower expected income compared to White males. The solid lines in Figure 4 present the CER measures for Black and White sons and replicate the finding of Chetty et al. (2020): Black sons earn less on average relative to White sons with parents in the same earnings percentile. The dashed lines below represent the counterfactual CER. Since Black sons are less likely to work for the employer of a parent, they benefit less from employer transmission. To make this point clear, Panel B of Figure 4 presents the proportion of the Black-White gap (vertical distance between the solid

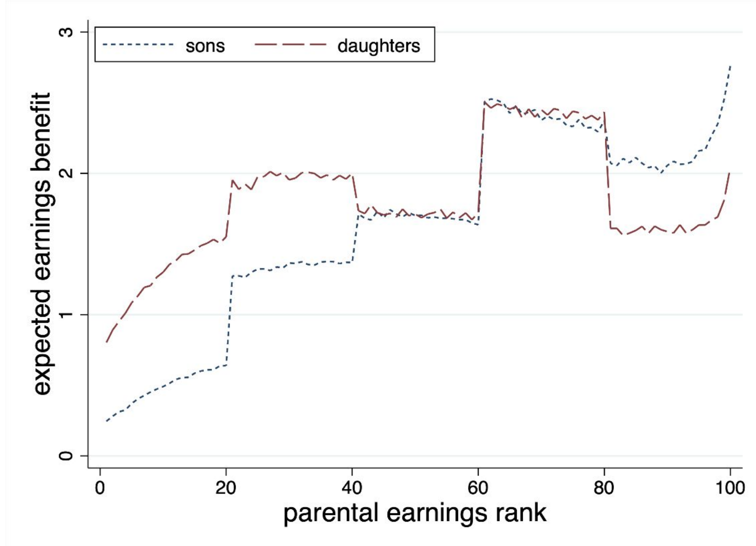
[54]Figure 3 presents the results separately for sons and daughters. Panel A of Figure A.13 plots analogous results for the pooled sample of sons and daughters. The counterfactual slope of the rank-rank relationship is 2% and 5% lower for the scenario in which no individual worker for the employer of the primary earner and secondary earner, respectively.

Figure 3: Conditional Expected Rank by Sex

(A) Observed and Counterfactural



(B) Expected Benefits

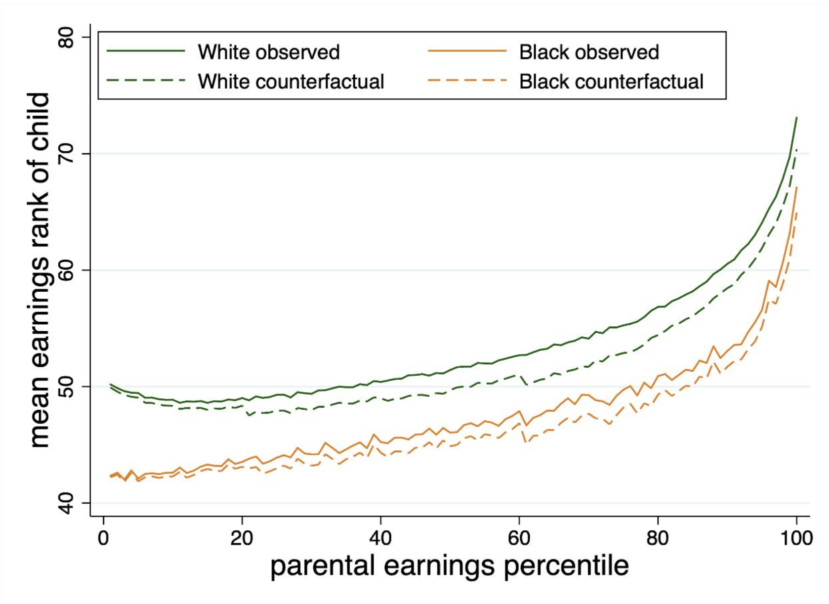


Notes: The solid lines in Panel A present the conditional expected rank (CER) separately for sons and daughters. The dashed lines below them represent the counterfactual CER that correspond to a scenario in which no individual works for the employer of either parent. Panel B plots the expected earnings benefit, which is the difference between the observed and counterfactual CER. The treatment effects used to construct the counterfactual estimates are estimated via two-stage least squares and are estimated separately by the quintile of the parental earnings distribution and the sex of the child. All statistics, aside from the two-stage least squares estimates, are calculated using sample weights.

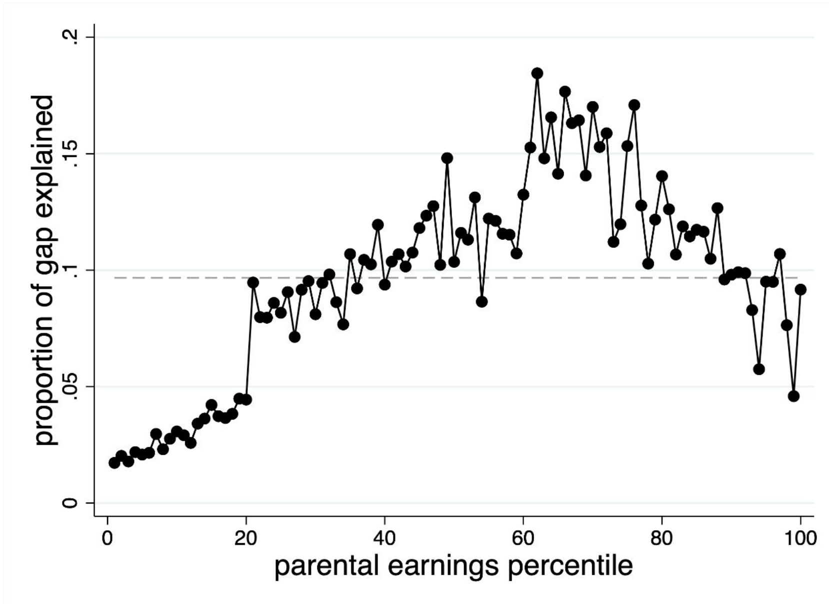
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Figure 4: Black-White Earnings Gap for Sons

(A) Observed and Counterfactural



(B) Explained by Employer Transmission



Notes: The solid lines in Panel A present the conditional expected rank (CER) separately for White and Black sons. The dashed lines represent the counterfactual CER that correspond to a world in which no individual works for the employer of either parent. Panel B plots the proportion of the Black-White earnings gap that is explained by the transmission of employers for each percentile of the parental earnings distribution. The dashed line represents the average across all percentiles. The treatment effects used to construct the counterfactual estimates are estimated via two-stage least squares and are estimated separately by the quintile of the parental earnings distribution and sex of the child. All statistics, aside from the two-stage least squares estimates, are calculated using sample weights.

Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

lines in Panel A) that is explained by the intergenerational transmission of employers at each percentile of the parental earnings distribution. On average, the transmission of employers explains about 10% of the conditional Black-White earnings gap. While other factors clearly play an important role in determining this earnings gap, my results suggest that young Black males are at a relative disadvantage in part because they are less likely to have an employed father who can help them find work.

5.3 Key Insights from Stylized Model

I synthesize my findings by developing a stylized model of intergenerational mobility in which parents affect the earnings of their children by shaping the development of their human capital and by providing access to high-paying employers. I summarize the key insights from the model in this section and refer the reader to Appendix D for the details. Relative to other models of intergenerational mobility, the novel features of my model are that I (1) incorporate an employer-specific component into individual earnings and (2) explicitly model the choices that lead individuals to work for a parent's employer.[55]

The key aspects of the model setup are as follows. Motivated by my finding that parents provide access to higher-paying employers as well as the literature on imperfect competition in the labor market, I depart from existing models of intergenerational by allowing earnings to depend on not only the human capital of the child, but also the pay premium associated with the employer to which they match. I assume that: there is a positive correlation between parental earnings and human capital, children with higher levels of human capital tend to match to higher-paying employers absent parental contacts, and working at the parent's employer affects earnings solely through the effect on the employer pay premium. Individuals will work for the employer of their parent if and only if the employer makes an offer and the child accepts the offer. The employer's

[55]Magruder (2010) and Corak and Piraino (2012) and the only two papers that have developed models of intergenerational mobility that incorporate parental contacts. Neither paper considers the role of employer pay premiums and neither paper considers the endogenous use of social contacts. Magruder (2010) assumes that parental contacts produce a positive correlation between the employment status of parents and children. Corak and Piraino (2012) assume that the earnings of the parent have a direct positive effect on the earnings of the child. In both papers that affect of parental contacts on intergenerational mobility is defined by the sign of a single parameter.

decision to make a job offer depends on the human capital of the child and the parent, whereas the child's decision to accept the offer depends on the earnings benefits.

The model highlights two key insights. First, the effect of the intergenerational transmission of employers on intergenerational mobility is theoretically ambiguous in sign. On the one hand, higher-earning parents may be in a better position to procure high-paying job offers for their children. On the other hand, children with low-earning parents tend to have lower levels of human capital and therefore may be more reliant on their parents to find a decent-paying job. The theoretical ambiguity ultimately stems from the fact that working for a parent's employer is endogenous and depends on decisions made by the employer and the child. Thus, while my empirical evidence suggests that employer transmission reduces mobility, this conclusion might differ in other contexts.

Gaining access to higher-paying employers is the *direct effect* of working for a parent's employer. However, parents might account for this direct effect when making investments in the human capital of their children; I refer to this as the *indirect effect*. The second insight of the model is that the indirect effect has a theoretically ambiguous effect on intergenerational mobility. On the one hand, working for a parent's employer increases the marginal returns to human capital investments by providing access to higher paying employers. On the other hand, the marginal returns are pushed down because higher ability individuals are less likely to work for their parent's employer and benefit less conditional on doing so. Thus, parental investment decisions could either amplify or dampen the direct effect of employer transmission on intergenerational mobility. The counterfactual exercise described in Section 5.1 should be viewed as a partial equilibrium analysis, which does not account for the possibility that parents (or children) might adjust their investments in human capital if there was no option to work at a parent's employer. While it would be interesting to explore the implications of the the indirect effect, quantifying the importance of the direct effect of employer transmission on mobility is the obvious starting point, as the indirect effect through human capital accumulation is unlikely to be important if direct effect is negligible.

6 Conclusion

This paper combines survey and administrative data in order to investigate how the earnings of young workers are affected by the intergenerational transmission of employers. I start with a descriptive analysis, and find that 7% of individuals work for the employer of a parent at their first stable job and 29% do so at some point between the ages of 18 and 30. This tendency is best explained by parents playing a direct role in the hiring or job search process to help children who have limited options in the labor market. I then use an instrumental variables strategy, which exploits exogenous variation in the availability of jobs at the parent's employer, and find that working for the employer of a parent increases earnings by 31%. These large earnings benefits are explained by parents providing access to higher-paying employers: Young workers who find their first stable job at the employer of a parent start their careers on a higher rung of the job ladder.

Individuals with higher-earning parents are more likely to work for the employer of a parent, and benefit more conditional on doing so, and thus the intergenerational transmission of employers increases the intergenerational persistence in earnings. I develop a new methodology that allows me to quantify this effect using descriptive statistics and causal estimates. I find that the elasticity of the initial earnings of an individual with respect to the earnings of their parents would be 10% lower if no one worked for the employer of a parent. Examining patterns by family background, sex, and race/ethnicity reveals that non-Black males with high-earning parents benefit the most from the intergenerational transmission of employers. My results likely understate the importance of parental labor market networks more broadly defined since parents may also provide access to jobs at other employers through social contacts, such as friends, former co-workers, or classmates. This is especially true for the implications for intergenerational mobility if higher-income parents are more likely to have contacts outside of their current employer.

My results relate to the normative assessment of whether rates of intergenerational mobility are too low in the United States, an assessment which depends on whether the economic system that produces the intergenerational persistence in earnings is equitable and efficient. While equity depends on subjective moral values, a core ideal in the United

State is that of equality of opportunity, which requires that an individuals' success be a function of their hard work and ability rather than the circumstances into which they were born. Thus, from an equity standpoint, my finding that individuals from high-income families disproportionately benefit from their parents' connections should raise concerns about the relatively low levels of intergenerational mobility in the United States.[56] My results do not speak directly to the implications for efficiency and future research should aim to understand whether the use of parental labor market networks leads to gains or losses in productivity.

My results are also informative of the positive assessment of what would be required to achieve equality of opportunity. One view is that the United States is a meritocracy, where economic rewards are determined by hard work and ability. According to this view, efforts to expand economic opportunity should aim to equip everyone with the skills they need to succeed in the labor market. Government programs such as Head Start, which provides access to early childhood education, and the Pell Grant program, which helps students pay for college, are both examples of programs that promote the development of skills for individuals from disadvantaged backgrounds. However, my results challenge a purely meritocratic view of the labor market, as individuals from high-income families are likely to earn more not only because they are more skilled, but also, because their parents are able to provide access to high-paying firms. If the labor market plays a direct role in propagating intergenerational disadvantage, then achieving equality of opportunity in terms of education will not necessarily produce equality of opportunity in the labor market. Rather, individuals from disadvantaged backgrounds may require additional support throughout their early careers. Gaining a better understanding of the mechanisms through which parents help their children find high-paying jobs may offer ideas for how to help young workers who cannot rely on the connections of their parents to more successfully navigate the labor market.

[56] According to Roemer (1998), equality of opportunity requires that the outcomes of individuals are not systematically determined by factors for which they are not responsible. Defining what to hold someone responsible for is a subjective judgment. But most people in the United States would likely agree that individuals should not be responsible for their parents' lack of connections in the labor market.

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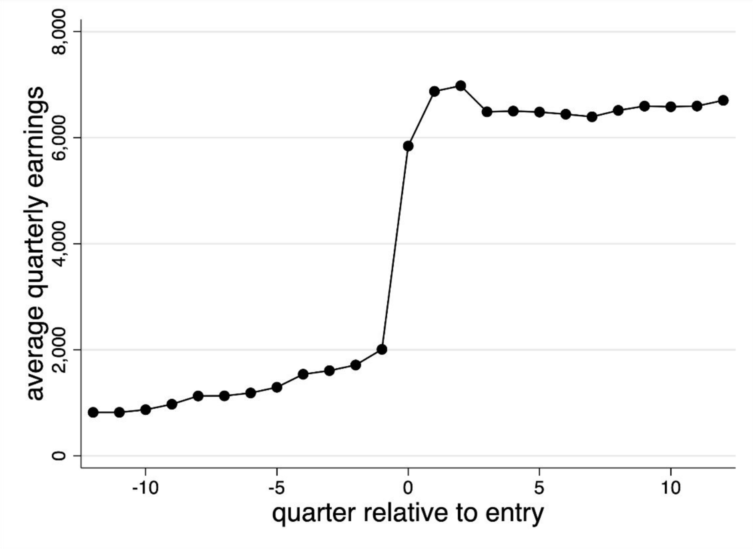
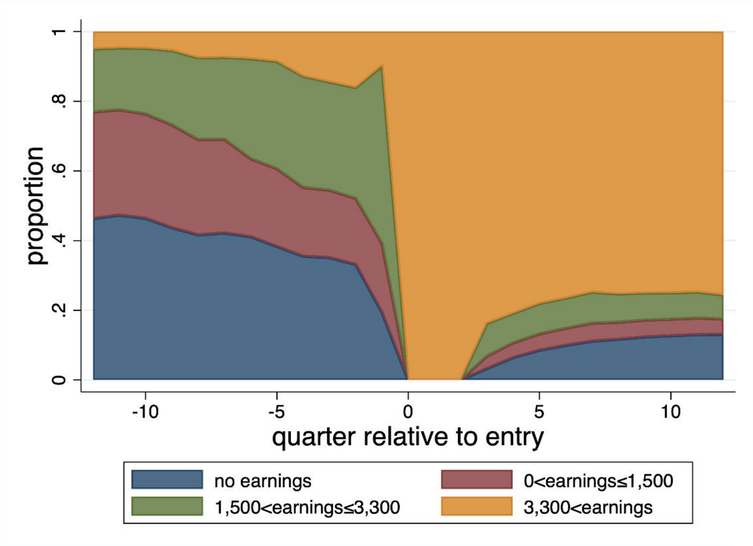
Appendix A Additional Empirical Results

