The Intergenerational Transmission of Employers and the Earnings of Young Workers*

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Abstract

This paper investigates how parental connections to firms shape early-career earnings. I use employer-employee linked data to study one important type of connection: jobs obtained at a parent's employer. 29 percent of individuals work for a parent's employer at least once by age 30. Exploiting transitory, firm-specific fluctuations in hiring conditions at the parent's employer, I estimate that working for a parent's employer increases initial earnings by 24 percent, primarily by providing access to higher-paying firms. Individuals with higher-earning parents are more likely to work for a parent's employer and experience larger earnings gains when they do. Consequently, the elasticity of initial earnings with respect to parental earnings would be 12 percent lower if no one found a job through these connections. The findings suggest that connections to firms through one's social network may be an important determinant of intergenerational mobility.

Keywords: intergenerational mobility, labor market networks, job ladders
JEL Codes: J31, J62, L14

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1 Introduction

To what extent do parental connections in the labor market shape the earnings of young workers? The answer depends on how often individuals find jobs through parental connections and how much more they earn at those jobs relative to their alternatives. Connections may reduce information frictions between workers and firms or influence hiring decisions through referrals or favoritism (Montgomery, 1991; Dustmann et al., 2016; Burks et al., 2015), potentially providing access to higher-paying jobs that would otherwise be out of reach (San, 2022). If individuals from higher-income families are more likely to use these connections or benefit more when they do, parental networks may be an important driver of intergenerational mobility.

This paper studies how parental connections shape the earnings of young workers by focusing on one specific channel: jobs obtained at a parent's employer. Working for the same firm as a parent—i.e., the *intergenerational transmission of employers*—is not uncommon: I find that 29 percent of individuals work for the same firm as a parent by age 30, which is consistent with prior evidence (Kramarz and Skans, 2014; Corak and Piraino, 2011; Stinson and Wignall, 2018). I use linked employer-employee data from the Longitudinal Employer-Household Dynamics (LEHD) program and the 2000 Decennial Census to measure how often young workers find jobs through these connections and estimate the causal effect on earnings. I then examine how the benefits vary with parental income and quantify the implications for intergenerational earnings persistence. The estimates of the magnitude and source of the earnings gains shed light on how and why connections, more broadly defined, may shape intergenerational mobility.

I begin by showing that 5 percent of individuals work for a parent's employer in their first job, with substantial differences by parental income. Among children of parents in the top percentile of the earnings distribution, 7 percent work for a parent's employer, compared to just 2 percent among those from the bottom percentile. The analysis focuses on the first stable job, which plays a critical role in shaping long-term career trajectories (Von Wachter and Bender, 2006; Kahn, 2010; Arellano-Bover, 2022).[1] However, these

[1]The definition of the first job is provided in Section 3.

patterns are not limited to the start of the career: individuals continue to work with their parents in subsequent jobs, and disparities by parental income widen over time.

Parental connections are one explanation for why children and parents may work for the same firm. However, parents and children may also sort into the same firms due to other reasons, such as shared skills or preferences. To isolate the role of connections, I compare the probability that a child works for the firm their parent recently joined and currently works for to the probability of working for a firm the parent will join in the near future. Children are five times more likely to work for a parent's current employer than a parent's future employer. Assuming the only systematic difference between the two firms is the presence of a connection, this implies that approximately 80 percent of individuals who work for a parent's employer do so as a result of parental connections. This may understate the role of connections if parents also have ties to future employers—for example, through extended family or friends.

The main empirical challenge is to estimate the earnings consequences: among individuals who work for their parent's employer, how much more do they earn there relative to their next-best option? Estimating this causal parameter is difficult because those who work for their parent's employer may differ in unobserved ways. Indeed, even when making comparisons within a family, I find that those without a college degree are twice as likely to work for their parent's employer compared to their college-educated siblings. An empirical strategy must account for the fact that young workers with limited outside options are more likely to rely on parental connections.

The ideal experiment to estimate the causal effect of working for a parent's employer would randomly prohibit some firms from hiring the children of current employees. Random assignment across firms would serve as an instrument, generating variation in access to jobs at a parent's employer that is orthogonal to other determinants of child outcomes. This design would identify the average effect for individuals who work for their parent's employer and allows for the possibility that some children would choose not to work for their parent's firm, and some firms would choose not to offer them a job.

I approximate the ideal experiment using an instrumental variables strategy that exploits transitory, firm-specific fluctuations in hiring at the parent's employer. The idea is to compare children whose parents work at the same firm but who enter the labor market in different years. If the firm happens to be hiring less when one child enters the labor market, they should be less likely to work there. To isolate variation that reflects firm-specific shocks rather than broader labor market trends, I compare this to changes over the same time period for children whose parents work for a different firm in the same industry and commuting zone.

I implement this design by instrumenting for whether an individual begins their career at a parent's employer using the hiring rate at that firm in the year before they enter the labor market, controlling for fixed effects for the parent's employer, fixed effects for the local labor market (defined by commuting zone, industry, and quarter), and parental earnings. To illustrate that the approach isolates transitory, firm-specific fluctuations in hiring, I show that child outcomes are strongly related to contemporaneous hiring rates at the parent's employer, but unrelated to hiring rates at the same firm just a few years earlier or at other observably similar firms in the same local labor market.

The first stage is strong: increasing the hiring rate from the 5th to the 95th percentile raises the probability of working for a parent's employer by 2.8 percentage points.

The main finding is that working for a parent's employer leads to a 24 percent increase in initial earnings at a young worker's first job. These gains are substantial—approximately one-third the size of the college wage premium—and demonstrate how connections can provide access to higher-paying jobs. The effects are also persistent: three years later, individuals who began their careers at their parent's employer earn 20 percent more. And the gains are larger for those from high-income families, as children of parents in the top 40 percent of the earnings distribution experience gains that are over twice as large as those whose parents are in the bottom 40 percent.

These earnings gains are largely explained by parents providing access to higher-paying blue-collar jobs for children who would otherwise work in the unskilled service sector. More generally, the results are consistent with frictional models of the labor market in which higher-paying firms occupy higher rungs on the job ladder (Manning, 2013). I estimate firm-specific pay premiums using the AKM decomposition of earnings (Abowd et al., 1999) and find that the impact on the AKM firm effect is virtually identical to the impact on individual earnings. In other words, parents provide access to firms that pay all workers higher wages, not to relatively high-paying jobs within these firms.

The key identification assumption, exogeneity, has three main components. Firm-specific, transitory fluctuations in hiring at the parent's employer must be unrelated to: (1) labor demand shocks at other firms, (2) changes in wages at the parent's employer, and (3) characteristics of the child—such as education or skills—that have an independent effect on earnings. I conduct three sets of analyses to assess the plausibility of each condition.

First, I address concerns about correlated labor demand shocks. While the local labor market fixed effects absorb common shocks across firms in the same commuting zone and industry, they may not fully capture variation among firms competing for the same workers. To test this, I use hiring conditions at the parent's future employer as a proxy for unobserved labor demand shocks. The intuition is that the skills parents pass to their children are likely valued by both the parent's current and future employer, but the parent has connections only at their current employer. I find that child outcomes are strongly related to hiring at the parent's current employer, but unrelated to hiring at the parent's future employer—helping to rule out bias from broader labor demand shocks.

Second, I address concerns about time-varying changes in firm wages. Expanding firms may hire more workers and raise wages, which would lead to bias by increasing the earnings of the always-takers—children who would work at their parent's employer regardless of hiring conditions. To address this concern, I show that the results are robust to isolating variation in hiring conditions that is orthogonal to changes in firm size and the earnings growth of incumbent workers. This robustness is likely due to the transitory nature of the identifying variation, as firms are less likely to adjust wage policies in response to short-term hiring fluctuations.

Third, I address concerns that parents may dynamically sort into firms or children may endogenously adjust their entry into the labor market, both of which could create a spurious correlation between hiring rates and child outcomes. I first show that results are robust to comparisons between siblings, using household fixed effects to control for unobserved family characteristics. Second, the results are similar when the hiring rate is measured in the year the child turns 18, and this rate is unrelated to whether the child ever finds a first job. This helps rule out potential bias arising from parental connections influencing not just where, but also when—or even whether—the child finds their first job. Finally, observable child characteristics—such as education—are unrelated to hiring conditions at the parent's employer, further reducing concerns about endogenous sorting.

This evidence supports the validity of the key identification assumptions, but it is impossible to definitively rule out all concerns. To further strengthen the credibility of the findings, I employ an alternative empirical strategy that relies on an entirely distinct set of identification assumptions. Specifically, I compare young co-workers with similar earnings and demographic characteristics who are displaced during the same mass layoff event. The idea is that these co-workers are likely to have similar skills and are forced to search for a new job in the same local labor market. This alternative strategy arrives at the same conclusion: individuals who subsequently join their parent's employer after being displaced earn 23 percent more because they find jobs at higher-paying firms.

The last section of the paper shows that individuals with higher-earning parents benefit more from these connections because they are more likely to work for a parent's employer and experience larger earnings gains when they do. To quantify the implications for intergenerational mobility, I develop a method to combine the descriptive and causal estimates and find that the elasticity of initial earnings with respect to parental earnings would be 12 percent lower if no one worked for a parent's employer. In other words, the intergenerational persistence in earnings is attributable, in part, to parents using their connections to provide access to higher-paying firms. These findings are qualitatively similar when I use the mass layoff design to estimate the earnings consequences or use the analysis of the parent's future employer to adjust for the possibility that some individuals might find work for a parent's employer even absent connections. Parental connections also account for 9 percent of the initial gender pay gap and 4 percent of the

Black-White mobility gap for boys documented by Chetty et al. (2020).

This paper builds on and relates to several literatures.

First, this paper contributes to the intergenerational mobility literature, which seeks to explain why economic outcomes persist across generations (Black and Devereux, 2010). Prior work shows that children from low-income families experience greater upward mobility in areas with more cross-class social connections (Chetty et al., 2022) and higher parental employment rates (Chetty et al., 2024). However, the mechanisms underlying these relationships remain unclear. For example, social connections may shape aspirations or directly improve access to jobs through referrals. San (2022) shows that parental connections shape earnings gaps between ethnic groups and other recent evidence documents a link between family background and place of work (Dobbin and Zohar, 2023), occupational choice (Haeck and Laliberté, 2025), and post-displacement earnings (Kaila et al., 2024). A key contribution of this paper is to show how one specific connection—access to a parent's employer—causally shapes early-career outcomes and intergenerational mobility. The results suggest that connections to firms through social networks, more broadly defined, may be an important driver of mobility, particularly given that a majority of jobs are found through a social contact (Ioannides and Datcher Loury, 2004).

Second, the paper relates to the large literature on labor market networks. Prior research finds that social connections shape labor market outcomes, but there is little consensus on the magnitude or source of the earnings gains of finding a job through a social contact (Bayer et al., 2008; Beaman, 2012; Cingano and Rosolia, 2012; Schmutte, 2015; Gee et al., 2017; Hellerstein et al., 2019; Zimmerman, 2019; Caldwell and Harmon, 2019; Barwick et al., 2023). Kramarz and Skans (2014), Corak and Piraino (2011), and Stinson and Wignall (2018) show that it is not uncommon for young workers to find jobs through connections at a parent's employer. I build on this descriptive evidence by estimating the causal effect of using these connections. I find large earnings gains, implying that parental networks can meaningfully contribute to earnings inequality.

Third, this paper contributes to the literature on how firm-level pay policies shape earnings inequality. The AKM literature documents large differences in pay across employers, but there is a debate about whether these gaps reflect firm-level pay premiums or arise from the endogenous sorting of workers across firms (Card et al., 2018). Several papers study workers who separate for plausibly exogenous reasons and find that changes in earnings closely track changes in firm pay premiums (Schmieder et al., 2023; Lachowska et al., 2022). I provide complementary evidence of the importance of firm pay premiums, as my empirical strategy isolates exogenous variation in the firms individuals join.