Figure A.1: Earnings Before and After Entry

(A) Average Earnings

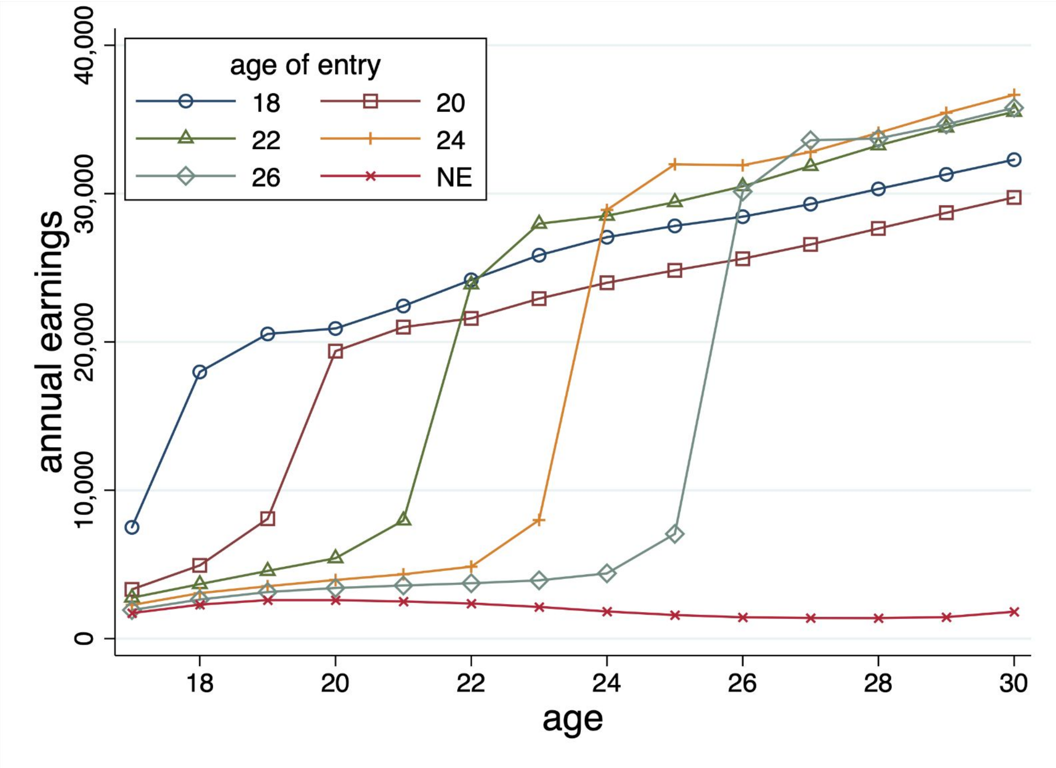


(B) Earnings Categories



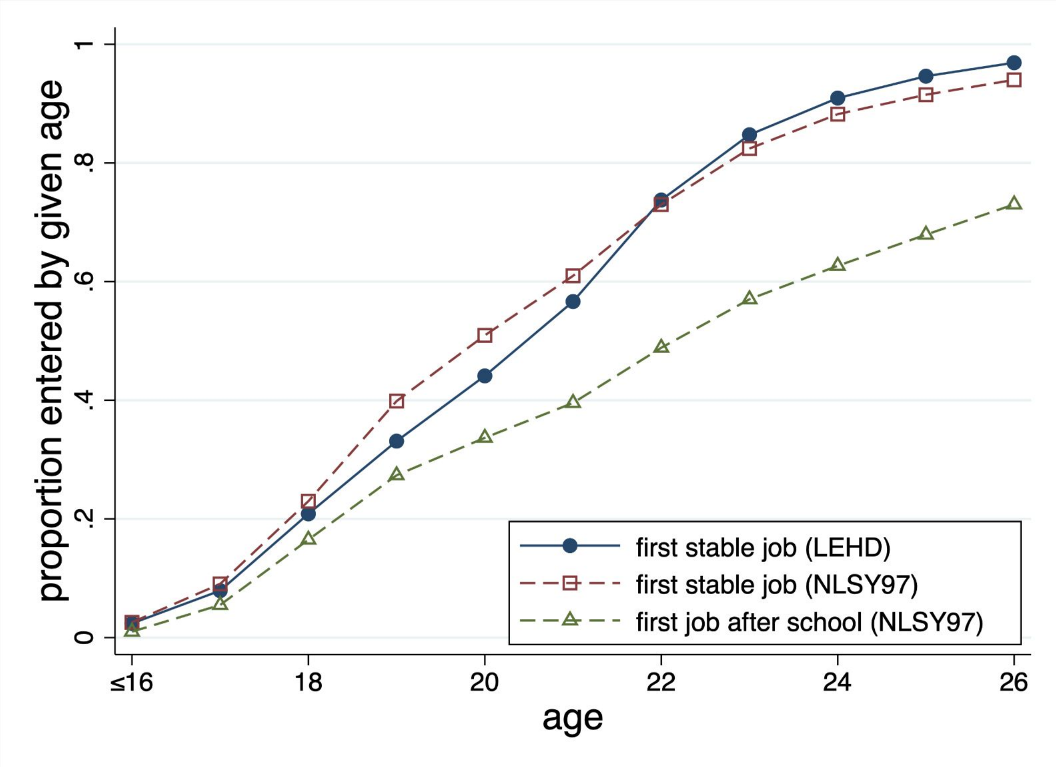
Notes: Both figures plot earnings in the 12 quarters before and after entry. Panel A plots the average quarterly earnings and Panel B plots the proportion of individuals with quarterly earnings in one of four mutually exclusive categories. All statistics are calculated using sample weights.
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Figure A.2: Age-Earnings Profile by Age of Entry



Notes: The figure plots the average annual earnings by age for different groups of workers defined by the age they were when they entered the labor market. The category, NE, is a group of workers that never entered the labor market. The sample includes all children who turned 30 by 2016 and all statistics are calculated using sample weights.
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Figure A.3: Age of Entry



Notes: The figure plots the cumulative proportion of children that have entered the labor market by the age indicated on horizontal axis. For comparison, I also plot results using alternative measures of entry constructed from the NLSY97. These measures include the first stable job (working at least 35 hours for 36 consecutive weeks) and the first stable job after all schooling is completed. All statistics are calculated using sample weights.
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics (LEHD) and 2000 Decennial Census files and data from the National Longitudinal Survey of Youth 1997 cohort (NLSY97).

Table A.1: Placebo Analysis

	Full Sample			Past Employer		Future Employer	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Employer Transmission							
proportion at same employer	0.0561	0.0013	0.0008	0.0444	0.0078	0.0472	0.0107
B. First Stage							
hiring rate	0.1187***			0.0781***		0.0893***	
	(0.0031)			(0.0073)		(0.0058)	
placebo hiring rate		0.0003**	0.0002*		0.0030**		0.0027***
		(0.0001)	(0.0001)		(0.0010)		(0.0006)
F-statistic	1434	10	6	113	9	235	20
C. Reduced Form							
hiring rate	0.0364***			0.0204		0.0183*	
	(0.0033)			(0.0106)		(0.0086)	
placebo hiring rate		-0.0016	0.0018		0.0092		0.0057*
		(0.0015)	(0.0015)		(0.0060)		(0.0025)
employer	primary earner	census tract	labor market	primary earner	past employer	primary earner	future employer
observations	11,460,000	10,470,000	10,530,000	1,114,000	1,209,000	1,434,000	1,554,000

Notes: The column headers indicate the sample restrictions where the results in: columns 1-3 are based on the full sample, columns 4-5 are based on the sample of primary earners who have a different employer in the quarter in which their child enters the labor market relative to their employer when the child was ten years old, and columns 6-7 are based on the sample of primary earner who have a different employer in the quater in which their child enters the labor market relative to their employer in 2016. The samples used in columns 4-7 also require that the past and future employers have strictly positive employment in the quarter in which the child enters the laboor market. The results in columns 1, 4 and 6 correspond to the current employer of the parent who is the primary earner. The results in columns 2, 3, 5 and 7 correspond to the placebo employers. The plabebo employers are defined as: (column 2) an employer in the same census tract and employer size category as the employer of the primary earner, (column 3) an employer in the same employer size cateory and local labor market (defined by there interaction between commuting zone and three-digit industry code), (column 5) the past employer, and (column 7) the future employer. Panel A presents the proportion of children who worked for the employer defined by the column. Panel B presents estimates from the first-stage specification. Panel C presents estimates from the reduce form specifcation. Standard errors are clustered at the level of the employer defined by the column and are presented in parentheses. All statistics are calculated on a sample without singleton observations.

Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

Table A.2: Intergenerational Transmission of Employers and Education

	works for parent's employer				
	(1)	(2)	(3)	(4)	(5)
less than high school	0.055***	0.061***	0.048***	0.023*	0.055***
	(0.003)	(0.004)	(0.006)	(0.009)	(0.002)
high school	0.043***	0.054***	0.047***	0.042***	0.048***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)
some college	0.020***	0.027***	0.024***	0.025***	0.024***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
parental earnings quartile	first	second	third	fourth	all
observations	180,000	183,000	177,000	165,000	705,000

Notes: Each column presents estimates from a separate regression. The outcome variable is an indicator equal to one if the first stable job is at the employer of either parent. The main independent variables include indicator variables for the highest level of education: less than high school, high school or equivalent, and some college or Associate degree. Bachelor's degree or advanced degree is the omitted educational category. Each regression controls for the interaction between the sex of the individual and the percentile of the parental earnigns distribution. All results are based on the sample of individuals who respond to the American Community Survey after they turn 25. Columns 1 through 4 present estimates based on the sample of individuals whose parents are in the first through fourth quartiles of the parental earnigns distribution, respectively. Column 5 includes all indviduals.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics, 2000 Decennial Census files and responses to the American Community Survey.
*** p≤0.001, ** p≤0.01, * p≤0.05

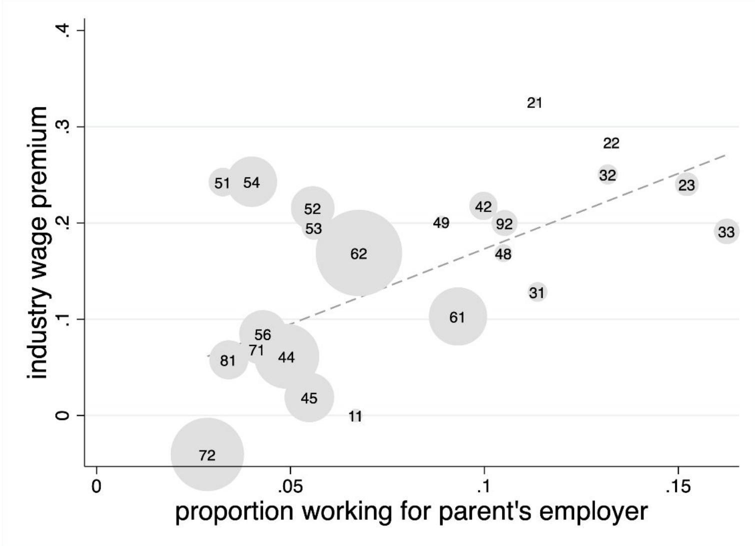
Table A.3: Intergenerational Transmission of Employers and Unemployment

	works for parent's employer			
	(1)	(2)	(3)	(4)
unemployment rate	-0.068**	0.128***	0.191***	0.064*
	(0.022)	(0.024)	(0.032)	(0.031)
covariates				
age of entry		X	X	X
quarter of entry			X	
county				X
observations	17,010,000	17,010,000	17,010,000	17,010,000

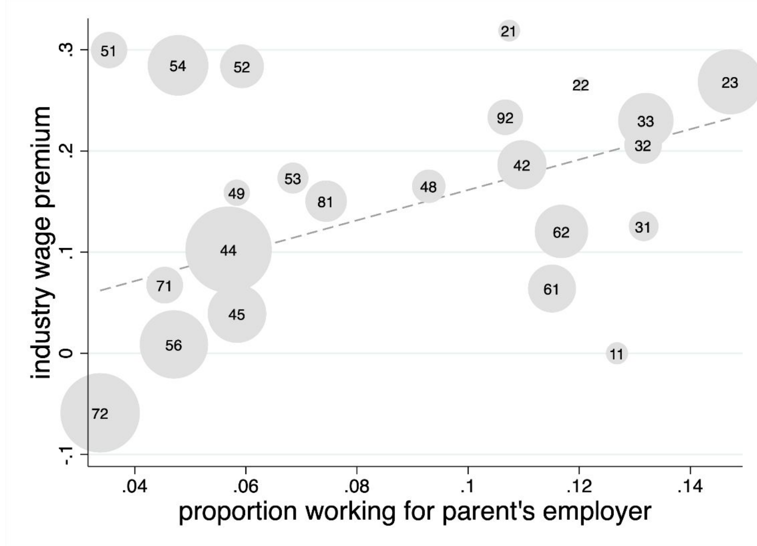
Notes: Each column presents estimates from a separate regression. The outcome variable is an indicator equal to one if the first stable job is at the employer of either parent. The main independent variable is the county-level unemployment rate, which ranges from zero to one, measured in the year in which the child enters the labor market. The different columns include additional covariates as indicated by the rows below the estimates. The covariates include fixed effects for: the age of entry, the quarter of entry, the county in which the individual entered the labor market. Standard errors are two-way clustered at the county and quarter of entry.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files and unemployment data from the U.S. Bureau of Labor Statistics.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

Figure A.4: Intergenerational Transmission of Employers and Industry Wage Premiums

(A) Daughters



(B) Sons

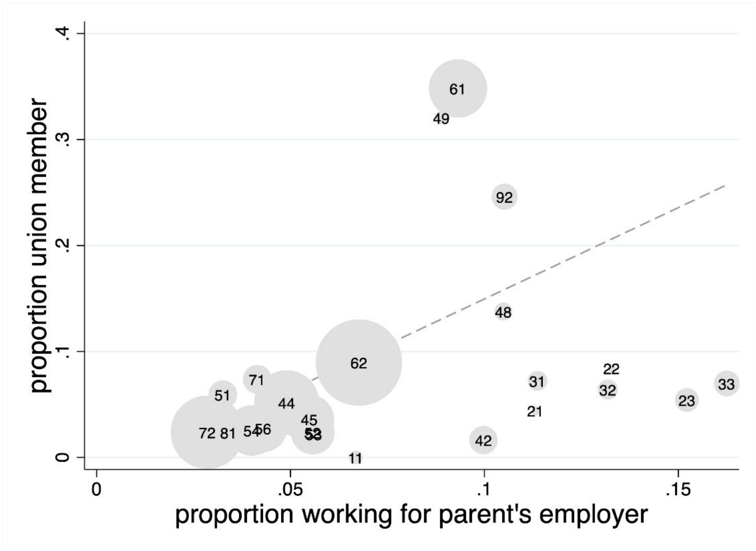


Notes: Panels A and B present results for daughters and sons, respectively. Each point on the plot presents information related to an industry, measured as the two-digit North American Industry Classification System code. The horizontal axis represents the proportion of individuals in a given industry who work for the employer of either parent at their first stable job, where this proportion is calculated separately for sons and daughters. The vertical axis is the industry-level wage premium. The wage premium is estimated using data from the Current Population Survey (CPS) by regressing log wages on an set of industry dummies (Agriculture, Forestry, Fishing, and Hunting is the omitted industry category) and controlling for year fixed effects, a third order polynomial in potential experience and fixed effects for the level of education. The sample from the CPS includes all employed individuals between the ages of 26 and 35 and regressions are estimated separately for males and females. The size of the marker is proportional to the number of individuals whose first stable job is in that industry.

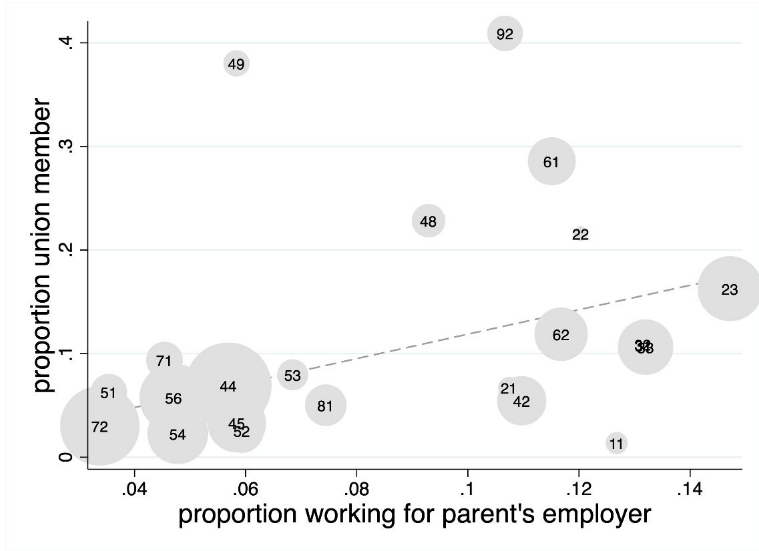
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files and publicly available data from the Current Population Survey.

Figure A.5: Intergenerational Transmission of Employers and Union Membership

(A) Daughters



(B) Sons



Notes: Panels A and B present results for daughters and sons, respectively. Each point on the plot presents information related to an industry, measured as the two-digit North American Industry Classification System code. The horizontal axis represents the proportion of individuals in a given two-digit industry who work for the employer of either parent at their first stable job, where this proportion is calculated separately for sons and daughters. The vertical axis presents proportion of individuals within that industry that are a member of a union. This statistic is calculated separately for males and females using data from the Current Population Survey, which include a sample of employed individuals between the ages of 26 and 35. The size of the marker is proportional to the number of individuals whose first stable job is in that industry.

Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files and publically available data from the Current Population Survey.