The remainder of the paper is organized as follows. Section 2 presents the conceptual framework. Section 3 describes the data. Section 4 examines how often and why individuals work for their parent's employer. Section 5 estimates the earnings effects, and Section 6 quantifies implications for intergenerational mobility. Section 7 concludes.

2 Conceptual Framework

This section presents a conceptual framework that relates the intergenerational transmission of employers to the intergenerational persistence in earnings. Let y_{ij} denote the log earnings of individual i at their first stable job, which is at firm j. And let y_p denote the log earnings of i's parents. My objective is to understand how the intergenerational persistence in earnings would change if no one worked for their parent's employer.

Using the potential outcomes framework, let $y_{ij(1)}$ denote the individual's earnings if they work for their parent's employer and let $y_{ij(0)}$ denote their earnings if they work for the firm that is their next best option (i.e., where they would work if they did not work for their parent's employer). The treatment effect of working for a parent's employer is the difference between potential outcomes and is denoted $\beta_i = y_{ij(1)} - y_{ij(0)}$. Thus,

$$y_{ij} = D_i\beta_i + y_{ij(0)}, \tag{1}$$

where D_i is an indicator equal to one if the individual works for their parent's employer.

I quantify how the intergenerational transmission of employers affects the intergenerational elasticity of earnings (IGE), which is a common measure of the intergenerational persistence in earnings. The IGE is the coefficient obtained from regressing y_{ij} on y_p and is denoted $\rho(y_{ij}, y_p)$. By combining equation 1 with the identity $\rho(y_{ij}, y_p) \equiv \frac{\text{cov}(y_{ij}, y_p)}{\text{var}(y_p)}$, it follows that the difference between the observed IGE and the IGE that corresponds to the counterfactual in which no one worked for their parent's employer can be written as

$$\rho(y_{ij}, y_p) - \rho(y_{ij(0)}, y_p) = \frac{\text{cov}(D_i\beta_i, y_p)}{\text{var}(y_p)}. \quad (2)$$

To estimate $\text{cov}(D_i\beta_i, y_p)$ I develop the following approximation:

$$\text{cov}(D_i\beta_i, y_p) \approx \mathbb{E}\Big[\mathbb{E}[D_i|r_p]E[\beta_i|r_p, D_i = 1]\mathbb{E}[y_p|r_p]\Big] - \mathbb{E}[D_i]\mathbb{E}[\beta_i|D_i = 1]\mathbb{E}[y_p], \quad (3)$$

where r_p is the quantile rank of parental earnings. The approximation relies on two insights. First, by iterated expectations, the average benefit of working for a parent's employer can be written as $\mathbb{E}[D_i\beta_i] = \mathbb{E}[D_i]\mathbb{E}[\beta_i \mid D_i = 1]$. Second, the expected value of the product of two random variables is approximately equal to the product of their expected values if there is little variation in one of the variables: $\mathbb{E}[D_i\beta_i y_p|r_p] \approx \mathbb{E}[D_i\beta_i|r_p]\mathbb{E}[y_p|r_p]$. See Appendix D.1 for details. To validate the approximation, I show that the IGE based on the micro data, 0.136, is similar to estimates derived from the approximation, 0.140. Section 6 explains why measuring the earnings of the child at their first job yields a smaller IGE compared to estimates of the IGE that use earnings measured later in life.

A key advantage of this method is that it accounts for non-random selection into a parent's employer. This contrasts with common approaches that assess changes in the IGE after adding controls. For example, Corak and Piraino (2011) and Stinson and Wignall (2018) compare baseline intergenerational earnings regressions to specifications that control for whether an individual works for their parent's employer.

Equation 2 shows that the intergenerational transmission of employers will increase the intergenerational persistence in earnings if the average benefits, $\mathbb{E}[D_i\beta_i \mid y_p]$, are increasing in parental earnings. This average benefit is the product of the proportion of individuals who work for their parent's employer and the average treatment effect on the treated (ATT). My goal is to estimate how these two objects vary with parental earnings.

To anticipate how the intergenerational transmission of employers might affect the intergenerational persistence in earnings, I develop a stylized model that is consistent with the main empirical findings from my paper. I summarize the key points here and refer the reader to Appendix D.2 for the details. Following the literature, earnings depend on human capital, which is positively correlated with parental earnings. I depart from existing models of intergenerational mobility by allowing earnings to also depend on a firm-level pay premium. Individuals receive a job offer through formal job search, and those with higher human capital tend to receive offers from firms with higher pay premiums. The parent's employer may also make a job offer to the child and this offer decision depends on the human capital of the child and the parent. The child will accept the offer if the benefits—which are positive if the parent's firm has a higher pay premium relative to the child's outside option—are sufficiently large.[2]

There are two insights from the model. First, the effect of the intergenerational transmission of employers on the intergenerational persistence in earnings is theoretically ambiguous. On the one hand, higher-earning parents are better able to produce high-paying job offers. On the other hand, children of lower-earning parents have lower levels of human capital and are more reliant on their parents to find a decent-paying job.

Second, human capital investment decisions could amplify or dampen the direct effect of employer transmission on the intergenerational persistence in earnings. On the one hand, the marginal returns to investment in human capital may be higher for those who work for a parent's employer (since these are high-paying firms). On the other hand, the marginal returns may be lower because higher-ability individuals have better outside options and therefore benefit less from parental connections. Thus, human capital investment decisions. My counterfactual exercise should be viewed as a partial equilibrium analysis, which does not account for the possibility that individuals might adjust investment in human capital if there was no option to work for their parent's employer.

3 Data

I rely on two main sources of data (1) the 2000 Decennial Census and (2) the LEHD program. The Decennial Census is a household survey that allows me to measure the

[2] Magruder (2010) and Corak and Piraino (2010) develop models of mobility that incorporate parental contacts. Neither paper considers the role of firm pay premiums nor the endogenous use of social contacts.

relationships between parents and children who live in the same household in 2000. In principle, these data include all individuals living in the United States. In practice, some individuals are not surveyed and non-respondents are more likely to be minorities and low-income households (Mulry, 2007). The LEHD is an employer-employee linked dataset produced by the U.S. Census Bureau and allows me to measure labor market outcomes of both parents and their children between 1990 and 2018. The LEHD 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. These data capture roughly 96 percent of private non-farm wage and salary employment in the United States but do not cover self-employment (Abowd et al., 2009). 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.

My sample frame includes individuals for whom I can measure parent-child relationships and early-career outcomes. Specifically, the sample frame includes children in the 2000 Decennial Census who (1) live with a parent, (2) are expected to graduate high school between 2000 and 2013, and (3) reside in a state that began reporting to the LEHD at least two years prior to the expected year of high school graduation.[3] 91 percent of individuals younger than 18 live with a parent in 2000. By the end of 2018, the youngest and oldest individuals in the sample were 23 and 37 and years old, respectively. The third criteria accounts for the fact that coverage of the LEHD varies by state, with 8 states and Washington, D.C. starting to report after 2000. There are approximately 48 million individuals in the sample frame.

I drop individuals from the sample if I am unable to link them across datasets or accurately measure parental earnings. Individuals are identified by a Protected Identification Key (PIK), which the Census Bureau generates using personally identifiable information. I drop 19 percent of the sample frame because the child is not assigned a

[3] Expected year of high school graduation is based on month and year of birth and individuals born between September 1st and August 31st are assigned to the same cohort. The sample frame includes individuals born between September 1st of 1981 and August 31st of 1995. When measuring parent-child relationships, I include biological, adopted, and step children.

PIK and therefore cannot be linked to the LEHD. I drop an additional 7 percent because a parent is not assigned a PIK or the household in the Decennial Census contains more than 15 individuals. Some individuals with very low earnings have earnings from other sources not covered by the LEHD. Thus, I drop an additional 7 percent of the sample frame if the combined annual earnings of the parents is less than $15,000 (I discuss the measurement of parental earnings in more detail below). Of the 48 million individuals in the sample frame approximately 32 million, or 67 percent of the sample frame, meet these restrictions (see Table B.1). The resulting sample is broadly representative of families for whom wages constitute the majority of earnings and income, a group that excludes the very poor (approximately the bottom 10 percent of households) and very rich (approximately the top 1 percent of households).[4]

First stable job. I define the first stable job, or just first job, as the first quarter in which an 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.[5] Conceptually, this is the first quarter in which work becomes the primary activity. I refer to this employment spell as the *first stable job* and measure *initial earnings* during the first full-quarter of employment at this job.[6] 26 million individuals, or 82 percent of those that meet the sample restrictions, obtain a first stable job by the end of 2018. Individuals who never find a first stable job have persistently low earnings, with an average annual earnings of only $1,130 at age 30.

Three pieces of evidence suggest that my definition of a first stable job is reasonable. First, individuals experience a dramatic and persistent increase in earnings when they start their first job. Average annual earnings increase from $7,084 to $29,080 in the year when the first job begins (Figure A.1 plots the age-earnings profiles). Second, the age at first job agrees with common other common definitions of labor market entry. 86 percent

[4]Using data from the the Current Population Survey, I find that wages tend to be the primary source of income for households above the 10th percentile of the income distribution. Smith et al. (2019) find that non-wage earnings become increasingly important in the top 1 percent of earners.

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

[6]A full-quarter employment spell occurs when a worker receives strictly positive earnings from the same employer in the current, previous, and subsequent quarter.

of young workers in my data find their first job between the ages of 18 and 26. I calculate an analogous measure using the NLSY97 and find that 86 percent of respondents find their first stable job between these ages.[7] Furthermore, 83 percent of workers in the NLSY97 data are not enrolled in school when they find their first job, which suggests that my measure is not primarily picking up jobs held by students. Third, 40 percent of young workers remain at their first employer for at least three years.

Parental earnings. 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. However, the LEHD present unique challenges as there is no way to distinguish between zero earnings and earnings that are not covered by the LEHD frame. To account for these issues, I construct a long-run measure of parental earnings by regressing quarterly earnings on an individual fixed effect and a third degree polynomial in age within samples defined by the interaction between state of residence in 2000, sex, and education.[8] Using these parent-specific age-earnings profiles, I calculate the average annual earnings between the ages of 35 and 55. Parental earnings is the sum of the individual earnings of both parents. I calculate percentile ranks based on parental earnings within cohorts defined by expected year of high school graduation. See Appendix C.1 for details.

Employers. 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] I use the terms "firm" and "employer" to refer to the entity identified by the SEIN. About half of individuals work for a firm with multiple establishments and the LEHD imputes the link between workers and establishments. I primarily focus on the firm, but in some analyses I use the establishment impute to measure the location of the job within a state.

[7]Figure A.2 presents the distribution of age at first job for individuals in my sample and in the NLSY97. The analogous measure constructed from the NLSY97 is the first time an individual works at least 35 hours for 36 consecutive weeks (three quarters). An alternative approach is to focus on labor market outcomes after all schooling is completed and Figure A.2 also presents results for this measure.

[8]The data are a panel measured at a quarterly frequency that include all strictly positive earnings records between 1990 and 2018 for the parents in the sample. Quarters with zero earnings are not included in the sample. I further restrict the panel to individuals between the ages of 25 and 65 and drop individuals that have fewer than 12 quarters of strictly positive earnings over the entire time period. Parents not included in this sample are assumed to have zero earnings.

[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 most earnings in that quarter.