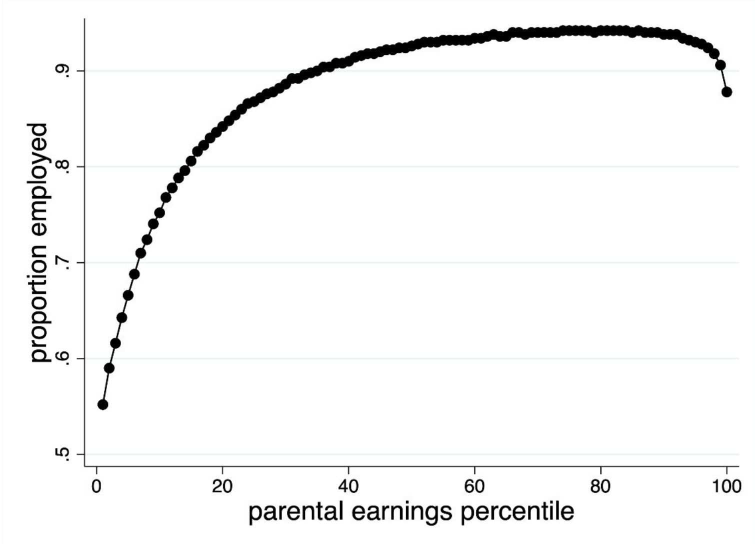
Table A.4: Intergenerational Transmission of Employers by Sex

	works for employer of			
	neither parent	father	mother	both parents
	(1)	(2)	(3)	(4)
A. Daughters				
	0.940	0.013	0.040	0.006
A. Sons				
	0.922	0.042	0.026	0.010

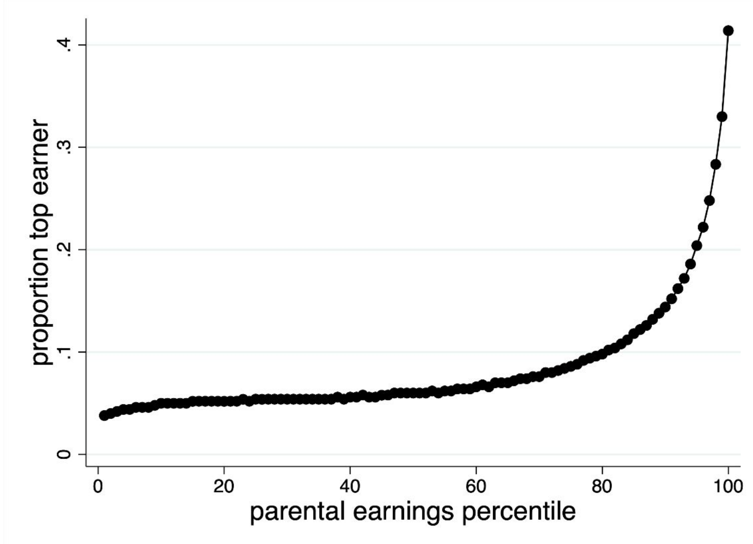
Notes: Panels A and B present results for daughters and sons, respectively. Columns 1 through 4 present the proportion of individuals who find their first stable job at the same employer as neither parent, the father, the mother, and both parents, respectively. The proportions are calculated separately by the sex of the child. All statistics are calculated using sample weights.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Figure A.6: Correlates with Parental Earnings

(A) Parent is Employed



(B) Parent is Top Earner within Employer

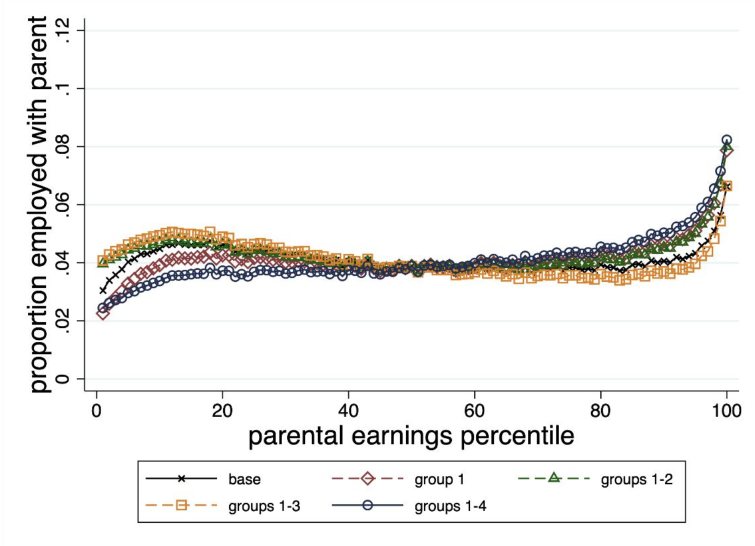


Notes: This figure describes the labor market outcomes of the parent who is the primary earner in the quarter in which their child enters the labor market. Panel A presents the proportion of parents (primary earner) that are employed. Panel B presents the proportion of parents (primary earner) whose earnings are in the top percentile of the within employer earnings distribution. All statistics are presented separately for each percentile of the parental earnings distribution and are calculated using sample weights.

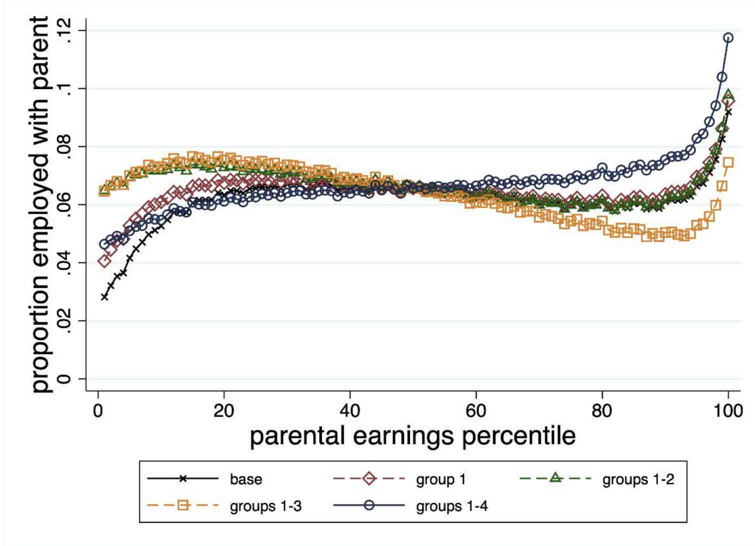
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Figure A.7: Intergenerational Transmission of Employers Conditional on Observable Characteristics

(A) Daughters



(B) Sons

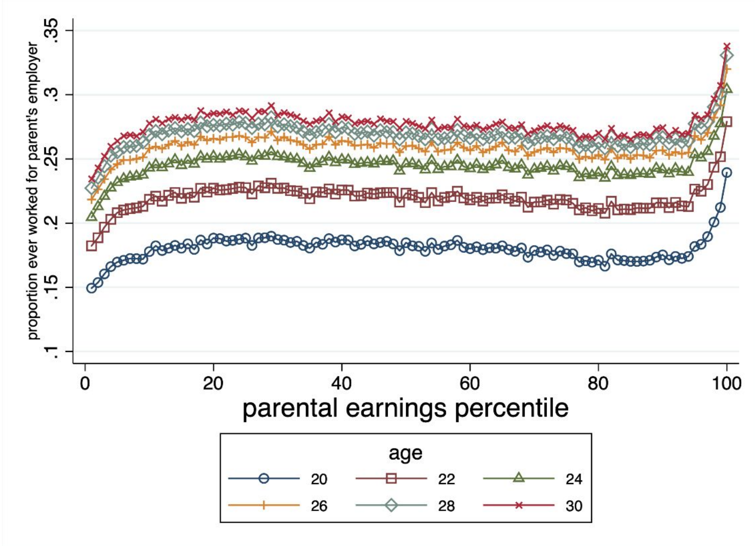


Notes: Panels A and B present results for daughters and sons, respectively. The figures present estimates from a regression of an indicator equal to one if the child works for the employer of the primary earner at their first stable job on a set of dummy variables for the percentile of the parental earnings distribution and a set of covariates. The median is the left out percentile and the figure presents the share of children who work for their parent's employer at median plus the point estimate for a given percentile. The base specification includes no other covariates. All other specifications sequentially add the covariates in groups 1-4. The covariates in group 1 include indicators for: race (Black, White, Asian, and other), Hispanic, born in the United States, sex of the primary earner is male, and parents are married or have unmarried partner. The covariates in group 2 include and indicator equal to one if the primary earner has strictly positive earnings in the quarter the child enters the labor market. The covariates in group 3 relate to the primary earner and include: categories of tenure (1,2,3,4 quarters, less than 2,3,4 and 5 years), and categorical variables for the decile of earnings rank within the employer. The covariates in group 4 relate to the employer of the primary earner and include: firm age, average log earnings of all workers, log of firm size, and an indicator for the two-digit industry. All variables in categories 4 and 5 are set to zero if the primary earner did not work. All regressions are estimated with sample weights via weighted least squares.

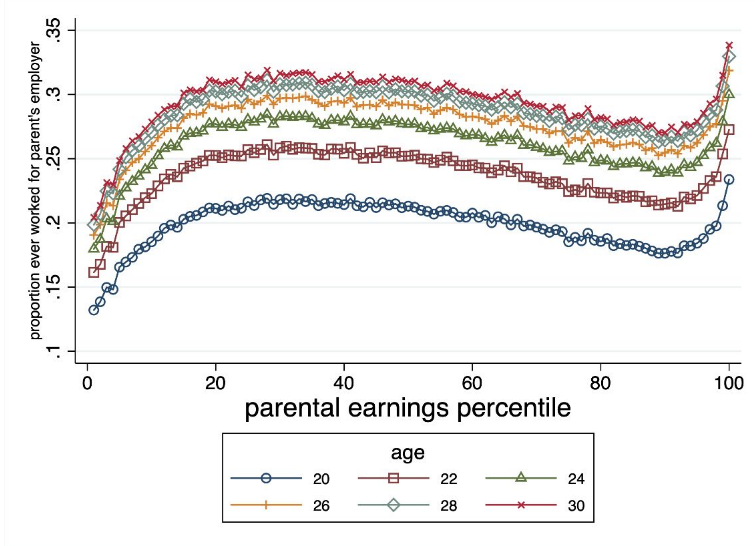
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Figure A.8: Long-Run Measures of the Intergenerational Transmission of Employers

(A) Daughters



(B) Sons



Notes: Each line in the plot the proportion of individuals who have ever worked for an employer of either parent between the ages of 18 and the age indicated in the legend. Each statistic is reported separately by the percentile of the parental earnings distribution. Panels A and B present results for the sample of daughters and sons, respectively. The results are based on a subsample that include children who turned 30 by 2016. All statistics are calculated using sample weights.

Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Table A.5: Control for Changes in Offer Wages

	log of quarterly earnings				
	(1)	(2)	(3)	(4)	(5)
works for parent's employer	0.307***	0.299***	0.312***	0.342***	0.307***
	(0.029)	(0.029)	(0.028)	(0.031)	(0.030)
added covariates	none	new hire earnings	earnings growth of parent	earnings growth of all employees	employment growth
observations	11,460,000	11,460,000	11,460,000	11,460,000	11,460,000

Notes: Each column presents estimates from a separate regression estimated by two-stage least squares. The outcome variable is the log of the first full-quarter of earnings at the first stable job. The endogenous variable is an indicator equal to one if the child works for their parent's employer (primary earner) at the first stable job. The excluded instrument is the average quarterly hiring rate at the parent's employer in the four quarters prior to the quarter in which the individual enters the labor market. Column 1 reproduces the main results and columns 2-5 extend the baseline specification to include controls for the log of average earnings of stable new hires in the year before entry, the average quarterly earnings growth of the primary earner in the year before entry, the average annual earnings growth of all workers in the year before entry, and the average quarterly employment growth rate in the year prior to entry, respectively. All specifications include a fixed effect for the parent's employer; a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry, a fixed effect for the cohort of the child, and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the United States. Standard errors are clustered at the level of parent's employer and are presented in parentheses.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

Table A.6: Household Fixed Effects

	log of quarterly earnings	
	(1)	(2)
works for parent's employer	0.199***	0.155***
	(0.040)	(0.045)
fixed effect	employer	household
control mean	8.757	8.757
observations	4,476,000	4,476,000

Notes: Each column presents estimates from a separate regression estimated by two-stage least squares. The outcome variable is the log of the first full-quarter of earnings at the first stable job. The endogenous variable is an indicator equal to one if the individual works for their parent's employer (primary earner) at the first stable job. The excluded instrument is the average quarterly hiring rate at the parent's employer in the four quarters prior to the quarter of entry. The specification in column 1 includes a fixed effect for the parent's employer whereas the specification in column 2 includes a fixed effect for the parent's employer by household. Both specifications are estimated on the same sample (which drop singleton observations) and include a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry, a fixed effect for the cohort of the child and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the United States. Standard errors are clustered at the level of parent's employer and are presented in parentheses.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

Table A.7: Effect on Timing of Entry

	average in three years prior to entry		
	quarterly earnings (1)	quarters worked (2)	quarter of entry (3)
works for parent's employer	-84.870	-0.066**	-3.973***
	(61.840)	(0.020)	(0.570)
control mean	1,269	0.612	13.170
observations	11,460,000	11,460,000	11,460,000

Notes: Each column presents estimates from a separate regression estimated by two-stage least squares. The outcome variables in columns 1 and 2 are average quarterly earnings and employment in the three years pior to entry, respectively. The outcome variable in column 3 is the quarter of entry relative to the expected quarter of high school graduation (based on birth cohort). The endogenous variable is an indicator equal to one if the child works for their parent's employer (primary earner) at the first stable job. The excluded instrument is the average quarterly hiring rate at the parent's employer in the four quaters prior to the quarter in which the individual enters the labor market. All specifications include a fixed effect for the parent's employer; a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the United States. The specificaitons in columns 1 and 2 also include a fixed effect for the cohort of the child. Standard errors are clustered at the level of parent's employer and are presented in parentheses.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.
*** p≤0.001, ** p≤0.01, * p≤0.05

Table A.8: Robustness to Measuring Hiring Rate at Alternative Times

	log of quarterly earnings								
	(1)	(2)	(3)	(4)	(5)	(6)	(6)	(8)	(9)
works for parent's employer	0.721***	0.590***	0.603***	0.414***	0.307***	0.250***	0.293***	0.347***	0.494***
	(0.129)	(0.102)	(0.074)	(0.053)	(0.029)	(0.013)	(0.013)	(0.015)	(0.023)
quarters used to calculate average hiring rate	[-8,-6]	[-7,-4]	[-6,-3]	[-5,-2]	[-4,-1]	[-3,0]	[-2,1]	[-1,2]	[0,3]
first stage F-statistic	177	275	461	798	1,434	4,308	7,725	5,445	2,219
observations	11,460,000	11,460,000	11,460,000	11,460,000	11,460,000	11,460,000	11,460,000	11,460,000	11,460,000

Notes: Each column presents estimates from a separate regression estimated by two-stage least squares. The outcome variable is the log of the first full-quarter earnings at the first stable job. The endogenous variable is an indicator equal to one if the child works for their parent's employer (primary earner) at the first stable job. In columns 1 through 9 the excluded instrument is the average quarterly hiring rate at the parent's employer in the four quarters prior to 6 quarters before through 3 quarters after the quarter in which the individual enters the labor market. All specifications include a fixed effect for the parent's employer; a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry, a fixed effect for the cohort of the child and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the United States. Standard errors are clustered at the level of parent's employer and are presented in parentheses.

Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

Table A.9: Effect on Industry and Employer Pay Premium

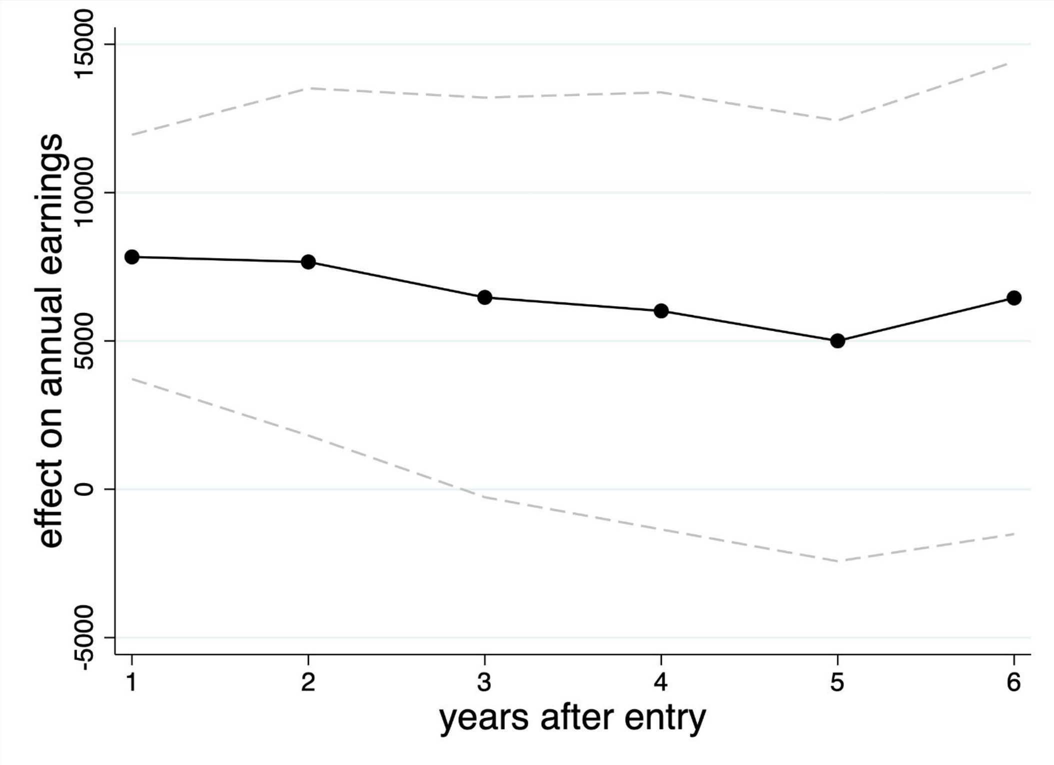
	Industry Pay Premium				Employer Pay Premium
	two-digit	three-digit	four-digit	six-digit	
	(1)	(2)	(3)	(4)	(5)
works for parent's employer	0.167***	0.200***	0.208***	0.230***	0.304***
	(0.013)	(0.015)	(0.016)	(0.016)	(0.024)
control s.d.	0.178	0.208	0.222	0.232	0.366
observations	11,460,000	11,460,000	11,460,000	11,460,000	11,460,000

Notes: Each column presents estimates from a separate regression estimated by two-stage least squares. The outcome variables in columns 1-4 are the estimated pay premiums associated with the two-digit, three-digit, four-digit and six-digit industry codes, respectively. The outcome variable in column 5 is the estimated pay premiums associated with the employer. The endogenous variable is an indicator equal to one if the child works for their parent's employer (primary earner) at the first stable job. The excluded instrument is the average quarterly hiring rate at the parent's employer in the four quarters prior to the quarter in which the individual enters the labor market. All specifications include a fixed effect for the parent's employer; a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry, a fixed effect for the cohort of the child and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the United States. Standard errors are clustered at the level of parent's employer and are presented in parentheses.

Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

Figure A.9: Long-Run Effects



Notes: Each point on the figure represents an estimate from a separate regression. The outcome is the annual earnings x years after entry, where x refers to the coordinate on the horizontal axis. The endogenous variable is an indicator equal to one if the child work for their parent's employer (primary earner) at the first stable job. The excluded instrument is the average quarterly hiring rate at the parent's employer in the four quarters prior to the quarter in which the individual enters the labor market. All specifications include a fixed effect for the parent's employer; a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry, a fixed effect for the cohort of the child and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the U.S. Standard errors are clustered at the level of parent's employer and are used to construct the 95% confidence interval, which is denoted by the dashed lines. All regressions are estimated on a sample of 3,441,000 individuals who are expected to graduate high school in 2004 or earlier and who entered the labor market between the year in which they were expected to graduate high school and six and a half years later. The F-statistic from the first stage is 364.
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Table A.10: Effect of Working for Employer of Secondary Earner

	works for parent's employer	log of quarterly earnings	
	(1)	(2)	(3)
hiring rate	0.071***	0.021***	
	(0.004)	(0.006)	
works for parent's employer			0.291***
			(0.081)
estimator	OLS	OLS	2SLS
F-statistic	365		
mean	0.042		
control mean		8.762	8.762
observations	4,447,000	4,447,000	4,447,000

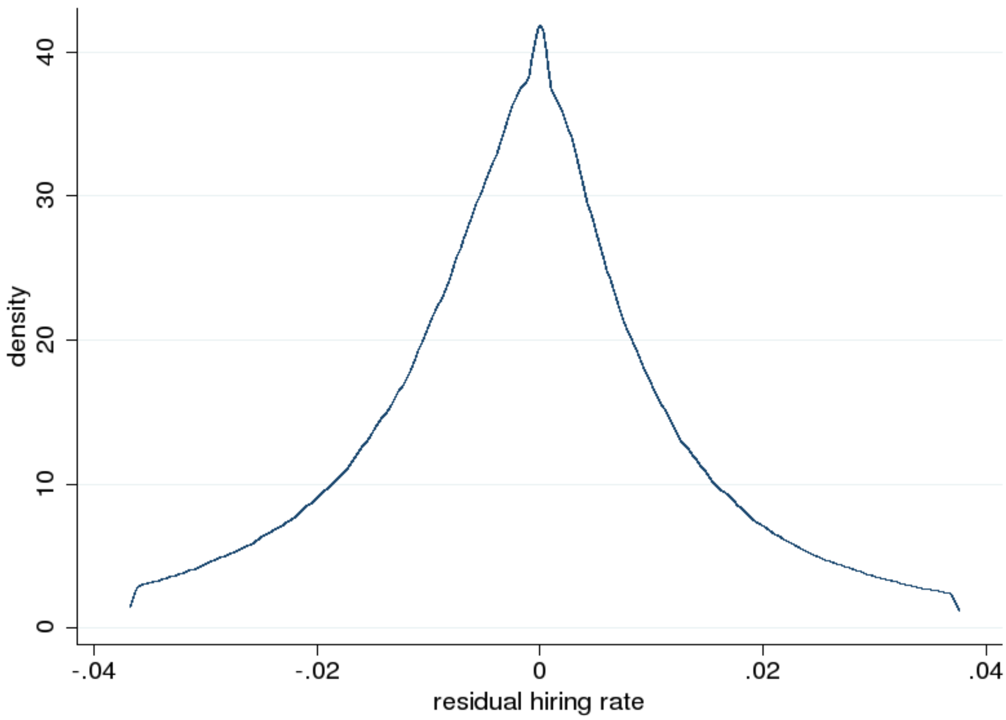
Notes: Each column presents results from a separate regression. The outcome variable in column 1 is an indicator equal to one if the child works for their parent's employer (secondary earner) at their first stable job and the outcome variable in columns 2-4 is the log of the first full-quarter earnings at the first stable job. The main independent variable in column 1 is the average quarterly hiring rate at the parent's employer and the main independent variable in columns 2-4 is an indicator equal to one if the individual worked for the employer of their parent. The results in columns 1-3 are estaimted by Ordinary Least Squares (OLS) and the results in column 4 are estimated by two-stage least squares (2SLS), where the instrument is the average quarterly hiring rate at the parent's employer in the four quarters prior to the quarter in which the child enters the labor market. All specifications include a fixed effect for the parent's employer; a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry, a fixed effect for the cohort of the child and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the United States. Standard errors are clustered at the level of parent's employer and are presented in parentheses.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.
*** p≤0.001, ** p≤0.01, * p≤0.05

Table A.11: Effect of Working for Father's and Mother's Employer

	log of quarterly earnings			
	(1)	(2)	(3)	(4)
works for father's employer	0.522***	0.326***		
	(0.102)	(0.039)		
works for mother's employer			0.281***	0.336***
			(0.056)	(0.075)
sample	daughters	sons	daughters	sons
first stage F-statistic	387.900	760.900	760.900	391.800
observations	3,511,000	3,691,000	3,691,000	4,168,000

Notes: Each column presents results from a separate regression. The outcome variable is the log of the first full-quarter earnings at the first stable job. Columns 1-2 estimate the effect of working for the mother's emloyer and columns 3-4 estimate the effect of working at the father's employer. The main independent variable is an indicator equal to one if the individual worked for the employer of their parent. Each specification is estimated by two-stage least squares, where the excluded instrument is the average quarterly hiring rate at the parent's employer in the four quarters prior to the quarter in which the child enters the labor market. All specifications include a fixed effect for the parent's employer; a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry, a fixed effect for the cohort of the child and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the United States. Standard errors are clustered at the level of parent's employer and are presented in parentheses.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.
*** p≤0.001, ** p≤0.01, * p≤0.05

Figure A.10: Residualized Hiring Rate



Notes: This figure presents the kernel density of the residuals from a regression of the average quarterly hiring rate at the parents' (primary earner) employer in the four quarters prior to entry on a fixed effect for the parents' employer; a fixed effect for the year of entry by two-digit industry code of parents' employer by state of parents' employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry, a fixed effect for the cohort of the child and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the United States. Standard errors are clustered at the level of parents' employer. The distribution is winsorized at the 5^{th} and 95^{th} percentiles according to the Census Bureau's rules.
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Table A.12: Heterogeneity by Residualized Hiring Rate

	log of quarterly earnings		
	(1)	(2)	(3)
works for parent's employer	0.436***	0.310***	0.228*
	(0.048)	(0.029)	(0.114)
estimation sample			
first tercile	X	X	
second tercile	X		X
third tercile		X	X
first stage F-stat	999	1,429	212
observations	7,304,000	7,606,000	7,308,000

Notes: Each column presents estimates from a separate regression estimated by two-stage least squares. The outcome variable is the log of the first full-quarter earnings at the first stable job. The endogenous variable is an indicator equal to one if the child works for their parent's employer (primary earner) at the first stable job. The excluded instrument is the average quarterly hiring rate at the parent's employer in the four quarters prior to the quarter in which the individual enters the labor market. The sample is partitioned into terciles based on the residualized hiring rate. The row below the estimates indicates whether observations from a given tercile are included in the estimation sample. All specifications include a fixed effect for the parent's employer; a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry, a fixed effect for the cohort of the child and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the United States. Standard errors are clustered at the level of parent's employer and are presented in parentheses.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

Table A.13: Characteristics of Compliers

	works for parent's employer		Characteristics of Compliers		
	no	yes	IV(p25)	IV(p50)	IV(p75)
	(1)	(2)	(3)	(4)	(5)
A. Individual					
male	0.50	0.60	0.54	0.54	0.51
White non-Hispanic	0.74	0.74	0.73	0.74	0.74
Black non-Hispanic	0.09	0.08	0.10	0.11	0.10
Asian non-Hispanic	0.03	0.02	0.03	0.03	0.03
Hispanic	0.11	0.13	0.12	0.10	0.10
other	0.03	0.03	0.03	0.03	0.03
born in United States	0.96	0.96	0.97	0.98	0.97
B. Parent and their Employer					
skilled services	0.49	0.38	0.49	0.48	0.66
unskilled services	0.15	0.26	0.20	0.16	0.10
manufacturing/production	0.35	0.36	0.31	0.36	0.25
tenure of parent	23.96	22.63	24.52	25.27	26.71
earnings rank within employer	68.49	77.93	63.97	51.65	65.40
parental earnings rank	55.47	54.40	58.48	66.39	60.33
Sample Size					
proportion of full sample	0.94	0.06	0.04	0.03	0.15

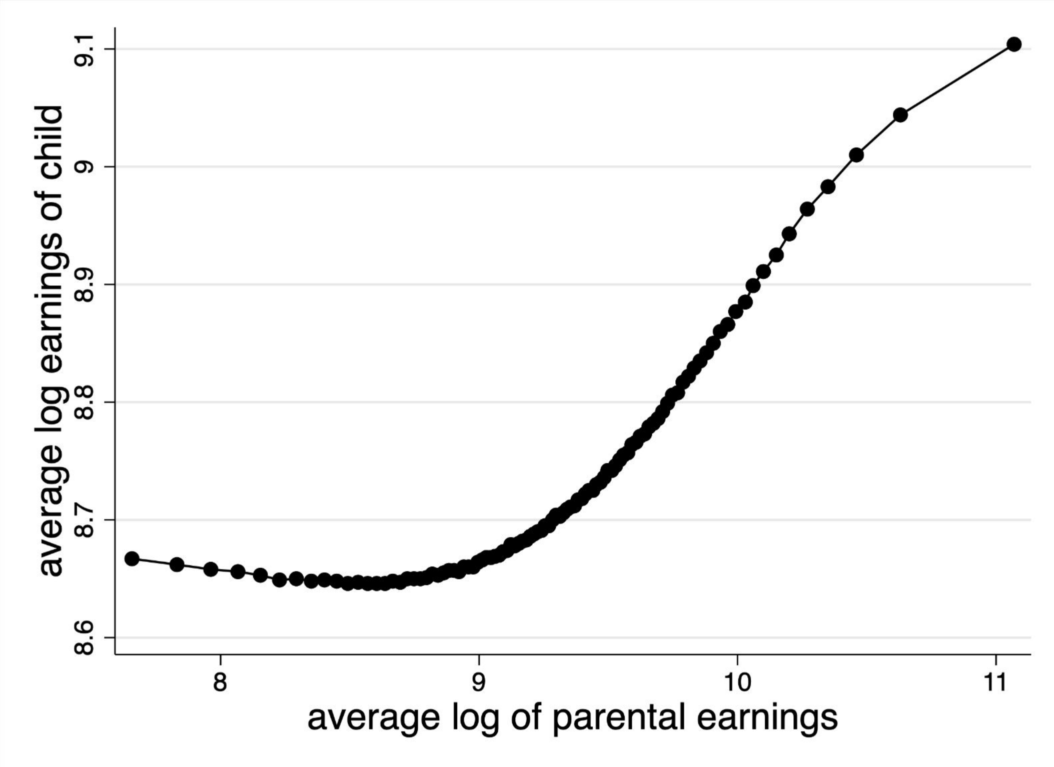
Notes: Each row presents estimates for the variable defined in the first column.Columns 1 and 2 present the average value of the variable for the sample of individuals who do not and do work for the employer of their parent at their first stable job, respectively. Colummns 3-5 present the average characteristics of the compliers for the case in which the instrumental variable is a binary variable equal to one if the residualized hiring rate exceeds the 25th, 50th, and 75th percentile, respectively. The complier characteristics are estimated using the methodology described by Abadie (2003). I winsorize the estimates of κ at the 1st and 99th percentiles to reduce the influence of outlier values. Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Table A.14: Intergenerational Elasticity of Earnings Using Long-Run Earnings

	earnings of child in 2016		
	(1)	(2)	(3)
Panel A. Including Zero Earnings			
log of parental earnings	0.378	0.417	0.396
	(0.002)	(0.002)	(0.001)
sample	daughters	sons	all
observations	8,416,000	8,591,000	17,010,000
Panel B. Excluding Zero Earnings			
log of parental earnings	0.2499	0.2203	0.2348
	(0.0006)	(0.0005)	(0.0004)
sample	daughters	sons	all
observations	7,412,000	7,706,000	15,120,000

Notes: Columns 1 through 3 present results based on a sample of daughters, sons, and all children, respectively. The estimates in Panel A are the coefficients from a regression in which the independent variable is the log of parental earnings and the dependent variable is the inverse hyperbolic sine of the earnings of the child in 2016. The samples used to estimate the regressions in Panel A include children who have zero earnings in 2016. The estimates in Panel B are the coefficients from a regression in which the independent variable is the log of parental earnings and the dependent variable is the log of the earnings of the child in 2016. The samples used to estimate the regressions in Panel B do not include children who have zero earnings in 2016. All regressions are estimated via weighted least squares using sample weights.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Figure A.11: Log Earnings of Parents and Children



Notes: The figure plots the average log earnings of the children against the average log earnings of the parents. Each point represents the average outcome of individuals and their parents for a given percentile of the parental earnings distribution. The horizontal and vertical axes correspond to the average value of log parental earnings and the average value of the log of the first full-quarter of earnings at the first stable job of the child, respectively. All statistics are calculated using sample weights.
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Table A.15: Intergenerational Elasticity of Earnings with Homogeneous Treatment Effects

	sample		
	daughters (1)	sons (2)	all (3)
A. Observed			
IGE	0.1565	0.1298	0.1430
	(0.0002)	(0.0003)	(0.0002)
B. No Transmission with Primary Earner			
percent change in IGE	-0.95%	-1.14%	-1.05%
	(0.09)	(0.11)	(0.10)
C. No Transmission with Either Parent			
percent change in IGE	-1.94%	-2.34%	-2.14%
	(0.18)	(0.22)	(0.20)
observations	8,416,000	8,591,000	17,010,000

Notes: The results in columns 1-3 corresopnd to daughters, sons and all children, respectively. Panel A presents the observed intergenerational elasticity of earnings (IGE), which is denoted $\rho(y_{ijt}, y_p)$ and is estimated with sample weights via weighted least squares. Panels B and C present the percent by which the IGE estimates in Panel A would change if no children were to work for the employer of the parent who is the primary earner or either parent, respectively. The percent change is defined as, $\frac{\rho(y_{ijt},y_p)-\rho(y_{i(j0)t},y_p)}{\rho(y_{ijt},y_p)} \times 100$. The treatment effects used to construct the counterfactual estimates are estimated via two-stage least squares and are estimated for the entire sample, pooling sons and daughters and children from all five quintiles of the parental earnings distrbution. Standard errors are presented in parentheses and are calculated using the delta method and take into account the uncertainty in the estimated earnings consequences.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