4 Use of Parental Connections

I begin by documenting how common it is to work for a parent's employer. Columns 1 through 3 of Table B.2 present summary statistics for the full sample as well as individuals who do and do not work for a parent's employer, respectively. 5 percent of individuals work for a parent's employer at their first job and these individuals tend to stay at these jobs longer, are less likely to be employed in the unskilled service sector and more likely to be employed in the blue-collar sector, and earn slightly less.[10] Of individuals who work for a parent's employer, only 19 percent have a parent who is in the top percentile of the within-firm earnings distribution, suggesting that the majority of these parents are employees, not owners or top executives.

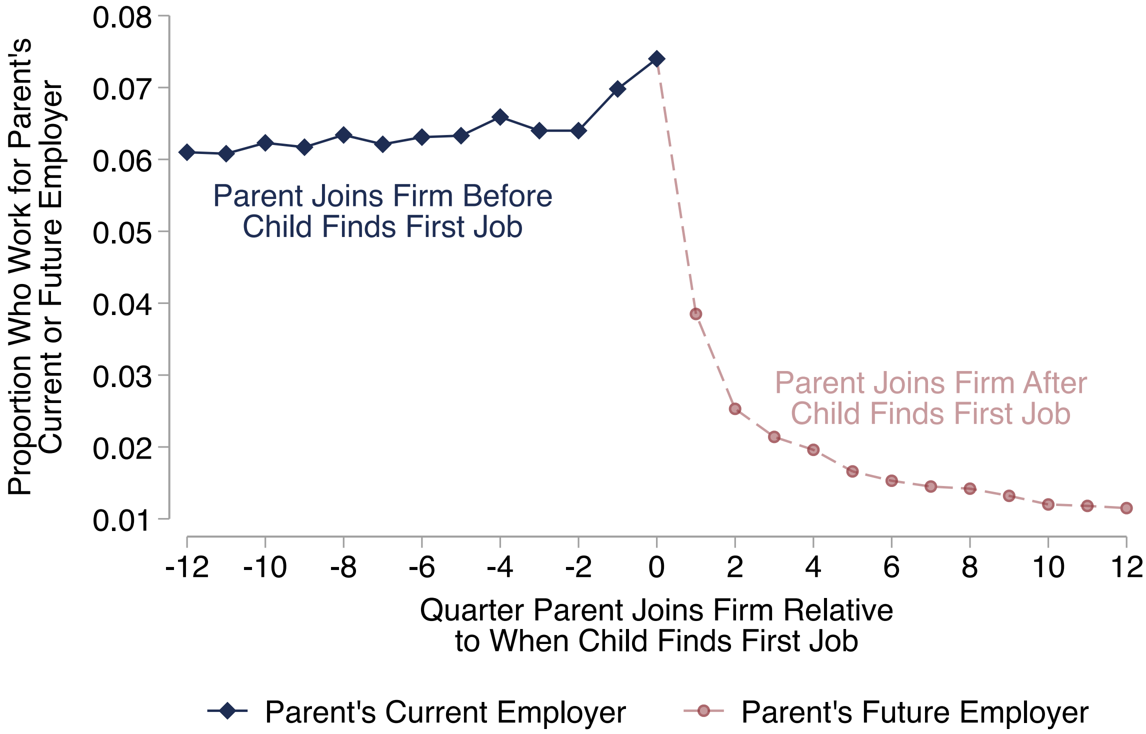
Figure A.4 plots the cumulative share of individuals who work for a parent's employer between the ages of 16 and 30. By age 30, 29 percent of individuals have worked for a parent's employer. This is consistent with Stinson and Wignall (2018), who find that 22 percent of sons work with their father by the time they are 30 years old.

Parental connections offer one explanation for why someone may work for the same firm as their parent. Connections could facilitate access to these jobs by reducing information frictions between workers and firms or influencing hiring decisions through referrals or favoritism. However, some individuals might work for the same firm as a parent for other reasons. For example, parents and children might have similar skills and live in the same area, which could lead them to sort into the same firms. To distinguish between these two explanations, we need to know how often individuals would work for their parent's employers if connections did not influence the hiring or job search process.

I answer this question by comparing the likelihood of working for a parent's current and future employer. The intuition is that these two firms are likely similar along many dimensions, with the one exception that parent are less likely to have connections at the future employer. I identify parents who begin a new job within three years of their child entering the labor market. Figure 1 plots the proportion of children who work for that

[10] I group two-digit North American Industry Classification System (NAICS) industry codes into three sectors: unskilled services, skilled services, and blue-collar. See Appendix C.2 for details.

Figure 1: Likelihood of Working for Parent's Current and Future Employer

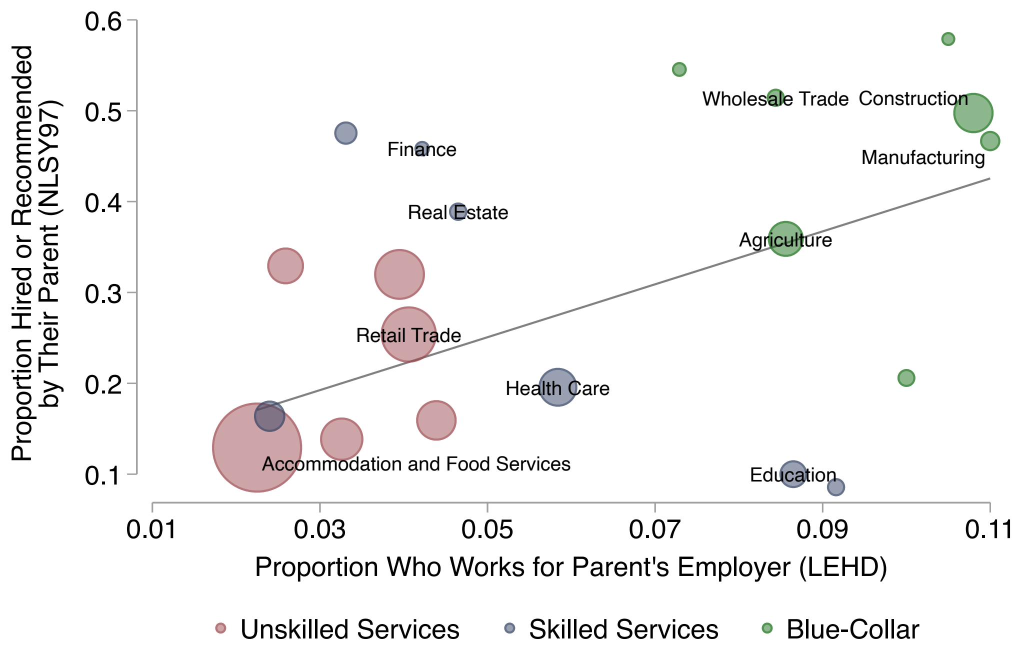


Notes: The horizontal axis defines a sample of individuals based on when their parent started working for a new firm. The sample is limited to parents who remain at these new jobs for at least 12 quarters. Thus, the blue diamond markers represent cases in which the parent recently joined and currently works for the firm when their child starts their first stable job. The red circle markers represent cases in which the parent will join the firm in the near future but is not currently working there when their child starts their first stable job. Each point plots the proportion of individuals who work for their parent's current or future employer.

firm against the quarter in which their parent started the job. The sample is restricted to parents who remain at the firm for at least three years, ensuring that if they joined the firm before their child entered the labor market, they are still at the firm when the child finds their first job. Individuals are 5 times more likely to work for a firm that their parents joined 2 to 3 years before child finds their first job compared to 2 to 3 years after.

If the presence of parental connections is the only systematic difference between the current and future employers, then these estimates suggest that approximately 80 percent ($0.8 = 1 - \frac{1}{5}$) of individuals who work for a parent's employer found their job via parental connections. This likely overstates the likelihood that an individual finds a job at their parent's employer for reasons unrelated to connections since parents may have ties to the

Figure 2: Industry-Level Association with Use of Social Contacts



Notes: Each point represents an industry-level statistic, with marker size proportional to sample size. The horizontal axis shows the share of individuals whose first job was at a parent's employer, measured using the LEHD and 2000 Decennial Census. The vertical axis shows the share of jobs in which individuals were hired or recommended by a parent, based on data from the NLSY97.

future employers indirectly through other social contacts like extended family or friends.

I also compare the likelihood of working for a parent's employer to a different, but observably similar firm located in the same local labor market. For each employed parent, I identify ten other firms in the same commuting zone, two-digit industry, and size class (greater or less than 500 employees). 6 percent of individuals work for their parent's employer at their first job, while only 0.03 percent of individuals work for the other firms, on average.[11] In other words, individuals are 200 times more likely to work for a parent's employer compared to a similar firm in the same local labor market. Furthermore, Table B.2 shows that 70 percent of individuals who work for a parent's employer live in urban areas and they are no more likely to work for large firms, suggesting that the patterns are not driven by cases in which a single employer dominates a local labor market.

Figure 2 shows that the likelihood of working for a parent's employer is highest in

[11] Across the 10 draws of firms, the minimum and maximum percent of individuals who work for one of these other firms is 0.033 and 0.035 percent, respectively.

industries where the use of labor market networks is most common. Using responses to the first wave of the NLSY97, I calculate the proportion of individuals who were hired by or recommended for their job by a parent. Figure 2 plots this statistic against the proportion of individuals who work for a parent's firm by industry. The correlation between these two measures is positive with regression coefficient of 2.5 (p-value=0.003). Both measures suggest that social connections are less commonly used in the unskilled service sector and more commonly used in the blue-collar sector.

Taken together, these results suggest that individuals who work for a parent's employer do so primarily because of parental connections. This is consistent with Loury (2006), who finds that 10 percent of young men found their current job through a parent, as well as other research emphasizing the role of social connections in the job search and hiring processes (Bayer et al., 2008; Hellerstein et al., 2011; Rajkumar et al., 2022).

5 Earnings Consequences

Among individuals who work for their parent's employer, how much more do they earn at those jobs relative to their next-best option? Estimating this causal parameter is difficult because individuals who work for their parent's employer may be different from those who do not. Section 5.1 highlights this empirical challenge and shows that those who work for a parent's employer are negatively selected. Section 5.2 presents the main results, using an instrumental variables strategy that exploits transitory and idiosyncratic fluctuations in job availability at the parent's employer. Section 5.3 employs an alternative empirical strategy, focusing on young workers displaced during mass layoffs.

5.1 Evidence of Negative Selection

I begin with a sibling comparison to show that those who work for a parent's employer have lower levels of education (i.e., they are negatively selected). I limit the sample to cases in which both an individual and at least one sibling responded to the American Community Survey (ACS) after age 25. Column 1 of Table 1 presents estimates from a regression of an indicator for whether the individual's first job was at a parent's employer on educational attainment (measured in the ACS), controlling for household fixed effects

Table 1: Selection into Working for a Parent's Employer by Educational Attainment

	Works for Parent's Employer	Initial Log Earnings at the First Job	
	(1)	(2)	(3)
High School	-0.009		0.026*
	(0.007)		(0.013)
Some College	-0.029***		0.010
	(0.007)		(0.013)
Bachelor's Degree or Higher	-0.031***		0.136***
	(0.007)		(0.013)
Works for Parent's Employer		0.038***	0.041***
		(0.009)	(0.009)
R-Squared	0.595	0.646	0.651
Observations	83,000	83,000	83,000

Notes: Each column presents estimates from a separate regression. The outcome is defined by the column header and is either an indicator for working for a parent's employer or initial log earnings. All specifications include fixed effects for the quarter in which the young worker starts their first job and fixed effects for the interaction between race, sex, and year of birth. The omitted education category is "less than high school." Standard errors are presented in parentheses.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.1$

as well as fixed effects for the quarter in which the individual found their first job and fixed effects for the interaction between race, sex, and year of birth. Siblings with higher levels of education are significantly less likely to work for a parent's employer: compared to those without a high school degree, those with a bachelor's degree or higher are 3.1 percentage points less likely to work for a parent's employer. This is a large difference, considering that only 3.8 percent of individuals with a bachelor's degree work for a parent's employer. In other words, those without a college degree are almost twice as likely to work for their parent's employer compared to their college-educated siblings.