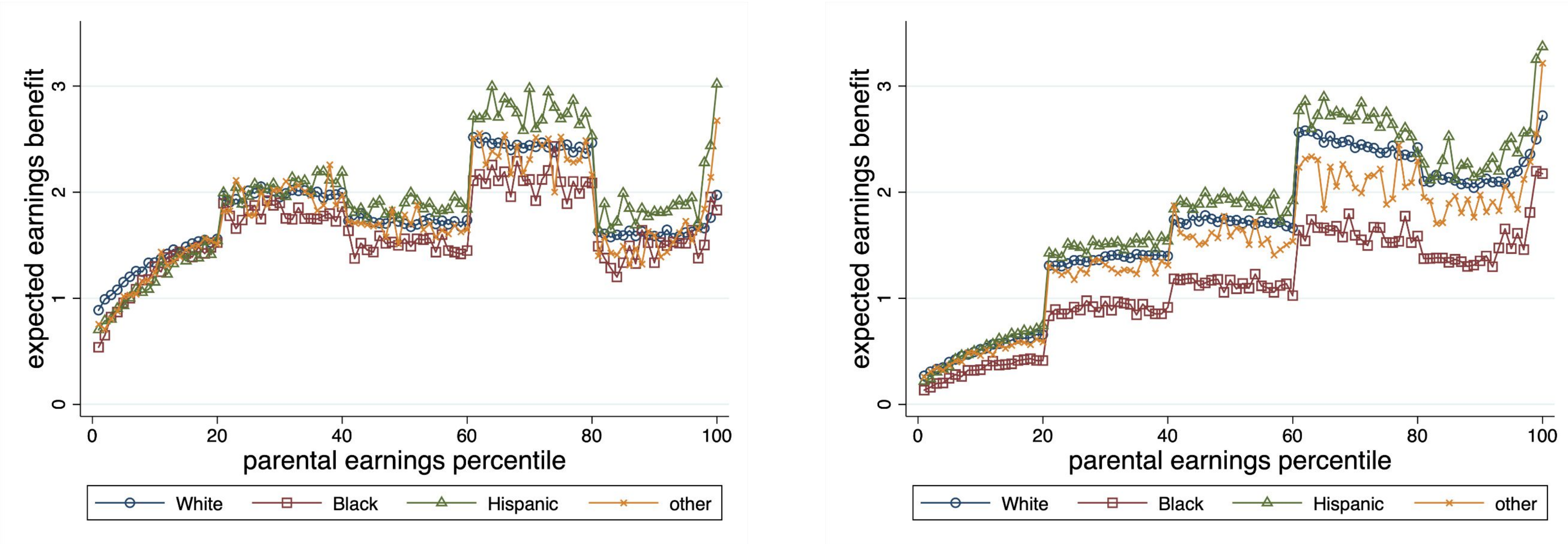
Table A.16: Effect on Earnings Rank by Sex and Parental Earnings

	percentile earnings rank					
	(1)	(2)	(3)	(4)	(5)	(6)
A. All						
works for parent's employer	14.30*	20.64***	15.93***	28.96***	20.63***	21.64***
	(05.88)	(04.70)	(04.42)	(05.34)	(05.46)	(01.97)
F-statistic	238.9	358.0	446.8	330.4	316.2	1434.1
observations	1,350,000	1,987,000	2,297,000	2,462,000	2,487,000	11,460,000
B. Daughters						
works for parent's employer	25.25*	31.27***	27.32*	39.76***	26.58*	31.13***
	(12.49)	(09.68)	(12.06)	(12.55)	(12.30)	(04.02)
F-statistic	64.3	131.2	100.0	106.1	130.9	679.8
observations	586,000	876,000	1,029,000	1,128,000	1,152,000	5,387,000
C. Sons						
works for parent's employer	08.23	15.95**	19.61***	29.59***	27.04***	21.72***
	(08.23)	(06.06)	(05.37)	(06.70)	(07.31)	(02.46)
F-statistic	97.7	198.3	245.7	176.9	161.3	854.2
observations	600,000	909,000	1,067,000	1,149,000	1,148,000	5,501,000
Sample Description						
parental earnings quintile	first	second	third	fourth	fifth	all

Notes: This table presents estimates based on subsamples defined by the interaction between the quintile of parental earnings (defined by the column) and sex (defined by the panel). The outcome varable is the percentile rank of the individual's earnings at their first stable job. The endogenous variable is an indicator equal to one if the individual works for their parent's employer (primary earner) at the first stable job. The excluded instrument is the average quarterly hiring rate at the parent's employer in the four quarters prior to the quarter of entry. All specifications include a fixed effect for the parent's employer; a fixed effect for the year of entry by two-digit industry code of parent's employer by state of parent's employer; and a vector of covariates that includes log annual earnings of the parent in the year prior to entry, a fixed effect for the cohort of the child and interactions between the sex of the child and race, ethnicity, and an indicator equal to one if born in the United States. Standard errors are clustered at the level of parent's employer and are presented in parentheses.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

Figure A.12: Expected Benefits by Sex and Race

(A) Daughters (B) Sons

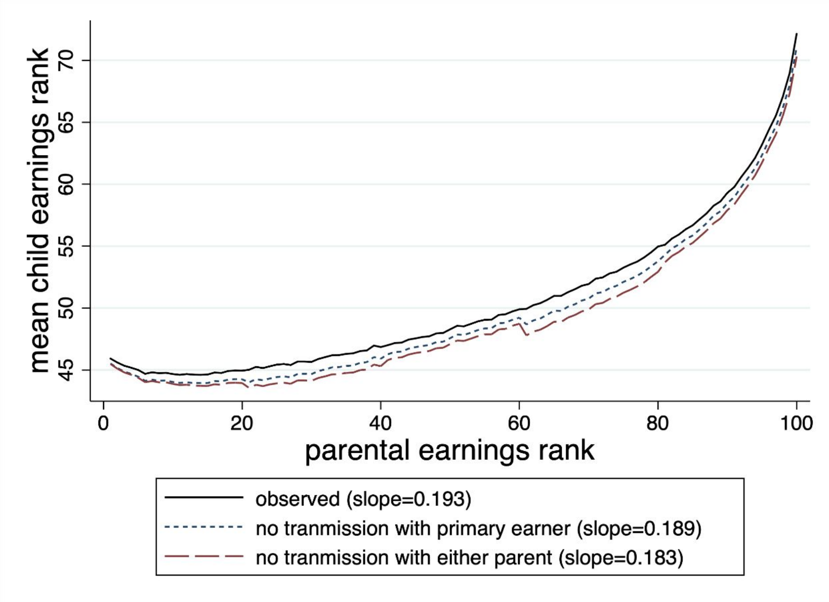


Notes: Each point presents the expected earnings benefits from working for the employer of either parent for the sub group defined by sex (daughters in Panel A and sons in Panel B), parental earnings (defined by the x-axis) and race/ethnicity. The expected earnings benefits are measured as the percentile rank. The treatment effects used to construct the counterfactual estimates are estimated via two-stage least squares and are estimated separately by the quintile of the parental earnings distribution. All statistics, aside from the two-stage least squares estimates, are calculated using sample weights.

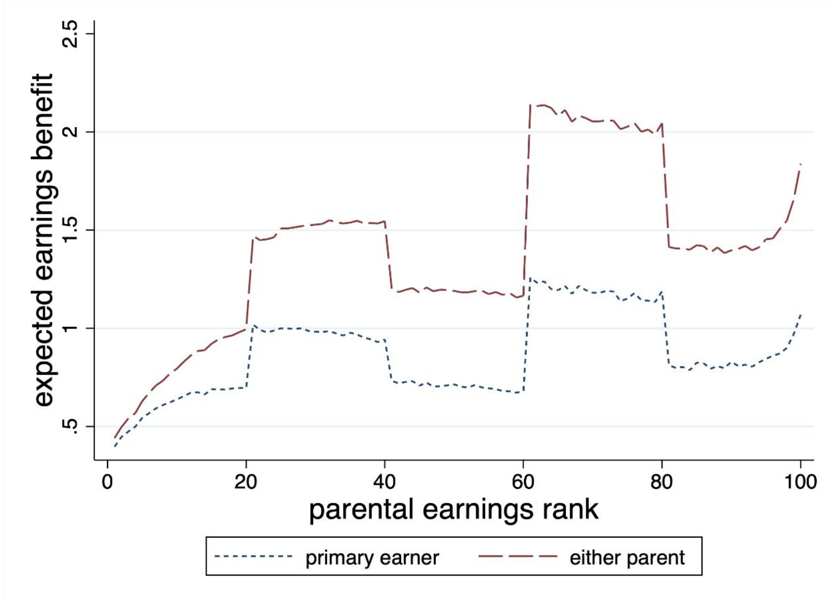
Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Figure A.13: Conditional Expected Rank

(A) Observed and Counterfactural



(B) Difference



Notes: The solid line in Panel A presents the conditional expected rank measure, which is the average percentile rank the of earnings at the first stable job of the child for each percentile of the parental earnings distribution. The dashed lines represent the counterfactual measures that correspond to the two different scenarios in which no individual works for the employer of the primary earner or either parent. Panel B plots the difference between the observed and counterfactual measure for each percentile of the earnings distribution. The treatment effects used to construct the counterfactual estimates are estimated via two-stage least squares and are estimated separately by the quintile of the parental earnings distribution. All statistics, aside from the two-stage least squares estimates, are calculated using sample weights.

Source: Author's calculations based on matched data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Appendix B Details on Data

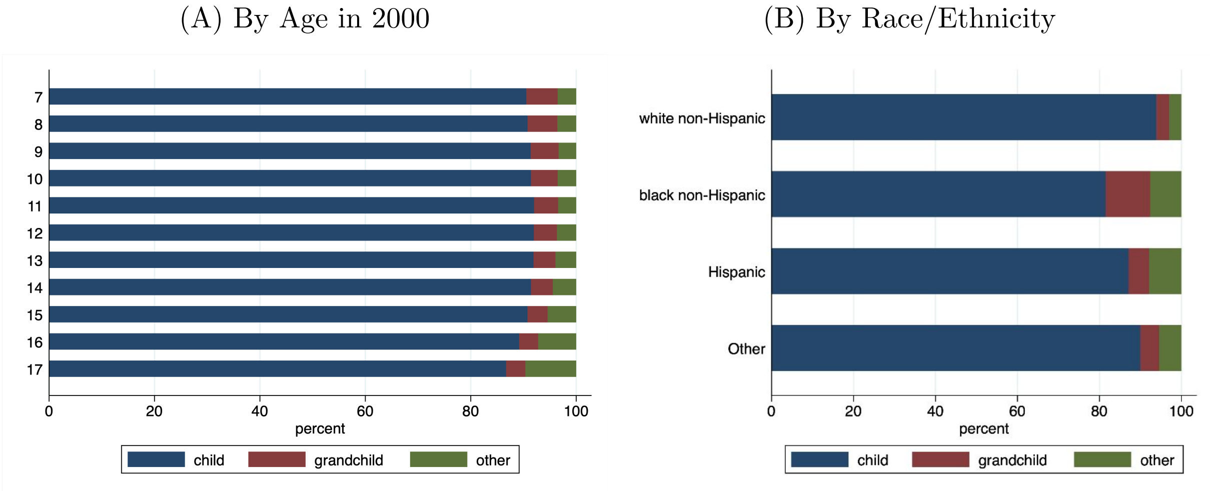
B.1 Sample Frame

The Hundred Percent Census Edited File (HCEF) is an edited version of the Hundred Percent Census Unedited File, which contains all household and person records included in the 2000 Decennial Census. Edits are applied to remove duplicate observations and to ensure consistency between the long and short-form files. While the Decennial Census surveys aim to interview everyone who resides in the United States, in practice, the sample frame considered in my paper does not include all children (within the appropriate age range) living in the United States in 2000. In addition to coverage issues in the 2000 Decennial Census discussed in the text and by Mulry (2007) and the technical report "Coverage Evaluation of Census 2000: Design and Methodology", some children do not live with their parents. Specifically, 91% of individuals younger than 18 lived with their parents in 2000. The remaining 9% individuals will be excluded from my sample since I require that the parent is the head of household.[57]

Panel A and B of Figure B.1 depict the share of individuals whose relationship to the household head is defined as a child by age in 2000 and race/ethnicity, respectively. While my sample frame excludes some individuals for these two reasons, it does include the vast majority of children who fall within the age range. Nevertheless, I point out that the results in this paper aim to be representative of the sample frame and I make no attempts to adjust for additional differences between the sample frame and other populations.

[57]This statistic is based on the authors own calculations using a 5% sample of the 2000 Decennial Census made available through IPUMS, see Ruggles et al. (2019).

Figure B.1: Relationship to Head of Household



Notes: The figures present the proportion of children born between 1982 and 1992 whose relationship to the head of household in the 2000 Decennial Census was defined as: child, grandchild, or other. Panel A breaks out the results by the age of the child at the time of the Decennial Census and Panel B breaks out the results by the race/ethnicity of the child.
Source: Author's calculations based on a 5% sample from the 2000 Decennial Census obtained from IPUMS, see Ruggles et al. (2019).

B.2 Sample Restrictions

I make several key sample restrictions in the move from the sample frame to the analysis sample, all of which are summarized in Table B.1. First, I implement a number of restrictions to ensure that I can accurately link the records of the children from the HCEF to the data from the Longitudinal Employer-Household Dynamics (LEHD) program. Individuals are identified by a Protected Identification Key (PIK), which the Census Bureau generates using personally identifiable information.[58] I use the PIK to link person records between the HCEF and the LEHD and to attach employer characteristics to jobs. Various types of measurement error in the HCEF may prevent a PIK from being accurately assigned to an individual. In order to ensure that each child is accurately assigned a PIK, I require that a unique PIK be assigned to the individual and the year and month of birth recorded in the Individual Characteristic File (ICF) match those recorded in

[58]See Wagner and Layne (2014) for a description of the methodology by which PIKs are assigned to individual observations.

the HCEF.[59] The decision to retain only observations with unique non-missing PIKs and matching year and month of birth between the HCEF and the LEHD is conservative, in the sense that it may drop some individuals who could accurately be linked across the two datasets. The justification for doing this is to limit measurement error in intergenerational relationships, which would arise if PIKs were incorrectly assigned to the child or either parent. While these restrictions reduce sample size, they do not introduce bias to the extent that the sample weights account for the selected nature of the sample. 79% of the children in the sample frame satisfy these restrictions.

Table B.1: Sample Restriction Criteria

	Observations Remaining	
Exclusion Criteria	number	percent
none (sample frame with no restrictions)	37,120,000	100%
child not assigned a unique PIK or the year and month of birth recorded in the HCEF does not match the date of birth in the Social Security Administration transaction file	29,165,000	79%
head of household and spouse (or unmarried partner) is not assigned a unique PIK, the year and month of birth recorded in the HCEF does not match the date in the LEHD or there are more than 15 individuals in the household	23,169,000	62%
the state in which the child resided in began reporting to the LEHD less than a year prior when they are expected to graduate high school or the year child entered the labor market, or if parental earnings is below the 5^{th} percentile	21,321,000	57%
child did not enter the labor market by the end of 2016	17,010,000	46%

Notes: This table describes the sample restrictions applied to the sample frame. The first column describes the criteria and the second column presents the rounded number of observations that remain after dropping the observations that meet the criteria. These numbers represent a cummulative count after the all sample restrictions described in preceding rows are applied. The third column presents this infomration as a percent of the total sample frame.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

[59]The ICF contains a record for every individual that ever appears in the LEHD and contains basic observable characteristics such as race, sex, and date of birth. The primary source for the date of birth variable is the Person Characteristic File (PCF), which is drawn from information recorded from transactions with the Social Security Administration.

Second, I implement a number of restrictions to ensure that I accurately measure the relationship between children and parents and link parental records to the LEHD. To ensure that the relationship between children and parents is accurately measured in the HCEF, I require that the household contains no more than 15 individuals in the HCEF. To ensure that I am able to link the records of the parents to the LEHD files, I require that a unique PIK be assigned to both parents and the year and month of birth recorded in the ICF match those recorded in the HCEF for both parents.[60] 62% of the children in the sample frame satisfy the restrictions in this and the preceding paragraph.

I construct sample weights in order to address the possibility that the first two sample restrictions produce a selected sample. Specifically, using a dataset that includes every child in the sample frame, I estimate the propensity score as the probability of satisfying the first two sample restrictions as a function of observable characteristics that include: sex, relationship to head of household (biological child, adopted child or step child), race (White, Black, Native American, Asian, or other), Hispanic ethnicity, number of parents in the household in 2000, and a vector of observable characteristics of the census tract in which the household resided in at the time of the 2000 Decennial Survey (share of parents that are single parents, median household income, poverty rate, proportion of residents who were living in the same house five years ago, urban/rural, proposition of households receiving public assistance). The sample weights are the inverse of the estimated propensity score.

Third, I implement a set of restrictions to ensure that the measurement of key labor market outcomes are not impacted by coverage issues in the LEHD. Since much of the analysis focuses on the labor market outcomes associated with first stable jobs, I drop children if their first stable job is likely to not be covered in the LEHD. Specifically, I identify the state in which children reside in in the year they are expected to graduate from high school and retain observations only if the state was participating in the LEHD for more than a year prior to that year and the year child entered the labor market. Since an important dimension of the project is to study differences across the parental

[60] Parents are defined as the household head and either their spouse or unmarried partner. Note that edits applied to the HCEF imply that there are at most two parents in each household.

earnings distribution, I also drop parents for whom I cannot reliable measure earnings. Specifically, I construct a long-run measure of parental earnings (discussed in detail in Appendix Section B.4) and I drop parents whose earnings is below the 5^{th} percentile. The percentile is calculated on a dataset with all previously discussed sample restrictions and also conditional on the child entering the labor market. For parents below this threshold, it is difficult to distinguish between low earnings and earnings missed in the LEHD and I find that measures of earnings and other economic indicators (such as the poverty rate of median value in the census tract in which the household lived in 2000) start to diverge for these households. These two sets of restrictions drop an additional 1.9 million children, which leaves 57% of the sample frame.

Lastly, much of the analysis is restricted to a set of children who enter the labor market. I define entry as the first quarter in which the individual earns at least $3,300 per quarter for three consecutive quarters and receives positive earnings from the same employer for those three quarters. 46% of the children in the sample frame satisfy the restrictions in this and the preceding paragraphs.

B.3 Edits to Individual Earnings Records

Earnings data in the LEHD come from Unemployment Insurance (UI) records, which report total amount paid to each worker per employer per quarter. In measuring quarterly earnings, I sum earnings records across employers within a quarter for each individual to construct a measure of total individual earnings per quarter. While the administrative data are not subject to various types of measurement error that plague survey data, they are not error free. A key issue is that data errors can produce very large outlier observations. Researchers typically deal with these by winsorizing the data–editing or dropping earnings records above some percentile of the distribution. The issue with this methodology is that it incorrectly impacts the earnings of workers who truly have earnings in the top percentiles.