Columns 2 and 3 of Table 1 regress initial log earnings on an indicator for whether the individual works for their parent's employer with and without controls for educational attainment, respectively. As in Column 1, these specifications also control for the quarter in which the individual found their first job as well as race, sex, and year of birth. Column 2 shows that siblings who work for a parent's employer at their first job earn 3.8 log points more, which rises to 4.1 log points in Column 3 when controlling for educational attainment.[12] The increase in the R-squared between Columns 2 and 3 is modest, and

[12]These estimates are broadly consistent with Kramarz and Skans (2014), who use Swedish data to

raises the possibility of selection on unobserved characteristics. Under the assumption that the full set of observed and unobserved characteristics perfectly predicts the outcome, the restricted estimator developed by Oster (2019) suggests that the change in coefficient and R-squared values between Columns 2 and 3 implies a true treatment effect of 19 log points. In other words, these estimates may be severely downward biased by selection on unobserved characteristics.

Table 1 illustrates that even when comparing siblings within the same household, there is substantial negative selection into working for a parent's employer. Intuitively, young workers with limited outside options are more likely to rely on parental connections to find work. Any empirical strategy must account for this selection.

5.2 Instrumental Variables Estimator

The ideal experiment to estimate the causal effect of working for a parent's employer would randomly prohibit some firms from hiring the children of current employees. Random assignment across firms would serve as an instrument, generating variation in access to jobs at a parent's employer that is orthogonal to other determinants of child outcomes. This design would identify the ATT and allows for the possibility that some children would choose not to work for their parent's firm, and some firms would choose not to offer them a job (i.e., there exist never-takers).

I approximate this ideal experiment using the hiring rate at the parent's firm as an instrument. The intuition is straightforward: when a firm is not hiring, it is less likely to extend a job offer to the child of an existing employee. Two main identification concerns arise. First, hiring rates may be correlated with broader local labor market conditions, which could independently affect young workers' outcomes. Second, parents employed at firms with higher hiring rates may differ systematically from those at firms with lower hiring rates. For example, larger firms may have lower hiring rates and employ workers with higher levels of education.

To address these concerns, I isolate variation in hiring rates that is both transitory and

compare the earnings of young workers who do and do not work for a parent's employer, controlling for observable characteristics. They find small wage losses in the short run, which appear to be offset by stronger wage growth in the medium run.

idiosyncratic to the parent's employer. Specifically, I control for time-invariant differences across firms and time-varying conditions in local labor markets by estimating the following regression via two-stage least squares:

$$\begin{aligned}
&\text{First stage: } D_i = \pi^1 + \gamma Z_{p(i)t-1} + \delta^1_{p(i)} + \lambda^1_{l(p(i),t)} + \phi^1 x_{it-1} + u_i \\
&\text{Second stage: } y_i = \pi^2 + \beta D_i + \delta^2_{p(i)} + \lambda^2_{l(p(i),t)} + \phi^2 x_{it-1} + v_i,
\end{aligned} \tag{4}$$

where individual i starts their first stable job in quarter t. The main dependent variable, y_i, is initial log earnings at the first job. The endogenous variable, D_i, is an indicator for whether i works for their parent's employer at their first stable job. If i has two parents, I assign the parent with the higher average earnings between $t-4$ and $t-1$. The instrument, $Z_{p(i)t-1}$, is the average quarterly hiring rate at the parent's employer $p(i)$ measured in the year before the first job begins (i.e., quarters $t-4$ to $t-1$).[13] Using the lagged hiring rate breaks any mechanical link between the current hiring rate and the decision to join the parent's employer. The specification controls for fixed effects for the parent's employer, $\delta_{p(i)}$; fixed effects for the local labor market in which the parent's employer is located, $\lambda_{l(p(i),t)}$, which is defined by the interaction between the commuting zone, two-digit industry, and quarter; and log parental earnings, x_{it-1}, measured as the average between quarters $t-4$ and $t-1$. Standard errors are clustered at the level of the parent's employer and the sample is restricted to parents with at least one year of tenure and excludes singleton observations.[14]

The identifying variation comes from differences across firms in how hiring rates change over time. The empirical strategy, defined by equation 4, compares children whose parents work for the same firm but who enter the labor market at different times. The first stage tests whether individuals are less likely to work for their parent's employer when that firm is hiring less, and whether this difference is larger at firms experiencing larger declines in hiring. Figures 3 and 4 illustrate the source of the identifying variation.

[13] I measure this using the End-of-Quarter Hiring Rate methodology from the Quarterly Workforce Indicators: the number of new hires who remain with the firm for at least one more quarter, divided by the average of the total employment at the beginning and end of the quarter.

[14] Singletons—observations which have a unique value of a fixed effect—are excluded because they do not contribute to identification and bias the standard error estimates.

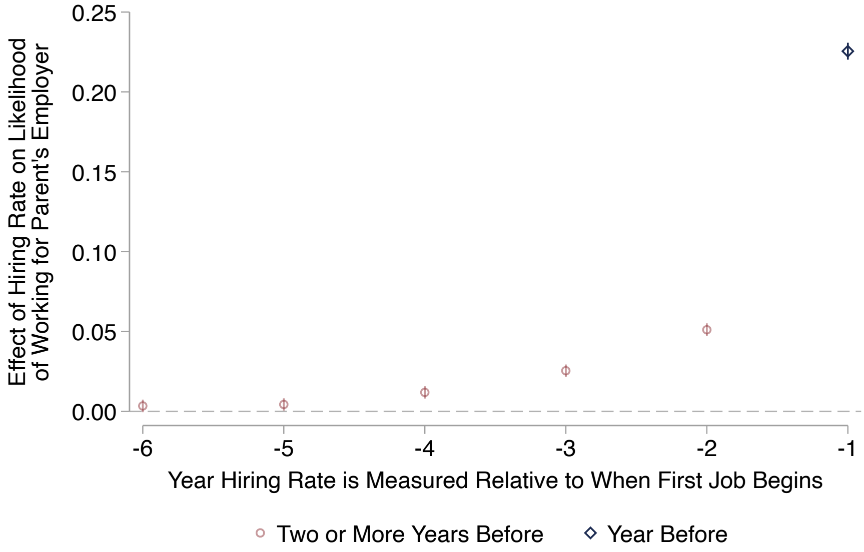
Figure 3 demonstrates that the empirical strategy exploits transitory fluctuations in hiring conditions. Each point represents an estimate from a separate regression, a variation of equation 4 where the hiring rate is measured at different points in time. The blue diamond in Panel A presents first-stage estimates, showing that the hiring rate at the parent's employer in the year before the child starts their first job strongly predicts whether they will work there. A 10 percentage point increase in the hiring rate leads to a 2.3 percentage point increase in the probability of working for the parent's employer. In contrast, when the hiring rate is measured three years earlier, the first-stage coefficient drops to .012—a 95 percent decline relative to the baseline. Panel B shows a similar pattern of decay in the reduced form. These results indicate that what matters for young workers' outcomes is hiring conditions just before they enter the labor market—not conditions further in the past. This illustrates the role of the parental employer fixed effects, which account for time-invariant differences across firms.

Figure 4 shows that the empirical strategy exploits variation in hiring conditions that is idiosyncratic to the parent's employer and orthogonal to broader labor market trends. Each point represents an estimate from a separate regression, with Panels A and B displaying first-stage and reduced-form estimates, respectively. The blue diamonds correspond to the main specification from equation 4 while the red circles represent placebo specifications, where all variables related to the parent's employer are replaced with those of a firm that is observably similar to the parent's employer. Specifically, I sample 10 firms that are in the same commuting zone, two-digit industry, and size class (above or below 500 employees) as the parent's employer. Hiring rates at placebo firms are largely unrelated to young workers' outcomes, highlighting the role of the local labor market fixed effects. Prior research finds that earnings are affected by aggregate economic conditions at the time an individual starts a new job or enters the labor market (Beaudry and DiNardo, 1991; Wachter, 2020). By controlling for local labor market fixed effects, my analysis isolates the effect of working for a parent's employer from the broader impact of finding a first job in strong labor market.

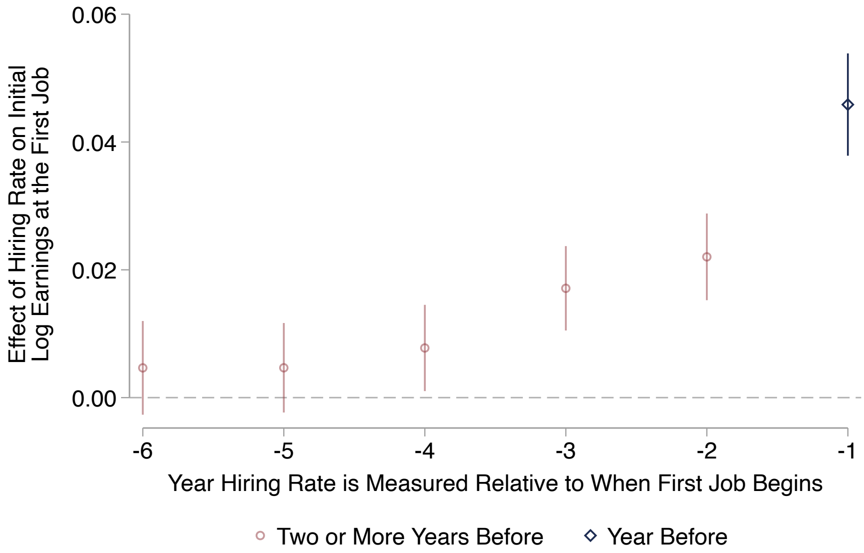
Three assumptions are needed to interpret estimates from equation 4 as causal. The

Figure 3: Association Between Outcomes and Lagged Hiring Rates at Parent's Employer