In order to retain top earners in my sample, I use an alternative methodology to deal with outliers. The methodology, which I have also employed in Fallick et al. (2019), is based on the fact that outliers often appear in the form of a large spike for a single quarter for an individual. Let $z_i = max\{median(y_{it}), 10000\}$ be the greater of the median of earnings observed for individual i over the entire sample and 10,000.[61] Then define earnings growth as:

$$\Delta_{it} = \frac{y_{it} - z_i}{\frac{1}{2}(y_{it} + z_i)} \tag{B.1}$$

where t is the quarter and y is the earnings. The growth rate, Δ_{it}, captures the extent to which earnings in a given quarter exceeds the typical earnings of that individual. The choice to set a minimum value of z is motivated by the desire to avoid editing the earnings of low earners, since the outliers are driven by very large levels of earnings.

I define outliers as earnings records that produce growth rates that exceed the 95^{th} percentile of the distribution. Let $\Delta(p95)$ denote the 95^{th} percentile, then the earnings variable used in this paper is defined as:

$$\tilde{y}_{it} = \begin{cases} y_{it} & \text{if } \Delta_{it} < \Delta(p95) \\ z_i * \frac{1+\frac{1}{2}\Delta(p95)}{1-\frac{1}{2}\Delta(p95)} & \text{if } \Delta_{it} > \Delta(p95) \end{cases} \tag{B.2}$$

This methodology edits outlier observations so that if the growth rate were calculated on the edited value it would be equal to the 95^{th} percentile. The advantage of this methodology over the traditional winsorization method is that it retains the earnings records of individuals who consistently have high levels of earnings.

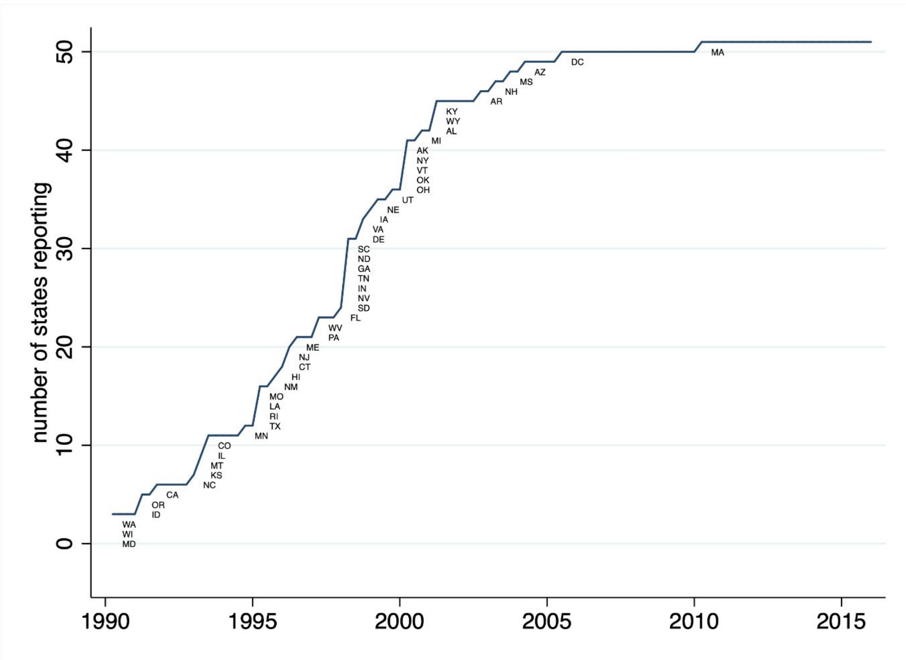
B.4 Measuring Parental Earnings

Parental earnings is an important variable given the focus on intergenerational mobility. The ideal dataset would contain earnings data for each worker over their entire working life, and lifetime earnings would simply be calculated as the sum of all observed earnings. However, the LEHD fall short of the ideal data because some sources of earnings are not included in the data and because they do not cover the full working life of all parents in the sample. Thus, I require an alternative method to estimate lifetime earnings.

[61] The median is calculated from a sample that contains strictly positive earnings.

A common approach in the literature is to calculate parental earnings as the average earnings over a limited number of years. For example, recent work by Chetty et al. (2014) measure parental earnings as the average earnings measured across five years. Even using comprehensive income data derived from the 1040 tax forms, there are various issues with their approach (see Mazumder 2016 for a detailed discussion). The first is related to the number of years over which the earnings are averaged. A large literature inspired by Solon (1992) and Zimmerman (1992) finds that measuring parental earnings over a short time periods introduces measurement error and leads to artificially low estimates of the intergenerational relationship in economic outcomes. Mazumder (2005) suggest that even fifteen years of data may not be enough to accurately measure lifetime earnings. The second issue, is that parental earnings measured at different points in the life cycle may not be comparable (see Jenkins 1987; Solon 1992; Grawe 2006; Bohlmark and Lindquist 2006; Haider and Solon 2006). For example, two individuals aged 35 and 55 might have similar earnings in a given year but very different levels of lifetime earnings.

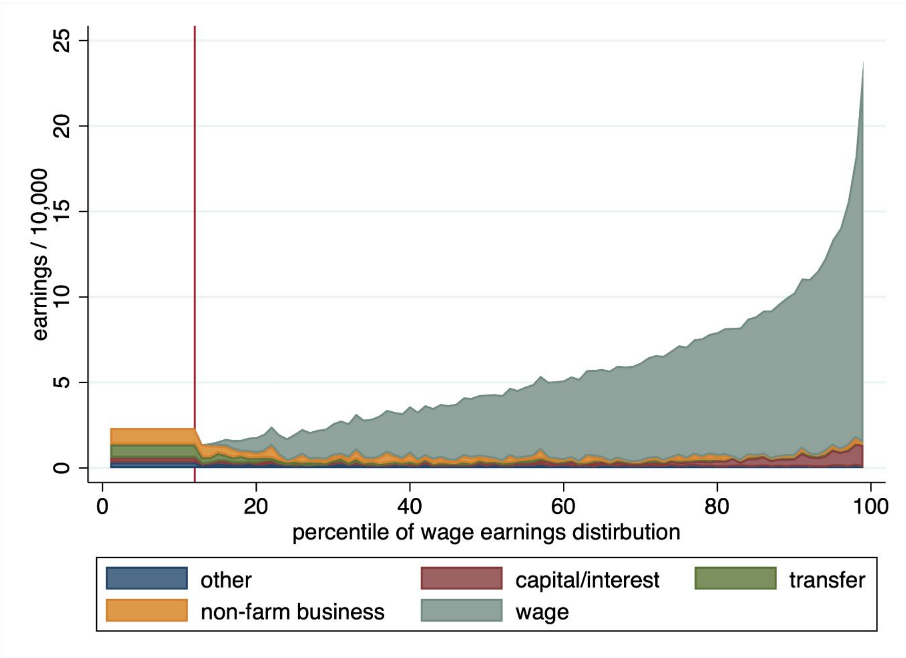
Figure B.2: States Participating in the LEHD Program



Notes: The figure plots the number of states that are reporting to the Longitudinal Household-Employer Dynamics (LEHD) program in a given year. The abbreviations below the solid line represent the states that begin reporting in that year.

There are also a number of additional issues that are specific to the LEHD. The main challenge is that it is not clear how to interpret missing data because it is difficult to distinguish between zero earnings and missing earnings. There are two main reasons why earnings data from the LEHD might be missing for a given individual in a given quarter. First, data availability in the LEHD varies on a state-by-state basis. While all states are currently reporting, coverage is less complete for years further in the past. Figure B.2 illustrates when the different states entered the program. While the residential data in the LEHD can be used to identify whether workers are living in a state that participates in the LEHD, imperfect coverage of these data and workers who commute across state boundaries make it difficult to accurately flag workers whose earnings are missing due to a lack of state reporting.

Figure B.3: Source of Earnings Across the Wage Earnings Distribution



Notes: The figure presents the average household earnings by the percentile of total household wage earnings. Income is broken out into five sources that include: capital/interest, transfer, non-farm business, other and wages. Percentiles below the vertical line have zero wage earnings. The sample includes all households that have at least one child present and excludes the households in the top percentile of the wage earnings distribution due to outlier values.
Source: Author's calculations based on data from the the 2000 March supplement to the Current Population Survey (CPS) and were obtained from IPUMS, see Ruggles et al. (2019).

Second, while most earnings (96% of salary employment) are covered under the UI system, the LEHD systematically misses some sources of earnings. Measurement issues at the bottom of the wage earnings distribution are of particular concern. Figure B.3 demonstrates this point by using data from the CPS to plot average total household income by source against percentiles of parental wage earnings distribution. For most of the distribution, wage earnings (which are accurately measured in the LEHD) are the primary source of both income and earnings. However, this is not true at the bottom of the distribution. Below the vertical line marks the set of households with no wage earnings (12% of household in this sample have no reported wage earnings). Below the 25^{th} percentile, alternative sources of income start becoming an increasingly more important source of total household income, so much so that households with zero reported wage earnings actually have higher average total income relative to households who have positive, but little, wage earnings. Most importantly, since my focus is on earnings, self-employment (not captured in the LEHD) is a main source of earnings for parents at the bottom of the wage earnings distribution. Wage earnings is the primary source of income for households with total income (as opposed to total wage earnings) that is above the 10th percentile. The same is not true for households with income below the 10th percentile, for whom transfer income is relatively more important. While Figure B.3 seems to indicate that wage earnings represent the primary source of earnings at the top of the distribution, Smith et al. (2019) find that non-wage earnings become increasingly important in the top 1% of earners. Taken together, the measure of parental earnings constructed using earnings data from the LEHD should be seen as representative of working families, which excludes roughly the bottom 10% and top 1% of earners.

In order to address the measurement issues in the LEHD, I use an estimation procedure that leverages all of the available data. In particular, I estimate the following regression:

$$y_{it} = \alpha_i + \beta^g X_{it} + u_{it} \tag{B.3}$$

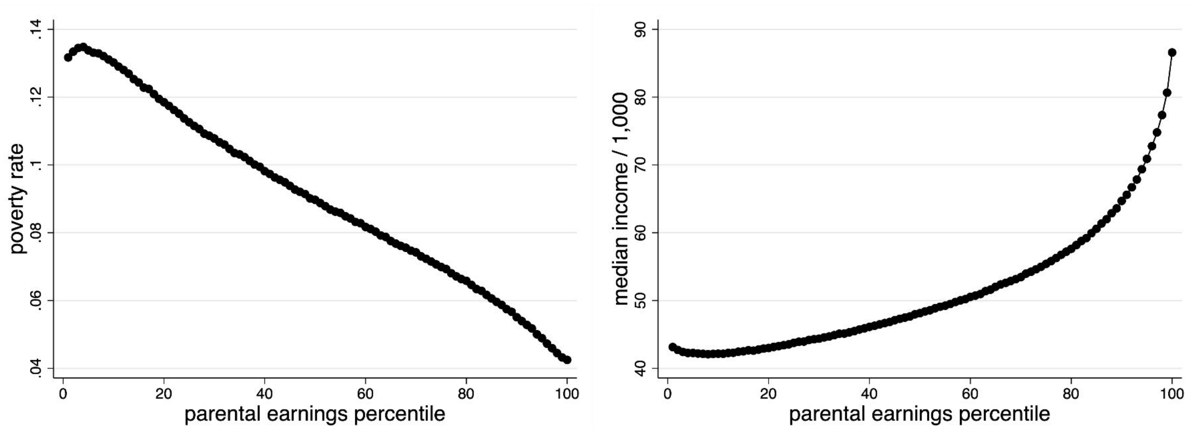
where is is the individual, t is the quarter, y is total quarterly earnings, α is an individual fixed effect and X is vector that consists of a third order polynomial in age. To allow for a flexible age earnings profile, I estimate this specification separately for groups, g, defined by the interaction between sex, race/ethnicity (White non-Hispanic, Black non-Hispanic, Asian non-Hispanic, Hispanic, and other), and state of residence in 2000. The data are a panel that include all strictly positive earnings records between 2000 and 2016 for the parents in the sample. I further restrict the panel to individuals between the ages of 30 and 60 and drop individuals that have fewer than 4 quarters of strictly positive earnings over the entire time period.

I use the estimates from this model to construct a measure of lifetime earnings for each parent. I predict the value of earnings for each quarter between the ages of 35 and 55 and define lifetime earnings as the average of these values. Individuals with either missing or negative values are assigned a lifetime earnings of zero. For single-headed households parental earnings is simply the lifetime earnings of the parent. For two-parent households, parental earnings is the average of the lifetime earnings of both parents.[62]

Figure B.4: Parental Earnings and Neighborhood Characteristics

(A) Poverty

(B) Median Income



Notes: The figure plots the average characteristic of the census block group of residence in 2000 for each percentile of the parental earnings distribution. The characteristics in Panel A and B are poverty rate and median income, respectively.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Much of the analysis relies on percentile ranks of parental earnings. Thus, it is critical that the estimates of lifetime earnings preserve the rank of the true values of lifetime earnings. While I do not have an objective measure of lifetime earnings against which to validate my measure, I do have other proxies. In particular, I use the HCEF to identify

[62]The choice to take the average earnings across parents is in line with the assumptions made by Chetty et al. (2014).

the census block group in which all households reside in 2000 and measure characteristics of those neighborhoods. I focus on poverty rate and median income, since these are likely to be correlated with lifetime earnings. Figure B.4 plots the average value of these neighborhood level variables against the percentile of the lifetime earnings distribution (percentiles are calculated within cohorts of children). If all measures are proxies of lifetime earnings then there should be a monotonic relationship between the variables. The figure illustrates that this is true for most of the distribution. The one exception is that very bottom of the distribution, where parental earnings may be measured with more error. But overall, the figure indicates a strong relationship between the measure of parental earnings used in this paper and other measures of economic status and thus should alleviate concerns related to measurement error.

If the imputed measure of parental earnings is a multiple of the true lifetime earnings value, then the estimates of IGE will be unaffected. However, if the error is not multiplicative, or differs across individuals, then measurement error may affect the estimates of IGE. A main concern is that my measure is unable to account for differences in labor force participation. By failing to account for periods of nonemployment, my measure will produce artificially high levels of lifetime earnings for parents who have many periods of zero earnings. This may reduce the elasticity of the initial earnings of a child with respect to parental earnings in the lower parts of the distribution. For this reason, it is useful to compare the results with elasticities to those using percentiles. It is worth pointing out that the issue of measuring the earnings of low-income households is not unique to my setting. For example, Chetty et al. (2014) find that their estimates of the IGE are sensitive to the inclusion of the households below the 10^{th} percentile.

B.5 Grouping Industries into Sectors

I group two-digit North American Industry Classification System (NAICS) industry codes into three distinct sectors, which are defined below. The unskilled service sector includes: retail trade (44,45); administrative and support and waste management and remediation services (56); arts, entertainment and recreation (71); accommodation and food services

(72); and other services (81). The skilled service sector includes: information (51); finance and insurance (52); real estate and rental and leasing (53); profession, scientific and technical services (54); management of companies and enterprises (55); educational services (61); health care and social assistance (62); and public administration (92). The manufacturing/production sector includes: agriculture, forestry, fishing and hunting (11); mining, quarrying, and oil and gas extraction (21); utilities (22); construction (23); manufacturing (31,32,33); wholesale trade (42); and transportation and warehousing (48,49).

B.6 Employer and Industry Pay Premiums

In order to estimate the earnings-premium associated with specific employers, I use the methodology developed by Abowd et al. (1999), or commonly referred to as the AKM model. Specifically, I estimate the following specification,

$$y_{it} = \alpha_i + \Psi_{j(i,t)} + X_{it}\beta + \epsilon_{it} \tag{B.4}$$

where i is the individual; t is the year; y is the log of average quarterly earnings; X_{it} is a vector of time varying controls that include a fixed effect for the year and a third order polynomial in age interacted with sex and education; α_i is an individual fixed effect; $\Psi_{j(i,t)}$ is a fixed effect for the employer of i in time t; and ϵ_{it} is a regression residual.[63] The estimate, $\hat{\Psi}_{j(i,t)}$, is a time-invariant measure of the employer pay premium (measured in quarterly earnings).

I estimate this specification using a national sample that includes all earnings records from the LEHD measured between the years 2000 and 2016 and workers between the ages of 25 and 40. I retain jobs that provide over half of the earnings for that year and calculate quarterly earnings as the average of full-quarter earnings for a given employer within the

[63]Identification of the age and time effects in the presence of individual fixed effects is achieved by following Card et al. (2013) and omitting the linear age term in for each sex by education group and using a cubic polynomial in age minus 40. This normalization assumes that the age-earnings profile is flat at age 40. While the normalization affects the estimates of the individual fixed effects and the covariate index $X_{it}\beta$, the employer fixed effects are invariant to the normalization used. Data on education comes from the individual characteristics file and is sourced from various surveys and is imputed for many observations.

year.[64] Due to computational constraints, I estimate the specification separately within 15 mutually exclusive samples defined by the 9 census divisions and the six largest states (CA, TX, FL, NY, PA, IL). As is standard in the literature, I restrict the sample to the largest connected set within each of these samples. In order to account for the fact that the level of firm pay premiums are not comparable across estimates from distinct samples, I follow Gerard et al. (2018) and normalize all employer fixed effects by subtracting the mean value of the fixed effect for employers in the accommodation and food services industry. Intuitively, this normalization assumes that employers in this industry offer a pay premium of zero, on average.