(A) First Stage



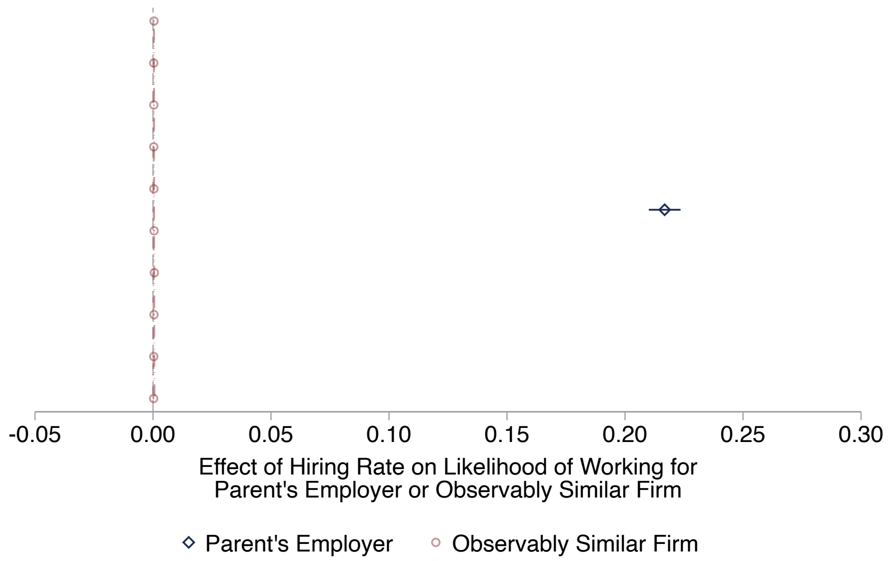
(B) Reduced Form



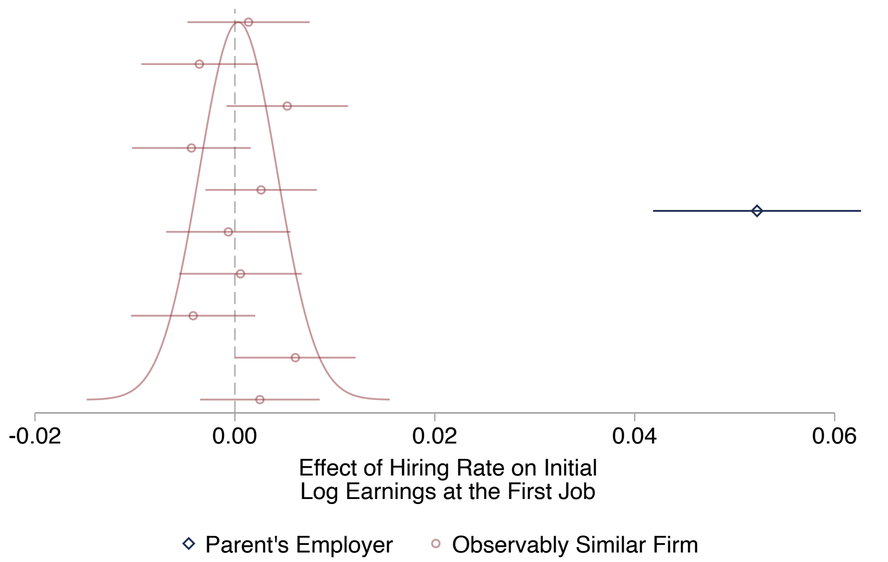
Notes: Each point represents an estimate from a regression of an outcome variable on the hiring rate at the parent's employer. The outcome variable is an indicator for whether the individual works for their parent's employer in Panel A and initial log earnings in Panel B. The horizontal axis defines the time at which the hiring rate at the parent's employer is measured. All regressions include fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. Standard errors are clustered at the level of the parent's employer and the vertical bars represent 95 percent confidence intervals.

Figure 4: Association Between Outcomes and Hiring Rates at Observably Similar Firms

(A) First Stage



(B) Reduced Form



Notes: Each point represents an estimate from a separate regression of an outcome variable on the hiring rate of the parent's employer (blue diamond) or an observably similar firm (red circle). The observably similar firm is matched to the parent's employer based on commuting zone, two-digit industry, and size class (above or below 500 employees). The outcome variable is an indicator for whether the individual works for the parent's employer or the observably similar firm in Panel A and initial log earnings in Panel B. All regressions include fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. Standard errors are clustered at the level of the parent's employer and the vertical bars represent 95 percent confidence intervals. A normal distribution is fitted to the point estimates corresponding to the observably similar firms.

first is relevance: the hiring rate must affect the probability of working for a parent's employer. The second is monotonicity, which requires that the hiring rate weakly increase the probability of working at a parent's employer for all individuals. The third is exogeneity: conditional on the covariates, the hiring rate is related to the earnings of the individual only through its effect on the propensity to work for the parent's employer.

The exogeneity assumption has three key components. The firm-specific, transitory fluctuations in hiring at the parent's employer—i.e., the variation isolated by equation 4—must be unrelated to: (1) labor demand shocks at other firms, (2) changes in wages at the parent's employer, and (3) characteristics of the child—such as education or skills—that have an independent effect on earnings.

Under these assumptions, the instrumental variables estimator identifies a *local average treatment effect* (LATE), which is the average effect for the *compliers*—the population whose treatment status depends on the instrument (Imbens and Angrist, 1994). Section 5.2.2 presents empirical evidence to assess the plausibility of these identification assumptions and Section 5.2.4 explores the relationship between the LATE and the ATT. But before turning to those analyses, Section 5.2.1 presents the main results.

5.2.1 Effect of Working for Parent's Employer on Initial Earnings

Table 2 presents the main estimates from equation 4. Columns 1 and 2 present the first-stage and reduced-form estimates and show that a 10 percentage point increase in the hiring rate at the parent's employer leads to a 2.2 percentage point increase in the probability that the individual works for their parent's employer at their first stable job (the first-stage F-statistic is 6,993) and a 0.47 log point increase in initial earnings. Column 3 presents estimates from the second stage, and shows that working for a parent's employer increases initial earnings by 21 log points, or 24 percent ($24 = (e^{0.212} - 1) \times 100$).

The estimated earnings benefits 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 the same size as the union wage premium (Farber et al., 2021) and one standard deviation of the inter-industry wage premium (Katz et al., 1989). For comparison, the college premium—the relative wage for college versus high school educated workers—is

Table 2: Effect of Working for Parent's Employer on Outcomes at First Job

	Works for Parent's Employer	Initial Log Earnings		AKM Firm Effect
	(1)	(2)	(3)	(4)
Hiring Rate at Parent's Employer	0.220***	0.047***		
	(0.003)	(0.004)		
Works for Parent's Employer			0.212***	0.229***
			(0.018)	(0.014)
First-Stage F-Statistic	6,993		6,993	6,922
Observations in Millions	17.6	17.6	17.6	17.49

Notes: Each column presents estimates from a separate regression corresponding to equation 4. Column 1 presents first-stage estimates, where the dependent variable is an indicator for whether the individual works for their parent's employer at their first stable job, and the main independent variable is the hiring rate at the parent's employer, measured in the four quarters before the first job begins. Column 2 presents reduced-form estimates, where the dependent variable is initial log earnings. Columns 3 and 4 present second-stage estimates, instrumenting for working for the parent's employer using the hiring rate; the dependent variables are initial log earnings and the AKM firm fixed effect of the first employer, respectively. All regressions include fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. Standard errors are clustered at the level of the parent's employer and are presented in parentheses.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.1$

68 log points (Acemoglu and Autor, 2011).

To provide initial evidence on the mechanism, Column 4 of Table 2 estimates the effect on the AKM fixed effect of the first employer.[15] Working for the parent's employer leads individuals to work for firms that pay all workers 23 log points more, which is very similar to the effect on individual earnings in Column 3. I split the sample by the median pay premium of the parent's employer, and find that working for the parent's employer leads to a 25 (2.6) and 10 (2.7) log point increase in initial earnings for individuals whose parents are employed by high- and low-paying firms, respectively (standard errors in parentheses). While there is some debate over how to interpret the AKM fixed effects, these results strongly suggest that the earnings gains are driven by parents providing access to higher-paying firms.[16] Section 5.2.3 explores this point in more detail.

[15]I estimate the AKM firm fixed effect using code adapted from Crane et al. (2022) and based on national data that excludes the young workers in my sample. See Appendix C.3 for details.

[16]Identification of the AKM empirical model places restrictions on the relationship between an unobserved error term and the individual- and employer-level components of earnings, whereas my empirical strategy makes no assumptions about the relationship between these variables. Importantly, the AKM model includes a firm fixed effect for the employer of the individual, whereas equation 4 includes a firm

5.2.2 Assessing the Validity of the Identifying Assumptions

Table 3 presents four sets of analyses that assess the three main components of the exogeneity assumption, defined at the end of Section 5.2.

The first key identification concern is that hiring conditions at the parent's employer may be correlated with broader labor demand shocks. For instance, if parents transmit specific skills to their children, and the parent's employer hires more when there is strong aggregate demand for those skills, the independence assumption would be violated, leading to upward bias in the estimated effects. To address this, the main specification controls for local labor market fixed effects, defined by the interaction of commuting zone, industry, and quarter. These controls absorb time-varying shocks to labor demand within narrowly defined markets. However, they do not account for the possibility that firms competing to hire the same workers might be in different industries or regions.

To assess this concern, I use hiring conditions at the parent's future employer as a proxy for such unobserved labor demand shocks. The intuition is that skill transmission occurs throughout childhood, so the types of skills parents pass on are likely to be valued by both the parent's current and future employers. Since children are unlikely to have connections to the parent's future firm, this strategy isolates variation in demand for particular skills from the effects of parental connections.

I estimate the following equation,

$$y_i = \pi + \gamma Z_{p(i)t-1} + \delta_{p(i)} + \lambda_{l(p(i),t)} + \theta \tilde{Z}_{f(i)t-1} + \tilde{\delta}_{f(i)} + \tilde{\lambda}_{l(f(i),t)} + \phi x_{it-1} + \eta_i, \tag{5}$$

where $f(i)$ is the future employer of i's parent and equation 5 is a modification of the main specification (equation 4) that includes controls for the hiring rate at the parent's future employer, $\tilde{Z}_{f(i)t-1}$, as well as fixed effects for the parent's future employer, $\tilde{\delta}_{f(i)}$, and its local labor market, $\tilde{\lambda}_{l(f(i),t)}$. The future employer is defined as the parent's subsequent employer (for those who eventually change jobs after their child enters the labor market)

fixed effect for the parent's employer. Limited mobility bias implies that the estimated AKM firm effects contain some noise, which creates issues when estimating the variance but does not bias my regression estimates since the AKM firm effect is an outcome variable (Kline et al., 2020; Bonhomme et al., 2023).

and the sample is limited to parents who have a future employer.

Column 1 of Table 3 presents estimates from equation 5 and shows that the initial earnings of the young worker are strongly correlated with the hiring rate at the parent's current employer but unrelated to the hiring rate at the parent's future employer. Columns 2 and 3 present second-stage estimates that omit and include controls for the hiring rate at the parent's future employer, respectively.[17] The estimated effect of working for a parent's employer is unaffected by controlling for the hiring rate at the parent's future employer. These results suggests that the main estimates are not driven by correlated labor demand shocks at other firms.

The second identification concern is that hiring activity at the parent's employer may be correlated with changes in firm-level wages. While fixed effects for the parent's employer absorb time-invariant differences across firms, they do not directly account for the possibility that expanding firms may both hire more workers and raise wages, violating the exclusion restriction and biasing the estimated effect upward. This concern is partly mitigated by the transitory nature of the identifying variation (see Figure 3), as firms are less likely to adjust wage policies in response to short-term hiring fluctuations. Furthermore, the rent-sharing literature finds that productivity shocks lead to only modest wage increases (Card et al., 2018) and Mueller et al. (2023) provide evidence that "firms' wage policies do not constitute an important margin of recruiting effort," a pattern that is particularly plausible for young workers in entry-level positions.

To assess this concern more directly, Column 4 of Table 3 presents estimates from a variant of equation 4 that includes two additional firm-level controls: the employment growth rate and the change in average earnings for incumbent workers, both measured in the quarter before the child starts their first job.[18] The estimated effect of working for a parent's employer is slightly larger in this specification—26 log points compared to 21 log points in the baseline—providing no evidence that the main results are driven by

[17] Both specifications control for fixed effects for the parent's future employer and its local labor market.

[18] The employment growth rate is calculated as the change in employment between quarters $t-5$ and $t-1$, capturing annual growth that is unaffected by seasonal variation. Changes in average earnings are computed using workers continuously employed from $t-8$ to $t-1$, comparing mean quarterly earnings in the periods $t-8$ to $t-5$ and $t-4$ to $t-1$.

Table 3: Validity of Exogeneity Assumption

	Initial Log Earnings at the First Job										Has First Stable Job
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Hiring Rate at Parent's Employer	0.030*										
	(0.016)										
Hiring Rate at Parent's Future Employer	0.007		0.006								
	(0.008)		(0.008)								
Works for Parent's Employer		0.173*	0.171*	0.264***	0.167**	0.159***	0.424*	0.434*	0.240***	0.179*	
		(0.092)	(0.093)	(0.025)	(0.059)	(0.037)	(0.195)	(0.191)	(0.017)	(0.073)	
Hiring Rate at Parent's Employer, at Age 18											0.000
											(0.003)
First-Stage F-Statistic		355.7	354.3	4,044	642.2	1,675	99.99	100.5	7,030	532.1	
Estimator	OLS	IV	IV	IV	IV	IV	IV	IV	IV	IV	OLS
Observations in Millions	2.312	2.312	2.312	17.35	3.21	3.21	.44	.44	17.6	11.8	14.28

Notes: Each column presents estimates from a separate regression corresponding to instrumental variables (IV) esimator defined in equation 4. The outcome variable is defined by the column header. The specifications in Columns 1-9 measure the hiring rate at parents' employer in the four quarters before the first job begins and control for fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. Columns 1-3 additionally control for the hiring rate at the parent's future employer as well as fixed effects for the parent's future employer and the labor market in which that firm is located. Column 1 presents reduced-form estimates and Columns 2 and 3 present second-stage estimates from specifications with and without the control for the hiring rate at the parent's future employer. Column 4 controls for the employment growth rate and the change in average earnings for incumbent workers, both measured in the quarter before the child starts their first job. Column 5 includes household fixed effects and restricts the sample to individuals who have at least one sibling in the sample. Column 6 estimates the baseline specification on the same sample as Column 5. Columns 7 and 8 restrict the sample to individuals who respond to the ACS after age 25. Column 7 estimates the baseline specification while Column 8 adds fixed effects for educational attainment. Column 9 augments the baseline specification to also control for parental earnings rank; marital status of the parents; and fixed effects for the interaction between race, sex, and year of birth. The specifications in Columns 10 and 11 measure the hiring rate at parents' employer in the year the child turns 18 and control for fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and the year in which the child turns 18; and log parental earnings, measured in the year the child turns 18. Column 10 presents second-stage estimates. Column 11 expands the sample to include individuals who never find a first stable job and presents reduced-form estimates. The reduced-form specifications in Columns 1 and 11 are estimated via ordinary least squares (OLS). Standard errors are clustered at the level of the parent's employer and are presented in parentheses.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.1$

expanding firms offering higher wages.

The third identification concern is that hiring conditions might be correlated with characteristics of the child—such as education or skills—that independently affect earnings. One way in which this could happen is if parents dynamically sort across firms in a way that lead to a correlation between the hiring rate and the characteristics of the employees. For example, if firms respond to negative shocks by reducing hiring and retaining only higher-skilled workers, parents employed during periods of low hiring will tend to be more educated.

I address the concern that parents dynamically sort across firms using a siblings comparison. Column 5 of Table 3 presents estimates from a modification of equation 4, which includes household fixed effects and Column 6 presents estimates from the baseline specification (equation 4). Both specifications are estimated on the sample of individuals who have at least one sibling. I find similar earnings gains when controlling for fixed effects for the household (16.7 log points) and the parent's employer (15.9 log points), which rules out all confounders, observed or unobserved, that are common to siblings within the same family.

I use education data from the ACS to directly test whether the hiring rate is correlated with child characteristics. I restrict the sample to individuals who respond to the ACS after age 25—after most educational and early labor market decisions have been made. Columns 7 and 8 of Table 3 show that the estimated earnings effects are nearly identical with and without controls for the child's educational attainment. I also find no effect when education is the outcome: working for a parent's employer leads to a statistically insignificant 3 percentage point reduction (p-value=0.87) in the probability of obtaining a bachelor's degree. Column 9 augments the baseline specification to include controls for parental earnings rank; marital status of the parents; and fixed effects for the interaction between race, sex, and year of birth. The main results are largely unaffected by the inclusion of these controls. Thus, observable characteristics of the child appear to be unrelated to hiring conditions at the parent's employer.

A spurious correlation between hiring conditions and child characteristics could also arise if parental connections influence not only where, but also when—or even whether—a child enters the labor market. This raises the possibility that measuring the instrument just before labor market entry could introduce endogeneity, if the timing of entry itself is affected by parental connections. To address this concern, Column 9 of Table 3 presents estimates from a specification in which the instrument is the average quarterly hiring rate at the parent's employer during the year the child turns 18, which is before most children have entered the labor market. In this setup, the instrument is measured independently of the actual timing of labor market entry.[19] The first-stage relationship is weaker, with an F-statistic of 532, so the estimates are less precise. But the results continue to show a substantial earnings gain of 18 log points. Column 10 provides a further check, expanding the sample to include all young workers—regardless of whether they ever find a first stable job—and estimates a reduced-form regression where the outcome is an indicator for ever finding a first job. The hiring rate at the parent's employer in the year the child turns 18 has no predictive power for this outcome.

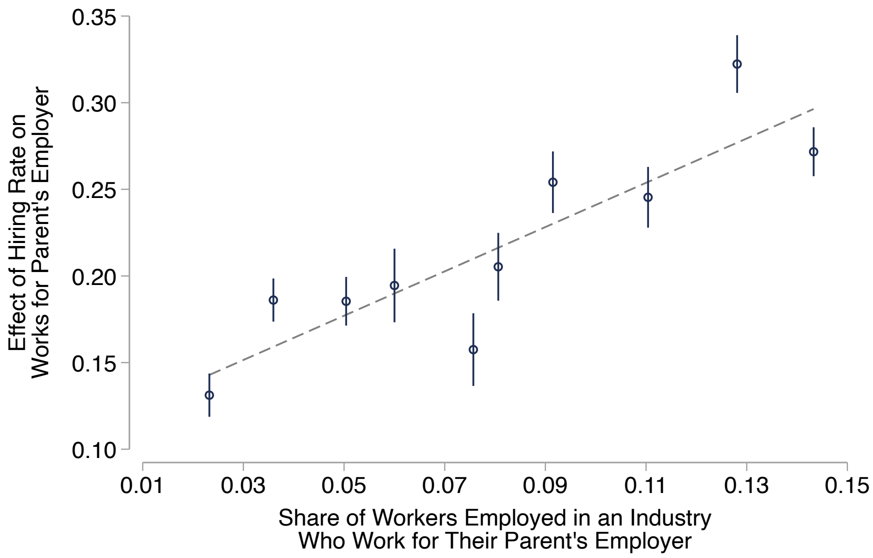
As a final test of the identification assumptions, I examine whether the results are stronger in industries where hiring through social networks is more common. Motivated by Figure 2—which illustrates that there is substantial variation across industries in the likelihood that young workers to find jobs through parental connections—I explore heterogeneity in effects by industry. To do so, I calculate the share of individuals whose first job is at a parent's employer for each three-digit industry and group industries into deciles based on this measure.

Panel A of Figure 5 presents first-stage estimates from a variant of equation 4 that interacts the hiring rate instrument with industry decile. The results show that in industries where family-based hiring is more common, increases in the hiring rate lead to larger increases in the probability that a child works for their parent's employer. The first-stage estimates are positive across all deciles, providing support for the monotonicity assumption.

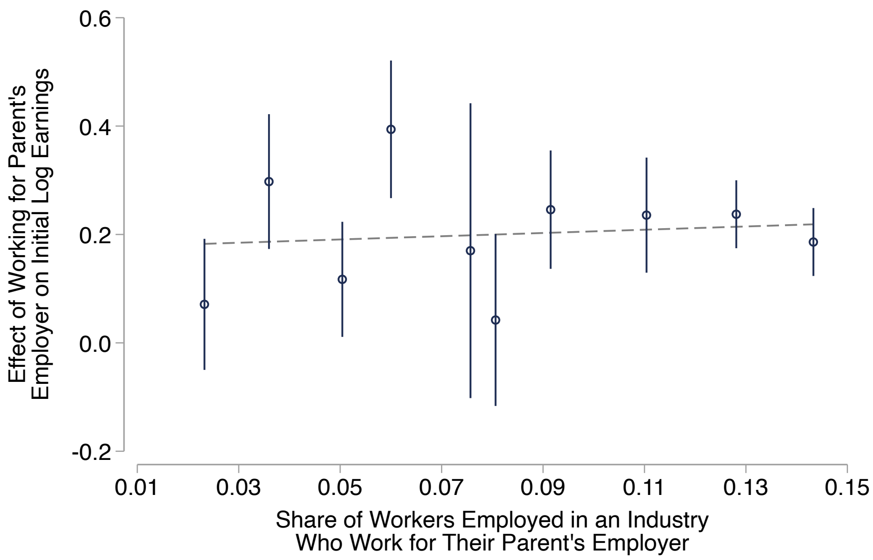
[19] The local labor market fixed effects are also defined based on the parent's employer when the child is 18. The sample includes individuals whose parent is employed at the same firm between the ages of 18 and 22, which is the five-year window in which the most children enter the labor market.

Figure 5: Heterogeneity by Industry-Level Measures of the Use of Parental Connections

(A) First Stage



(B) Second Stage



Notes: Industries are grouped into deciles based on the share of young workers in that industry who work for a parent's employer. Panel A presents first-stage estimates and regresses an indicator for whether the individual works for their parent's employer on the interaction between the hiring rate at the parent's employer and the industry decile of to parent's employer. The estimated coefficients are plotted against the share of workers in that industry decile who work for their parent's employer. Panel B presents the analogous second-stage estimates, where the outcome variable is initial log earnings. Both regressions include fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. Standard errors are clustered at the level of the parent's employer and the vertical bars represent 95 percent confidence intervals.

Panel B of Figure 5 presents second-stage estimates of the effect of working for a parent's employer on initial earnings by industry decile. The results show no systematic relationship between the size of the earnings gain and the prevalence of family-based hiring. This pattern is reassuring, as it helps rule out violations of the independence assumption that apply to industries in which family-based hiring is less common. Intuitively, if the reduced-form relationship between earnings and the hiring rate were attributable to factors unrelated to working for a parents employer—such as aggregate labor market conditions—then we would have expected to see larger second-stage estimates in industries where the use of parental connections is rare.

This section addressed the most plausible threats to identification, including concerns about correlated labor demand shocks, changes in firm-level wages, sorting of parents across firms, and endogenous timing of labor market entry. While I cannot rule out all sources of potential bias, the results are consistent across a range of specifications designed to test the key identifying assumptions. To further strengthen the credibility of the findings, Section 5.3 employs an alternative empirical strategy that examines the outcomes of young workers displaced during a mass layoff—providing a complementary research design that relies on an entirely distinct set of identification assumptions. But before doing so, Section 5.2.3 presents additional evidence on the mechanisms.

5.2.3 Mechanisms and Other Results

There are two primary channels through which working for a parent's employer could impact earnings. First, parental connections may provide access to higher-paying firms—firms that offer higher wages to all employees. Such access could be facilitated through information sharing about job opportunities (Mortensen and Vishwanath, 1995) or through preferential hiring practices. Second, firms may offer higher wages to the children of current employees, relative to otherwise similar workers. This could reflect reduced information asymmetries between the employer and applicant (Montgomery, 1991), or productivity gains from working alongside a parent (Heath, 2018). Table 2 provides evidence for the first mechanism, showing that parental connections increase access to firms with higher AKM firm effects. Table 4 builds on this result and examines impacts on additional characteristics of the firms where individuals begin their careers.

Parents provide access to firms in higher-paying industries. Column 1 of Table 4 shows that working for a parent's employer increases the two-digit industry-level pay premium of the child's first employer by 14 log points.[20] This accounts for 67 percent of the increase in individual-level earnings. Columns 2 through 4 examine broader industry groupings. Working for a parent's employer reduces the likelihood of working in the unskilled service sector by 42 percentage points and increases the likelihood of working in the blue-collar sector by 37 percentage points. To the extent that young workers are aware of pay differences across industries, these results are difficult to reconcile with a model in which parents simply provide general advice about where to apply. Instead, they point to a more targeted mechanism in which parental connections provide access to specific, higher-paying job opportunities.

A wide class of models illustrate how search and matching frictions lead to dispersion in firm-level pay policies (Burdett and Mortensen, 1998; Postel-Vinay and Robin, 2002). Consistent with this class of models, Columns 5 and 6 of Table 4 illustrate that working for the employer of a parent leads individuals to start their careers on a higher rung of the firm job ladder as defined by average earnings and the proportion of hires made through poaching flows.[21] Column 7 shows 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) find 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 8 indicates that working for a parent's employer leads individuals to work for younger firms.