I am unable to compute the employer pay premium for employers that lie outside of the largest connected set within each of the 15 mutually exclusive samples. In practice this happens in a very small fraction of cases. In order to avoid disclosure issues related to releasing results on multiple samples, I impute missing data with the mean value of individuals who do not work at the employer of a parent and include a control for imputed values in the empirical specification.

I estimate the industry-level premium using the same data and methodology except I replace the employer fixed effect with a fixed effect for the industry code. Because all industries are connected through worker mobility, estimation is performed on the national sample but to ease computational burden, I take a random 10% subsample of workers. I am able to estimate an industry-level pay premium for all industries, and thus there are no missing data for this variable.

B.7 Employer- and Firm-Level Variables

B.7.1 Poaching Hires

For each employer I calculate the share of new stable hires that are acquired through poaching flows as opposed to nonemployment flows. In order to explain how poaching rates are constructed, it is useful to establish the following terminology. Each worker with

[64] Outliers in the earnings data are dealt with using the same methodology described in Appendix Section B.3.

positive earnings in quarter t can have one of four types of employment spells defined in Table B.2, where "+" denotes positive earnings and "0" denotes zero earnings at the employer at quarter t.

Table B.2: Classification of Employment Spells

	earnings at employer		
	t-1	t	t+1
beginning of quarter	+	+	0
end of quarter	0	+	+
middle of quarter	0	+	0
full quarter	+	+	+

A worker with a beginning of quarter employment spell is relatively attached to the employer at the start of quarter t but separates from the employer at some point during quarter t. Similarly, a work with an end of quarter employment spell joins the employer at some point during quarter t and experiences a stable spell of employment that continues into the following quarter. Middle of quarter employment spells represent spells that begin and end within the quarter and, following the conventions used to construct the Job-to-Job Flows statistics, I do not use them when constructing poaching rates.

Workers who experience an end of quarter employment spell in quarter t are defined as stable new hires. These workers begin their employment spell at some point during quarter t, and I define the hire as a poaching hire if the worker also left their previous employer in quarter t. In other words, a poaching hire is an individual who switches employers and begins their new job no later than one quarter after leaving their old job. In practice, I identify poaching hires as individuals who experience an end of quarter employment spell in quarter t and experience either a full quarter or end of quarter employment spell (at a different employer) in quarter t-1. All stable new hires that do not meet these criteria are defined as hires from nonemployment.

For each employer, I calculate the total number of stable hires made through poaching and nonemployment flows between 2000 and 2016. I then calculate an employer-level poaching rate as the proportion of stable new hires made through poaching flows over the entire period. Lastly, I rank employers from 0 to 100 based on their poaching hire rate, where the ranks are calculated using average employer size as weights.

A small fraction of employers have insufficient observations to calculate this measure. In order to avoid disclosure issues related to releasing results on multiple samples, I impute missing data with the mean value of individuals who do not work at the employer of a parent and include a control for imputed values in the empirical specification.

B.7.2 Average Earnings

I calculate average earnings at the employer using full quarter employment spells. Specifically, using data between 2000 and 2016, I retain all workers who experience a full quarter employment spell and take the log of their earnings (I top code earnings at $1,000,000 to mitigate the impact of outliers). The employer-level average of log earnings is simply the average of the quarterly earnings records. I rank employers from 0 to 100 based on their average log earnings, where the ranks are calculated using average employer size as weights. There are no missing data for any of the employers in the sample.

B.7.3 Productivity

The firm-level measure of productivity is based on data from the Revenue Enhanced Longitudinal Business Database (RE-LBD). The RE-LBD supplements the LBD with revenue data from the Census Business Registrar (BR). The BR contains annual measures of revenue measured at the tax reporting or employer identification number (EIN) level. Haltwanger et al. (2016) describe how the revenue data and the employment data from the LBD are combined to construct firm level measures of log revenue per worker, which represent the measure of productivity.

There are two limitations of this particular measure of productivity. First, the coverage is not universal since the employment and revenue data for some firms cannot be linked and since the coverage excludes non-profit firms and firms in the Agriculture, Forestry, Fishing and Hunting (NAICS=11) and Public Administration (NAICS=92) industries. Haltwanger et al. (2016) show that the revenue data cover about 80% of firms in the LBD and patterns of missing productivity data are only weakly related to observable firm characteristics. Second, the revenue per worker measure fails to account for differences in intermediate inputs across industries, which imply that this measure cannot be used to compare productivity of firms that are located in different industries.

In order to overcome the latter limitation, I follow Haltiwanger et al. (2017) and construct a time invariant measure of productivity. Specifically, after attaching firm productivity to the employer-level dataset, I calculate average productivity for each employer as the employment-weighted average of log revenue per worker observed across all periods. From each employer I then subtract the employment-weighted average of productivity at the level of the four-digit NAICS industry code. Thus, this measure of productivity is a time invariant measure that captures the productivity of an employer relative to other employers in the same industry. Productivity ranks that range from 0 to 100 are calculated within four-digit industry codes and are employment weighted, where employment refers to the average number of employees at the employer observed over the sample period.

B.7.4 Firm Age and Size

Measures of firm age and firm size are derived from the Longitudinal Business Database (LBD).[65] The LBD is an annual dataset that covers the universe of establishments and firms in the US non-farm business sector with at least one paid employee. Establishment-level employment is measured as the number of workers on payroll in the pay-period that covers the 12th day of March in the previous year. Firm size is simply the sum of employment at all establishments within the firm. Firm age measures the number of years since the firms formation and accounts for changes in firm identifiers as well as mergers and acquisitions.[66]

[65]See Jarmin and Miranda (2002) for a detailed description of the LBD and Haltiwanger et al. (2014) for a description of how firm-level outcomes from the LBD are linked to the employers int he LEHD.

[66]See Davis et al. (2007) for a detailed description of how the firm age variable is constructed.

B.8 References

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Appendix C Approximation Methodology

By definition, $\text{cov}(D_{it}\beta_{it}, y_p) = \mathbb{E}[D_{it}\beta_{it}y_p] - \mathbb{E}[D_{it}\beta_{it}]\mathbb{E}[y_p]$. By iterated expectations,

$$\mathbb{E}[D_{it}\beta_{it}] = \mathbb{E}\big[\mathbb{E}[D_{it}\beta_{it}|D_{it}]\big] = \mathbb{E}[D_{it}]\mathbb{E}[\beta_{it}|D_{it} = 1] \tag{C.1}$$

and

$$\mathbb{E}[D_{it}\beta_{it}y_p] = \mathbb{E}\big[\mathbb{E}[D_{it}\beta_{it}y_p|r_p]\big] \tag{C.2}$$

where r_p is the percentile rank of parental earnings. Because the Pearson correlation coefficient is bounded between -1 and 1, it follows that,

$$cov(D_{it}\beta_{it}, y_p|r_p)^2 \leq var(D_{it}\beta_{it}|r_p) \times var(y_p|r_p) \tag{C.3}$$

In practice, I condition on r_p, but one could think to condition on more detailed ranks. As the number of ranks approaches the sample size, $var(y_p|r_p)$ approaches zero and the covariance term therefore approaches zero. Thus,

$$\begin{aligned}\mathbb{E}[y_p D_{it}\beta_{it}|r_p] &= \mathbb{E}[y_p|r_p] \times \mathbb{E}[D_{it}\beta_{it}|r_p] + cov(D_{it}\beta_{it}, y_p|r_p) \\ &\approx \mathbb{E}[y_p|r_p] \times \mathbb{E}[D_{it}\beta_{it}|r_p]\end{aligned} \tag{C.4}$$

where equation C.3 suggests that $cov(D_{it}\beta_{it}, y_p|r_p)$ will be close to zero when conditioned on parental earnings ranks that are defined at a sufficiently high level of detail. Combing these pieces yields the approximation in equation 8.

I assess the performance of the approximation methodology by using the same methodology to approximate the observed IGE. By definition, $\rho(y_{ijt}, y_p) = \frac{\text{cov}(y_{ijt}, y_p)}{\text{var}(y_p)}$. The variance term, $\text{var}(y_p)$, is directly observed and I use the following approximation for the covariance term,

$$\text{cov}(y_{ijt}, y_p) \approx \mathbb{E}\Bigg[\mathbb{E}[y_p|r_p] \times \mathbb{E}[y_{ijt}|r_p]\Bigg] - E[y_p] \times E[y_{ijt}] \tag{C.5}$$

Where this approximation relies on the same assumption used to derive equation 8.

Table C.1 compares the estimates of the IGE from the micro data, in Panel A, to the approximated values, in Panel B. The approximated values are virtually identical to the actual values, which suggests that the methodology performs well in this context.

Table C.1: Approximation of the Intergenerational Elasticity of Earnings

	(1)	(2)	(3)
A. Individual-Level Data			
$\rho(Y_i, Y_{p(i)})$	0.157	0.130	0.143
B. Approximation			
$\rho(Y_i, Y_{p(i)})$	0.155	0.131	0.143
sample	daughters	sons	all

Notes: The results in columns 1-3 corresopnd to daughters, sons and all children, respectively. Panel A presents the estimated coefficient from a regression of the log of the first full-quarter of earnings at the first job of the child on the log of parental earnings. The regression is estimated via weighted least squares with sample weights applied. Panel B presents the approximations of the values in Panel A.
Source: Author's calculations based on data from the Longitudinal Employer-Household Dynamics and 2000 Decennial Census files.

Standard errors for the counterfactual estimates in Table 6 are estimated via the delta method. Specifically, let

$$\begin{aligned}\Gamma(\vec{B}) &= \frac{\rho(y_{ijt}, y_p) - \rho(y_{ij(0)t}, y_p)}{\rho(y_{ijt}, y_p)} \times 100 \\ &= \left(\frac{100}{\rho(y_{ijt}, y_p) var(y_p)}\right) \sum_{q=1}^{5} \hat{\beta}^q \left[\frac{1}{100}\left(\sum_{k=(q-1)*20+1}^{q*20} \mathbb{E}[y_p|r_p = k]\mathbb{E}[D_{it}|r_p = k]\right) - \mathbb{E}[y_p]\mathbb{E}[D_{it}]/5\right]\end{aligned} \tag{C.6}$$

where $\vec{B} = [\hat{\beta}^1, \hat{\beta}^2, \hat{\beta}^3, \hat{\beta}^4, \hat{\beta}^5]$ is a 1×5 vector where the components are the effects conditional on parental earnings for the five parental earnings quintiles. Then we have,

$$\frac{\partial \Gamma(\vec{B})}{\partial \beta^q} = \frac{100}{\rho(y_{ijt}, y_p) var(y_p)} \times \left[\frac{1}{100} \sum_{k=(q-1)*20+1}^{q \times 20} \big(E[y_p|r_p = k]E\big[D_{it}|r_p = k\big]\big) - E[y_p]E[D_{it}]/5\right] \tag{C.7}$$

Assuming independence between the β^k estimates, leads to the following expression by the delta method,

$$se(\Gamma(\vec{B})) = \sum_{k=1}^{5} var(\beta^k) \times \left[\frac{\partial \Gamma(\vec{B})}{\partial \beta^k}\right]^2 \tag{C.8}$$

where $var(\beta^k)$ is simply the square of the standard error from Table 5.

Appendix D Stylized Model

This section synthesizes my findings by developing a stylized model that describes how the intergenerational transmission of employers affects intergenerational mobility. Relative to other models of intergenerational mobility, the novel features of my model are that I: (1) incorporate a employer-specific component into individual earnings and (2) explicitly model the choices that lead individuals to work for a parent's employer. The key insights from the model include:

1. parents affect the earnings of their children not only by shaping the development of their human capital, but also by providing access to higher-paying employers;

2. if working at the parent's employer is determined by choices made by the employer and the child, then there are conditions under which the instrumental variables estimator identifies the average treatment effect for the population that works for their parents employer, even in the presence of selection bias and selection on gains;

3. the effect of the intergenerational transmission of employers on intergenerational mobility is theoretically ambiguous;

4. if parents adjust their investments in the human capital of their children based on their expectations of whether the child will work for their employer, this could either amplify or dampen the implications for intergenerational mobility.

Let y_{ij} denote the log earnings of individual i at employer j.[67] Assume that log earnings are additive in the log of the human capital (h_i), the employer pay premium (f_j), and an idiosyncratic error terms (u_i). Thus,

$$y_{ij} = h_i + f_j + u_i \tag{D.1}$$

The individual component is defined independent of where the individual is employed and employer transmission affects earnings entirely through its effect on the employer pay premium.

[67] To simplify exposition, I drop the time subscripts that are used in the main text.

Using the notation of the potential outcomes framework, let $j(1)$ denote the parent's employer and let $j(0)$ denote the employer that represents the outside option. The employer pay premium can be written as,

$$f_j = f_{j(0)} + D_i \beta_i \tag{D.2}$$

where D_i is an indicator equal to one if the individual works for their parent's employer and zero otherwise and $\beta_i = f_{j(1)} - f_{j(0)}$ is the effect of working for a parent's employer.

An individual's outside option is related to their human capital. Specifically, the labor market exhibits sorting between workers and firms, characterized by the following equation:

$$f_{j(0)} = \lambda h_i + \nu_i \tag{D.3}$$

where ν_i is an idiosyncratic error term and $\lambda > 0$ indicates that individuals with higher levels of human capital tend to match to employers that offer higher pay premiums. The same matching process applies to parents, but I abstract from the possibility that parents might work for the employers of their parents.[68] Furthermore, the relationship between the human capital of the child and earnings of the parent is characterized by,

$$h_i = x + \theta y_{pj(1)} + \eta_i \tag{D.4}$$

where p denotes the parent of i, η_i is an idiosyncratic error term and $\theta > 0$ implies that human capital is increasing in parental earnings.

Whether a child works for the employer of their parent depends on choices made by both the employer and the child. Let O_i be equal to one if the parents' employer makes a job offer to the child and zero otherwise. The offer decision depends on the instrument, $z_i \in \{z', z''\}$ with $z' > 0 > z''$, and the human capital of the parent and the child. Specifically, $O_i = \mathbb{1}\{\phi h_p + \gamma h_i > z_i\}$, where ϕ and γ could be positive or negative.[69] Let

[68] Formally, I assume that $D_p = 0$, where p denotes the parent of i. This assumption simplifies the analysis and allows me to write the earnings benefits associated with working for the parent's employer as function of parental earnings and unobserved error terms $\beta_i = (\frac{\lambda}{1+\lambda} - \lambda\theta) y_{pj(1)} + [\lambda/(1+\lambda)](\lambda\nu_p - u_{pj(1)}) - [\lambda x + \lambda\eta_i + \nu_i]$.

[69] ϕ might be positive if higher-ability parents have more control over the hiring process because they

A_i be equal to one if the child would accept a job offer from the parent's firm. The child will choose to accept the offer if the earnings gains, β_i, exceed any costs, c, such that $A_i = \mathbb{1}\{\beta_i > c\}$. The child will work with their parent only if they receive a job offer and it is optimal for them to accept,

$$D_i = \mathbb{1}\{\phi h_p + \gamma h_i > z_i\} \times \mathbb{1}\{\beta_i > c\} \tag{D.5}$$

Unlike the standard selection models, equation D.5 illustrates that selection into treatment depends on the choices of multiple agents.

Combining equations D.1, D.2, D.3 and D.4 yields the following relationship between the earnings of the child, the earnings of the parent and the effect of the transmission of employers,

$$y_{ij} = \alpha_1 + \alpha_2 y_{pj(1)} + D_i\beta_i + \epsilon_i \tag{D.6}$$

where $\epsilon_i = \nu_i + (1+\lambda)\eta_i + u_i$ is an unobserved error term, and where $\alpha_1 = (1+\lambda)x$ and $\alpha_2 = (1+\lambda)\theta$. Equation D.5 illustrates that D_i is related to ϵ_i through the unobserved error terms, implying that estimating equation D.6 via OLS will produce biased estimates with a sign that is theoretically ambiguous.[70]

Under the assumption that the instrument is orthogonal to the unobserved components of the individual's earnings ($z_i \perp\!\!\!\perp \eta_i, \nu_i, u_i$) and parent's earnings ($z_i \perp\!\!\!\perp \nu_p, u_p$), an instrumental variables estimator that uses z_i as an instrument identifies a local average treatment effect (LATE), which is defined as $\mathbb{E}[\beta_i | D_i(z') < D_i(z'')]$. In the standard one-agent selection framework the LATE will depend on the value of the instruments since the decision-making process directly links the benefits and instruments.

In my context, in which selection into treatment is determined by two agents, this link is potentially broken. The implication is stated in the following proposition,

hold leadership positions, or negative if lower-ability parents work at firms that rely more heavily on networks in the hiring process. γ may be positive if firms are more likely to make a job offer to high ability workers, or negative if parents exert more effort to procure job opportunities for low ability children.

[70] To more clearly see the relationship between D_i and ϵ_i note that the offer and acceptance decisions can be re-written as: $O_i = \mathbb{1}\{(\frac{\phi}{1+\lambda} + \gamma\theta)y_{pj(1)} + \gamma x - \frac{\phi}{1+\lambda}(\nu_p + u_p) + \gamma(x + \eta_i) > z_i\}$ and $A_i = \mathbb{1}\{(\frac{\lambda}{1+\lambda} - \lambda\theta)y_{pj(1)} + (\frac{\lambda}{(1+\lambda)})(\nu_p/\lambda - u_p) > c + \lambda x + \lambda\eta_i + \nu_i\}$. See Appendix Section D.2 for details.

Proposition 1 *If $\phi = 0$ and $\gamma = 0$, then $O_i \perp\!\!\!\perp \beta_i$ and*

$$\underbrace{E[\beta_i | D_i = 1]}_{ATT} = \underbrace{E[\beta_i | D_i(z') < D_i(z'')]}_{LATE} \tag{D.7}$$

Proof 1 *If $\gamma = 0$ and $\phi = 0$ then $O_i = \mathbb{1}\{0 > z_i\}$ and it follows that $O_i \perp\!\!\!\perp \beta_i$. For any two values of the instrument, $z' > 0 > z''$, it follows that,*

$$\begin{aligned} \underbrace{E[\beta_i | D_i = 1]}_{ATT} &= E[E[\beta_i | A_i = 1] | O_i = 1] \\ &= E[E[\beta_i | A_i = 1] | O_i(z') < O_i(z'')] \\ &= \underbrace{E[\beta_i | D_i(z') < D_i(z'')]}_{LATE} \end{aligned} \tag{D.8}$$

where the first and third inequalities hold by the law of iterated expectations and the second inequality holds as a result of $O_i \perp\!\!\!\perp \beta_i$.[71]

If the offer decision is unrelated to the human capital of the parent ($\phi = 0$) and the human capital of the child ($\gamma = 0$), then the offer decision and the earnings gains will be independent ($O_i \perp\!\!\!\perp \beta_i$). Under these conditions, the instrument affects the treatment status of a random sample of individuals who would accept job offers at their parent's employer and the LATE is equivalent to the ATT. This equivalence, which may hold even in the presence of selection bias and selection on gains, is possible because treatment status is determined by the choices of multiple agents.

While the empirical evidence suggests that the intergenerational transmission of employers reduces mobility, the relationship is theoretically ambiguous. This is formalized in the following proposition, which states that the counterfactual IGE corresponding to a wold in which no one worked for a parent's employer could be greater or small than the observed IGE.

Proposition 2 *Consider a deterministic case of the model by letting z_i, η_i, ν_i and u_i be equal to zero and let $c \geq 0$. Then the following statements are true:*

[71]It also exploits the fact that $O_i \perp\!\!\!\perp A_i$, which follows directly from $O_i \perp\!\!\!\perp \beta_i$.

- *if* $\frac{1}{1+\lambda} > \theta$ *and* $\phi > -\theta\gamma(1+\lambda)$ *then* $\rho(y_{ij}, y_{pj(1)}) > \rho(y_{ij(0)}, y_{pj(1)})$

- *if* $\frac{1}{1+\lambda} < \theta$ *and* $\phi < -\theta\gamma(1+\lambda)$ *then* $\rho(y_{ij}, y_{pj(1)}) < \rho(y_{ij(0)}, y_{pj(1)})$

Proof 2 *To prove the results it is useful to start by noting the implications of the deterministic setting (η_i, ν_i, u_i and z_i are set to zero) for the following expressions,*

$$\begin{aligned} O_i &= \mathbb{1}\{(\frac{\phi}{1+\lambda} - \theta\gamma)y_{pj(1)} > 0\} \\ A_i &= \mathbb{1}\{(\frac{\lambda}{1+\lambda} - \lambda\theta)y_{pj(1)} - \lambda x > c\} \\ \beta_i &= (\frac{\lambda}{1+\lambda} - \lambda\theta)y_{pj(1)} - \lambda x \end{aligned} \tag{D.9}$$

It is straightforward to show that $cov(\beta_i, y_{pj(1)}) = (\frac{\lambda}{1+\lambda} - \lambda\theta)var(y_{pj(1)})$. *In the first case, when* $\frac{1}{1+\lambda} > \theta$ *and* $\phi > -\theta\gamma(1+\lambda)$, *it immediately follows that* $\frac{\partial \beta_i}{\partial y_{pj(1)}} > 0$, $\frac{\partial O_i}{\partial Y_{pj(1)}} > 0$, $\frac{\partial A_i}{\partial y_{pj(1)}} > 0$ *and* $\frac{\partial D_i}{\partial y_{pj(1)}} > 0$. *Under the assumption that* $c \geq 0$, D_i *and* β_i *are both increasing in* $Y_{pj(1)}$, *and it follows that* $D_i\beta_i$ *is a monotonic transformation of* β_i. *Thus,* $cov(\beta_i, y_{pj(1)})$ *and* $cov(D_i\beta_i, y_{pj(1)})$ *have the same sign, which implies that,* $cov(D_i\beta_i, y_{pj(1)}) > 0$. *The proof for the second case uses the same logic.*

Proposition 2 highlight highlights two competing forces. On the one hand, the transmission of employers will reduce mobility if high income parents are best able to procure high-paying job offers for their children. On the other hand, the transmission of employers will increase mobility if children from low income households have lower levels of human capital and are more reliant on their parents to find work. In contrast to previous theoretical work by Corak and Piraino (2012) and Magruder (2010), which does not model selection into the parent's employer, reasonable arguments can be made that the transmission of employers could either increase or reduce intergenerational mobility, making this relationship theoretically ambiguous. Thus, while my empirical evidence suggests that employer transmission reduces mobility, this conclusion might differ in other contexts depending the characteristics of the labor market and the human capital accumulation process.

D.1 Extension with Parental Investment in Human Capital

Within economics, virtually all of the theoretical work on intergenerational mobility builds on the framework of Becker and Tomes (1976, 1986), in which the persistence of economic outcomes across generations is driven by investments human capital that are determined by optimizing behavior on the part of the parents. Even the two papers that have studied the role of parental labor market networks from theoretical perspective, Corak and Piraino (2012) and Magruder (2010), have used this approach. In contrast, I have ignored the decisions related to human capital investment and have instead focused on the component of earnings attributable to employer pay premiums. I refer to these effects on the employer pay premium, which are conditional on the human capital of the children, as the "direct effects." While I argue that this is most important feature to focus on, these channels are not mutually exclusive and may interact in interesting ways. I explore this possibility in this section by extending the stylized model to allow for parents to shape the human capital of their children through investments. I refer to the effects mediated by parental investment decisions as the "indirect effect" of the intergenerational transmission of employers.

I consider a model in the vein Becker and Tomes (1976, 1986) in which parents make decisions regarding the optimal investments of the human capital of their children. For tractability I focus on the deterministic setting (z_i, η_i, ν_i and u_i are equal to zero) and assume that children only accept job offers from their parents when the earnings benefits are positive ($c \geq 0$). Furthermore, I maintain the assumptions underlying equations D.1, D.2 and D.3. However, I do not impose the assumption stated in equation D.4, because the goal of this section is to derive the relationship between parental earnings and the human capital of the child as the result of optimizing behavior on the part of the parents. For notation, I use lower case letters to denote the log of upper case variables (for examples, $h_i = log(H_i)$).

Parents care about their current period consumption, C_p, and the total financial resources of their children, which depends on the earnings of the children, Y_{ij}, and bequests,

B_i, plus interest accrued at rate R. Parents solve the following problem:

$$\max_{C_p, C_i, B_i} \{v(C_p) + u(Y_{ij} + RB_i)\} \text{ subject to } C_p + S_i + B_i \leq Y_{pj(1)} \tag{D.10}$$

where S_i represents investment in the human capital of the children and $u(\cdot)$ and $v(\cdot)$ are continuous functions that both have the following properties: $u'(\cdot) > 0$, $u''(\cdot) < 0$ and $u'(0) = \infty$. This setup assumes that there are no credit constraints, as bequests may be negative.

While there are a number of ways to generate intergenerational persistence in earnings in the absence of credit constraints, I follow Becker et al. (2018) and assume that there are complimentarities between the human capital of the parent and the production of human capital of the child. Specifically, investment translates into human capital according to the following production function, $H_i = H_p^{\sigma} S^{\alpha}$. Intuitively, this captures the fact that investments in human capital might be more productive if made by parents with higher ability. I also assume that $\alpha(1 + \lambda) < 1$ which implies that there are diminishing returns to parental investment. The optimal level of investment in human capital is defined by the level at which the marginal rate of return is equal to the interest rate, $\frac{\partial Y_{ij}}{\partial S_i} = R$. Combining terms, we can rewrite the expression determining optimal investment as follows,

$$\alpha(1 + \lambda) H_p^{\sigma(1+\lambda)} S_i^{\alpha(1+\lambda)-1} exp\{D_i \beta_i\} + H_p^{\sigma(1+\lambda)} S_i^{\alpha(1+\lambda)} \frac{\partial exp\{D_i \beta_i\}}{\partial S_i} = R \tag{D.11}$$

where the left-hand side represents the marginal returns to investments in human capital and the right-hand side represents the marginal returns to bequests.

To understand how the transmission of employers shapes the investment decision it is useful to consider three cases. As a starting point consider the case in which parents do not account for employer transmission when making investment decisions ($exp\{D_i \beta_i\} = 1$ and $\frac{\partial exp\{D_i \beta_i\}}{\partial S_i} = 0$). Under these conditions is it straight forward to show that the optimal level of investment is given as:

$$S_i' = [\frac{R}{\alpha(1+\lambda)}]^{1/[\alpha(1+\lambda)-1]} H_p^{\sigma(1+\lambda)/[1-\alpha(1+\lambda)]} \tag{D.12}$$

Thus, the optimal level of parental investment is increasing in the human capital of the parent and decreasing in the interest rate and it produces the following relationship between the human capital of the child and the earnings of the parent, $h_i = x + \theta y_{pj(1)}$, where $x = \frac{-\sigma}{1-\alpha(1+\lambda)} log(\frac{R}{\alpha(1+\lambda)})$ and $\theta = \frac{\sigma/(1+\lambda)-(1-\alpha)}{1-\alpha(1+\lambda)}$. Note that this linear relationship is exactly the one assumed in Section D.

How will this relationship change if parents consider the possibility of helping their child to secure a job within their employer when making investment decisions? In a step towards answering this question, consider a second case in which parents account for the fact that the transmission of employers might affect the level of earnings ($exp\{D_i\beta_i\} \neq 1$) but they do not account for the fact that investments might affect the gains associated with transmission ($\frac{\partial exp\{D_i\beta_i\}}{\partial S_i} = 0$). Under these assumptions, the optimal level of investment is defined as, $S_i'' = S_i' \times exp\{\frac{D_i\beta_i}{1-\alpha(1+\lambda)}\}$ and it follows that,

$$s_i' - s_i = \frac{D_i\beta_i}{1-\alpha(1+\lambda)} \geq 0 \tag{D.13}$$

Because $\exp\{D_i\beta_i\} \geq 0$ and $\alpha(1+\lambda) < 0$, this mechanism leads to an increase in parental investment. Intuitively, the transmission of employers provide access to firms that pay higher wages and thus parents who expect their children to work with them will expect a higher rate of return on investments in human capital.[72]

In the third case I allow for the investment decisions of parents to also depend on the anticipated effects of a rise in human capital on the gains of working for a parent's employer ($\frac{\partial exp\{D_i\beta_i\}}{\partial S_i} \neq 0$).[73] Because $\frac{\partial exp\{D_i\beta_i\}}{\partial S_i} < 0$, it is immediately apparent that if we were to plug in S_i'' into equation D.11 the sum of the terms of the left hand side would

[72] Different assumptions could lead to alternative conclusions. For example, both Corak and Piraino (2012) and Magruder (2010) assume that the effect of networks on earnings is additive in levels, which leads them to conclude that parental investment decisions are unaffected by the presence of parental labor market networks.

[73] As in case 2, I continue to allow for the possibility that $exp\{D_i\beta_i\} \neq 0$.

be less than the interest rate on the right hand side. Furthermore, under the assumption that $\gamma < 0$, both $\alpha(1+\lambda)H_p^{\sigma(1+\lambda)}S_i^{\alpha(1+\lambda)-1}exp\{D_i\beta_i\}$ and $H_p^{\sigma(1+\lambda)}S_i^{\alpha(1+\lambda)}\frac{\partial exp\{D_i\beta_i\}}{\partial S_i}$ are (weakly) decreasing in S_i, and it follows that the optimal level of investment in case 3 is less than the optimal level in case 2, $S_i''' < S_i''$. In the mechanism highlighted in this case, the intergenerational transmission of employers reduces the incentive to invest in human capital because the earnings gains associated with working the parents' employer are declining in the human capital of the child (both along intensive and extensive margins).

Taken together, the total indirect effect of the intergenerational transmission of employers on the level of parental investment is theoretically ambiguous.[74] On the one hand, the transmission of employers will increase the marginal returns to human capital investments by providing access to high-paying firms. On the other hand, the marginal returns are pushed down by the fact that higher ability children are less likely to work with their parents and gain less conditional on doing so.

The implications for intergenerational mobility are similarly ambiguous. For simplicity, consider the case in which $\theta(1+\lambda) < 1$ and $\phi > -\theta\gamma(1+\lambda)$, which implies that the direct impact of employer transmission will increase IGE. Because these conditions imply that $D_i\beta_i$ is increasing in parental earnings, children from high income families will tend to be the greatest beneficiaries of working with their parents (being more likely to do so and experiencing greater benefits conditional on doing so). The mechanism highlighted in case 2 will amplify the disparities between children from high and low income households while the mechanism highlighted in case 3 will mitigate these differences. The total indirect effect on intergenerational mobility will depend on which force dominates.

D.2 Sign of Selection Bias

In order to highlight the empirical challenges created by the unobserved components of earnings, start by decomposing the following estimator into a causal effect and selection

[74] This follows from the fact that I have shown that $S_i' \leq S_i''$ and $S_i''' < S_i''$. Thus the total effect (difference between S_i' and S_i''') will depend on whether the mechanism highlighted in case 2 or 3 is stronger.

bias,

$$\underbrace{E[y_{ij}|D_i=1,y_{pj(1)}]-E[y_{ij}|D_i=0,y_{pj(1)}]}_{\text{estimator}}=E[y_{ij(1)}-y_{ij(0)}|D_i,y_{pj(1)}]+E[y_{ij(0)}|D_i=1,y_{pj(1)}]-E[y_{ij(0)}|D_i=0,y_{pj(1)}]$$

$$=\underbrace{E[\beta_i|D_i=1,y_{pj(1)}]}_{\text{ATT}}+\underbrace{E[\epsilon_i|D_i=1,y_{pj(1)}]-E[\epsilon_i|D_i=0,y_{pj(1)}]}_{\text{selection bias}} \tag{D.14}$$

where $\epsilon_i=(1+\lambda)\eta_i+\nu_i+u_i$. From inspection, ϵ_i will generate selection bias if and only if $cov(\epsilon_i,D_i)<0$.

In order to sign the selection bias term we must rewrite D_i as a function of parental earnings and the idiosyncratic error terms. The assumption that parents do not share an employer with their own parents (the employer of p is $j(1)$) in conjunction with equations D.1, D.2 and D.3 implies that $f_{ij(1)}=\frac{\lambda y_{pj(1)}-\lambda u_i+\nu_p}{1+\lambda}$. Combining this expression with equation D.4 yields,

$$A_i=\mathbb{1}\{(\frac{\lambda}{1+\lambda}-\lambda\theta)y_{pj(1)}+\frac{\lambda}{1+\lambda}(\nu_p/\lambda-u_p)>c+\lambda x+\lambda\eta_i+\nu_i\} \tag{D.15}$$

Equation D.3 implies $h_p=\frac{y_{pj(1)}-\nu_p-u_p}{1+\lambda}$ and combing this with equation D.4 yields,

$$O_i=\mathbb{1}\{(\frac{\phi}{1+\lambda}+\gamma\theta)y_{pj(1)}-\frac{\phi}{1+\lambda}(\nu_p-u_p)+\gamma(x+\eta_i)>z_i\} \tag{D.16}$$

Thus, because $D_i=O_i\times A_i$, we have written D_i as a function of parental earnings and the idiosyncratic error terms.

Inspection of how η_i and ν_i enter equations D.15 and D.2 illustrates two potential sources of selection bias. First note that ν_i and O_i are independent while A_i and ν_i are negatively correlated. Thus, ν_i and D_i will be negatively correlated and ν_i will generate negative selection bias. Intuitively, children who receive job offers at low-paying firms will be more willing to accept offers at their parents employers and this will lead us to underestimate the benefits of employer transmission. Second, η_i and A_i are negatively correlated, which again will tend to produce negative selection bias. Intuitively, low-ability children will have more limited outside employment opportunities and will be more willing to work at their parents' employer. However, the relationship between η_i

and O_i is ambiguous and will depend on the sign of γ. If $\gamma < 0$ then η_i and O_i will be negatively correlated, which will produce negative selection bias because the low ability children will be more likely to receive job offers. However, if $\gamma > 0$ then η_i and O_i will positively correlated. In this latter case, the effect of η_i on the selection bias term will be ambiguous and will depend on the relative importance of its effect on O_i and A_i.

D.3 References

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