The outcome in Column 9 of Table 2 is the child's earnings rank within their first employer. Here the effect is negative, suggesting that, while parents provide access to higher-paying firms, they do not provide access to relatively high-paying jobs within firms.

[20]Industry pay premiums are estimated using national data excluding the young workers in my sample and regressing log earnings on individual and industry fixed effects. See Section C.3 for details.

[21]The outcomes in Columns 5 and 6 correspond to the rank of time-invariant characteristics of the first employer relative to the national distribution of firms. For examples of papers that use similar measures to define job ladders see Bagger and Lentz (2019) and Haltiwanger et al. (2018).

Table 4: Effect of Working for the Parent's Employer on the Characteristics of the Child's First Employer

		Sector			Firm Ranking				
	Industry Premium (1)	Skilled Services (2)	Unskilled Services (3)	Blue-Collar (4)	Average Earnings (5)	Poaching Hires (6)	Log Firm Size (7)	Firm Age (8)	Within-Firm Earnings Rank (9)
Works for Parent's Employer	0.14***	0.05**	-0.42***	0.37***	22.04***	7.72***	-1.21***	-3.15***	-6.45***
	(0.01)	(0.02)	(0.02)	(0.02)	(1.10)	(0.99)	(0.10)	(0.53)	(1.06)
Mean	-0.13	0.37	0.47	0.16	43.80	54.50	5.66	23.62	39.30
Standard Deviation	0.16	0.48	0.50	0.37	27.00	23.30	2.47	12.78	24.70
First-Stage F-statistic	6,993	6,993	6,993	6,993	6,993	6,996	6,993	6,993	6,929
Observations (Millions)	17.60	17.60	17.60	17.60	17.60	17.60	17.60	17.60	17.14

Notes: This table presents second-stage estimates from the two-stage least squares specification defined in equation 4. The outcomes in Columns 1 through 8 are characteristics of the individual's first employer and are defined by the column header. The outcome in Column 9 is the individual's earnings rank within their first employer. All regressions include fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. Standard errors are clustered at the level of the parent's employer and are presented in parentheses. The mean and standard deviation of the outcome variable corresponds to the sample of individuals who do not work for their parent's employer.
*** $p\leq0.001$, ** $p\leq0.01$, * $p\leq0.1$

Table 5: Effect of Working for the Parent's Employer on Job Mobility and Earnings Three Years After Entry

	Type of Job Transition			Annual Earnings After Entry		
	Stay (1)	J2J (2)	J2N (3)	One Year (4)	Two Years (5)	Three Years (6)
Works for Parent's Employer	0.20***	-0.15***	-0.04*	6,683***	6,784***	5,566***
	(0.02)	(0.02)	(0.02)	(646)	(843)	(954)
Mean	0.36	0.41	0.23	26,700	26,800	28,100
Standard Deviation	0.48	0.49	0.42	15,900	20,300	22,700
First-Stage F-statistic	5,883	5,883	5,883	5,883	5,884	5,883
Observations (Millions)	14.98	14.98	14.98	14.98	14.98	14.98

Notes: This table presents second-stage estimates from the two-stage least squares specification defined in equation 4. The outcomes in Columns 1 through 3 are indicators three different types of job transitions: (Stay) remained continuously employed at the parent's employer for three years, (J2J) changed jobs without experiencing a full quarter of nonemployment, and (J2N) separated from the parent's employer and experienced at least one full quarter of nonemployment. The outcomes in Columns 4 through 6 are annual earnings in the one to three years after the individual begins their first job. All regressions include fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. Standard errors are clustered at the level of the parent's employer and are presented in parentheses. The mean and standard deviation of the outcome variable corresponds to the sample of individuals who do not work for their parent's employer.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.1$

Working for a parent's employer leads individuals to stay at their first employer longer. Column 1 of Table 5 indicates that working for a parent's employer increases the probability of remaining at the first employer for at least three years by 20 percentage points. Columns 2 and 3 illustrate that this effect is driven by a reduction in the probability of making a job-to-job transition as opposed to affecting the probability of making a job-to-nonemployment transition. If the outcomes in Columns 2 and 3 are viewed as proxies for quits and layoffs, respectively, then these results suggest that parental connections provide access to firms that are more desirable than the child's outside option.

Columns 4 through 6 of Table 5 illustrate that the earnings benefits are quite persistent. Working for a parent's employer increases annual earnings in the first year of the job by $6,683. Three years later, those who started their careers at their parent's employer earn $5,566 more. These effects are consistent with parent's providing access to jobs on a higher rung of the firm job ladder. The initial gains are large, but they slowly fade over time as other young workers move to higher-paying jobs and experience slightly stronger earnings growth.

5.2.4 Interpreting the Local Average Treatment Effect

Does the instrumental variables estimator identify average effects for a narrow subgroup, or does it provide estimates that are broadly representative of average effects for all young workers who use parental connections? In other words, how does the LATE compare to the ATT?

I investigate this question by residualizing the hiring rate at the parent's employer on the set of covariates defined in equation 4. I then create three binary instruments that are equal to one if the residualized hiring rate is larger than the 25th, 50th, and 75th percentiles, respectively. Panel A of Table B.3 presents second-stage estimates using the binary instruments. The estimated earnings gain are 25, 26, and 22 log points when using the three different instruments. Panel B presents first-stage estimates, which imply that 16, 8, and 5 percent of those who work for a parent's employer are in the complier population.[22] These results show that compliers constitute a meaningful share of the

[22]Complier shares are calculated by multiplying the first-stage coefficient by the fraction of the sample with an instrument value equal to one, and dividing by the overall probability of treatment.

treated population and that treatment effects are relatively stable across the distribution of the instrument.

To explore treatment effect heterogeneity in more detail, I also residualize initial log earnings and the indicator for working for parent's employer on the set of covariates defined in equation 4. I then construct ventiles based on the residualized hiring rate. Figure A.5 plots the average values of the residualized log earnings against the average value of the residualized treatment dummy for each ventile.[23] The linear fit replicates the second-stage estimates in Table 2. Consistent with Table B.3, there is little heterogeneity in the slope across the hiring rate distribution: a marginal increase in the probability of treatment induced by an increase in the hiring rate leads to a roughly constant increase in initial earnings. This suggests that compliers at different parts of the hiring rate distribution have similar average treatment effects. Furthermore the range on the horizontal axis shows that increasing the hiring rate from the 5th to the 95th percentile raises the probability of working for a parent's employer by 2.8 percentage points.

Taken together, the lack of treatment effect heterogeneity across the instrument distribution, combined with the non-trivial size of the complier population, provides suggestive evidence that the LATE is a reasonable approximation of the ATT in this setting.

Appendix D.3 develops a theoretical framework to better understand the link between the LATE and ATT. The key insight is that this is a setting with two-sided selection: working for the parent's employer depends on decisions made by both the young worker and the firm. The hiring rate instrument shifts the firm's willingness to make a job offer, while the earnings gains enter the child's decision to accept that offer. Under plausible conditions, this two-sided selection process breaks the link between the instrument and the treatment effects. Appendix D.3 identifies conditions under which the compliers and the treated are a random sample of individuals who would accept an offer from their parent's employer and show that these conditions imply the LATE is equal to the ATT.

The theoretical and empirical evidence suggests that the LATE is a reasonable approximation of the ATT, though some uncertainty remains about the magnitude of any

[23] Figure A.6 visually displays the first-stage and reduced-form estimates by plotting the average residuals for the treatment indicator and log earnings against the ventile of the residualized hiring rate.

differences between these two parameters. The next section presents estimates based on alternative, complementary empirical strategy. Comparing the two sets of estimates provides another way to assess the external validity of the LATE.

5.3 Alternative Empirical Strategy Using Mass Layoffs

This section presents estimates from an alternative empirical strategy, which compares young workers who are displaced during a mass layoff. The approach is motivated by the displaced worker literature, which has used mass layoffs as an exogenous source of job loss (Jacobson et al., 1993). The results complement the main estimates in Section 5.2, as they rely on entirely distinct identification assumptions.

Following the literature, I identify a mass layoff event as a sharp and persistent contraction in firm size. Specifically, firm j experiences as mass layoff in quarter t if firm size, E_{jt}, declines by at least 30 percent in the following and subsequent year (i.e., $(E_{jk} - E_{jt})/E_{jt} < .3$ for $k \in \{t+4, t+8\}$). To address measurement issues, I require that the firm has between 50 and 5,000 employees in quarter t and I drop cases that are likely driven by changes in the firm identifier.[24] I define displaced workers as a subset of the intergenerational sample who have at least one year of tenure at firm j in quarter t, who separate during the mass layoff event, and who find a new job within two years. If an individual appears multiple times in the sample, I retain the first instance.

I estimate the following regression via ordinary least squares to compare the outcomes of young workers who are displaced in the same mass layoff event but subsequently take jobs either at their parent's employer or at a different firm:

$$\Delta y_{it+1} = \alpha + \beta D_{it+1} + \phi X_{it} + \lambda_{j(i,t)} + u_{it+1}, \tag{6}$$

where individual i is employed at a firm that experiences a mass layoff in quarter t and subsequently finds a new job in $t+1$; D_{it+1} is an indicator for whether i joins their parent's employer in their first job after displacement (as before, I focus on the parent

[24]Firms are dropped from my sample if any worker who is there at time t experiences a change in the firm identifier (the "SEIN") but not a change in the job identifier (the "fid"). If a firm experiences multiple mass layoffs I only retain the first event.

who is the primary earner); Δy_{it+1} is the difference between the log of quarterly earnings at the new job after displacement and the average log earnings in the four quarters prior to displacement; X_{it} is a vector of covariates defined prior to displacement, including the average of the outcome (e.g., log earnings) in the four quarters prior to displacement, parental earnings rank, fixed effects for age, and fixed effects for the interaction between race, sex, and year of birth; and $\lambda_{j(i,t)}$ is a fixed effect for the employer from which i was displaced in quarter t. Standard errors are clustered at the level of firm, $j(i,t)$. This specification takes the form of the estimator discussed by Dube et al. (2023).

The key identification assumption is that displaced workers who join their parent's employer are not systematically different from their observably similar displaced co-workers who do not. Two features of the research design lend credibility to this assumption. First, by comparing individuals employed at the same firm prior to displacement and controlling for pre-displacement earnings and demographic characteristics, the analysis accounts for a wide range of pre-displacement differences that could shape subsequent outcomes. Second, comparing workers displaced during the same mass layoff event mitigates concerns about endogenous job mobility and differential exposure to local economic conditions, as all workers are forced to search for new jobs in the same labor market.

Column 1 of Table 6 presents estimates from a bivariate regression of y_{it} on D_{it+1} and shows that displaced workers who are subsequently re-employed at their parent's employer earn 12.9 log points less prior to displacement. These results are consistent with Table 1 and show that individuals who find jobs through parental connections are negatively selected. Column 2 adds the firm fixed effect, $\lambda_{j(i,t)}$, and the difference falls to 3.7 log points, which shows that there is significantly less selection when comparing workers within the same firm.

Column 3 of Table 6 presents estimates from a regression of Δy_{it+t} on D_{it+t} and $\lambda_{j(i,t)}$ and shows that displaced workers who subsequently find new jobs at their parent's employers experience a 22 log point gain in earnings relative to those who do not. Column 4 presents estimates from equation 6 and adds the full vector of covariates. The estimated earnings gains are quite stable at 21 log points, or 23 percent ($23 = (e^{0.208} - 1) \times 100$).

Table 6: Analysis of Displaced Workers

	Pre-Displacement Log Earnings		Change in Log Earnings		Change in AKM Firm Effect
	(1)	(2)	(3)	(4)	(5)
Works for Parent's Employer	-0.129***	-0.037***	0.223***	0.208***	0.188***
	(0.012)	(0.010)	(0.017)	(0.015)	(0.005)
Employer Fixed Effects		X	X	X	X
Individual-Level Covariates				X	X
Observations	313,000	313,000	313,000	313,000	308,000

Notes: Each column presents estimates from a separate regression estimated via OLS. The outcome variable is defined by the column header and the main dependent variable is an indicator for whether the individual joined their parent's employer in their first job after displacement. Columns 2 through 5 control for a fixed effect for the employer from which the individual was displaced. Columns 4 through 5 additional control for the average of the outcome variable in the four quarters prior to displacement, parental earnings rank, fixed effects for age, and fixed effects for the interaction between race, sex, and year of birth. The sample includes young workers displaced by a mass layoff. Standard errors are presented in parentheses.
*** p≤0.001, ** p≤0.01, * p≤0.1

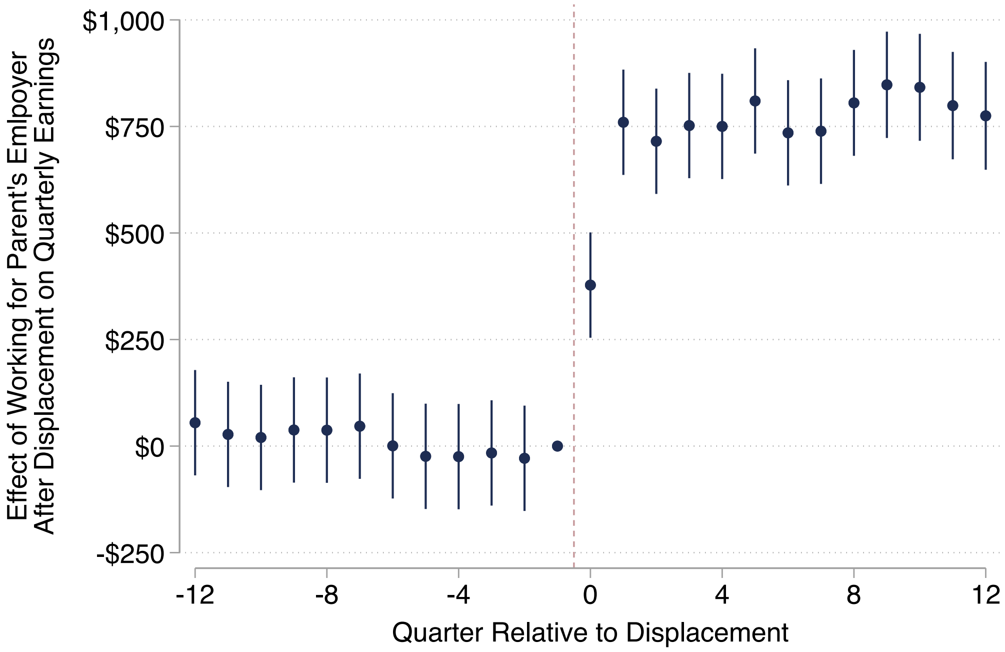
Column 5 shows that working for a parent's employer leads individuals to work for firms that pay all workers 19 log points more, which is very similar to the estimated impacts on individual earnings.

Equation 6 controls for level-differences in earnings prior to displacement, but it is possible that workers may be on differential trajectories. To investigate this, I estimate the following event-study specification:

$$y_{it} = \alpha_i + \sum_{k \neq -1} D_{it}^k \beta^k + \phi_{jt} + \gamma X_{it} + u_{it}, \tag{7}$$

where i is the individual, t is the quarter relative to displacement, j is the employer prior to displacement, D_{it}^k is equal to one if the individual joined their parent's employer at their first job after being displaced and the displacement occurred k quarters ago as of quarter t, ϕ_{jt} is a fixed effect defined by the interaction between employer prior to displacement and the quarter relative to displacement, X_{it} is a quadratic in age, and u_{it} is a regression residual clustered at the level of j. The estimation sample is a balanced panel that includes the twelve quarters before and after the displacement event (inclusive of quarters with zero earnings).

Figure 6: Analysis of Displaced Workers, Event-Study Specification



Notes: The figure represents estimates from the event-study specification described in equation 7, which regresses individual log earnings on the interaction between quarter relative to award and an indicator for whether the individual finds a job at the parent's employer after displacement. The specification controls for a quadratic in age and fixed effects for the employer prior to displacement. The sample includes young workers who were displaced by a mass layoff and is a balanced panel containing data 12 quarters before and after the mass layoff event. Standard errors are clustered at the level of the mass layoff event and the vertical bars denote the 95 percent confidence interval.

Figure 6 presents the estimates from equation 7 and shows that individuals who join their parents employers after being displaced earn significantly more than those who do not. The pre-displacement estimates show no sign of differential pre-trends leading up to the displacement, which supports the validity of the research design.[25]

Taken together, these results strengthen confidence in the main findings, as the instrumental variables strategy and the displaced workers analysis rely on distinct identification assumptions yet reach the same conclusion: working for a parent's employer significantly increases earnings, by providing access to higher-paying firms.

[25]The displaced workers separate in quarter 0 but could have earnings at the origin firm in this quarter. For this reason, the earnings losses are largest in quarter 1, which is the quarter after the job separation occurs. This pattern is apparent in Figure A.7, which presents the raw average quarterly earnings before and after displacement for individuals who do and do not join their parent's employer.

6 Intergenerational Persistence in Earnings

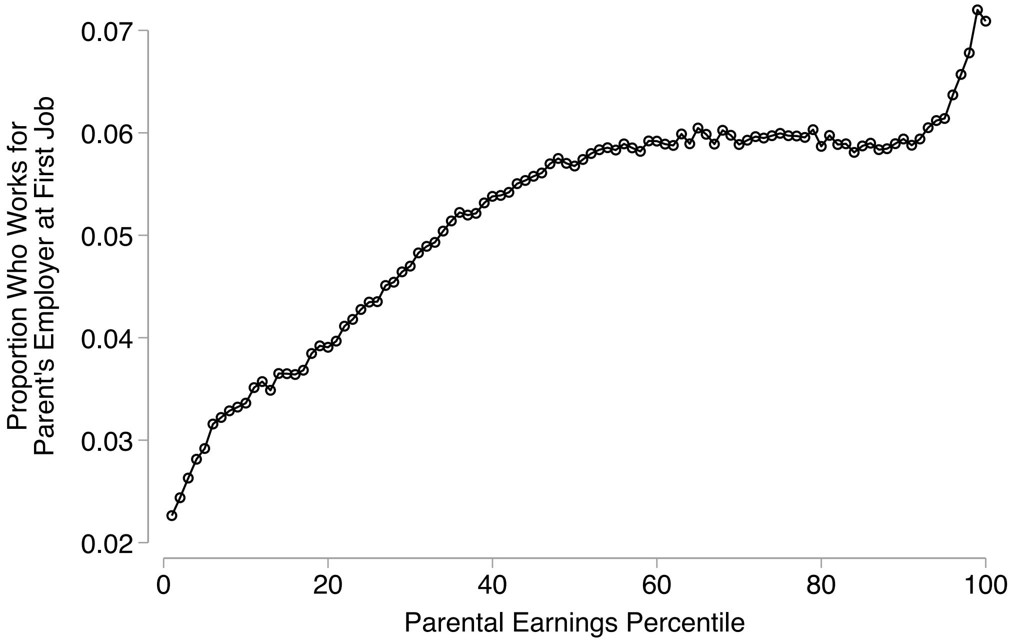
Sections 4 and 5 show parental connections provide access to higher-paying job opportunities. The implications for intergenerational mobility depend on whether individuals from high- or low-income families benefit more. This section documents how the benefits vary across the parental earnings distribution and uses the methodology from Section 2 to quantify how the intergenerational persistence in earnings would change if no one worked for their parent's employer.

Individuals with higher-earning parents are more likely to work for their parent's employer. Figure 7 presents the proportion of individuals who work for a parent's employer for each percentile of the parental earnings distribution. Only 2 percent of individuals with parents at the bottom percentile of the earnings distribution work for a parent's employer. In contrast, 7 percent of individuals with parents at the top percentile of the earnings distribution work for a parent's employer. I find similar disparities looking at longer run measures. Figure A.8 shows that 31 percent of individuals whose parents are in the top decile of the earnings distribution work for a parent's employer at some point between the ages of 16 and 30, compared to 25 percent for the bottom decile.

One explanation for why individuals with higher-earning parents are more likely to work for their parent's employer is that their parents are more likely to be employed and hold a position of authority within the firm. The percent of individuals who have an employed parent when they find their first job rises steeply from 42 percent to 63 percent between the 1st and 20th percentiles of the parental earnings distribution and eventually plateaus at 85 percent. The percent of individuals who have a parent who is a top earner at their firm rises gradually from 3 to 14 percent between the 1st and 90th percentiles of the parental earnings distribution and then rises steeply to 33 percent in the top percentile. 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 or is a top earner within their firm.[26]

[26]Figure A.9 presents these results in detail by plotting the proportion of parents that are employed and that are top earners within their employer against the percentile of parental earnings.

Figure 7: Works for Parent's Employer by Parental Earnings

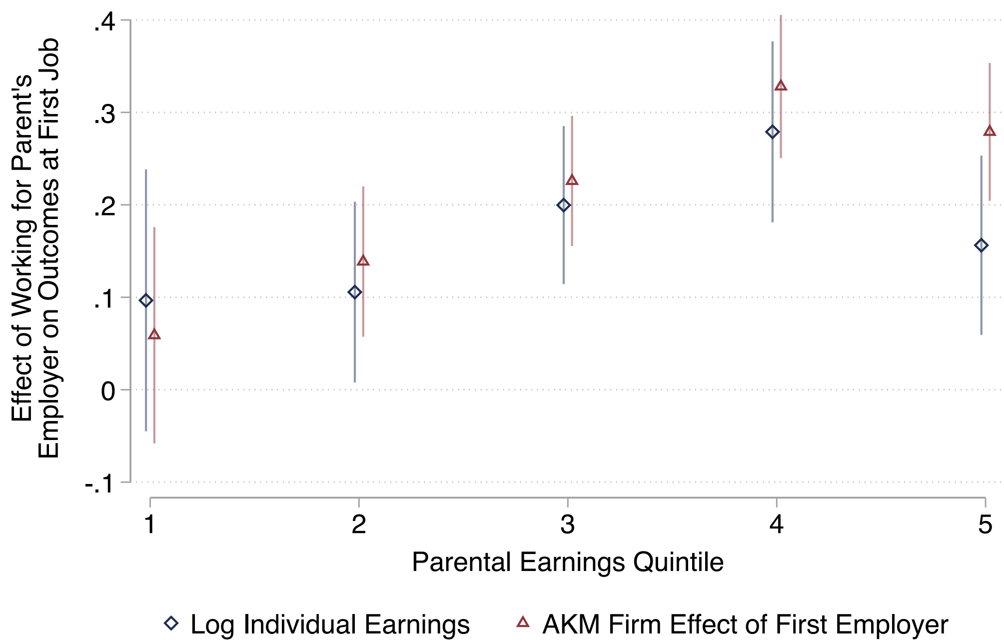


Notes: The figure plots the proportion of individuals whose first stable job is at the same firm as either parent for each percentile of the parental earnings distribution.

Individuals with higher-earning parents experience larger earnings gains from working for a parent's employer. Figure 8 presents estimates from the main instrumental variables specification estimated on five distinct samples defined by the quintile of the parental earnings distribution. Working with a parent in the bottom quintile of the earnings distribution leads to a statistically insignificant 10 log point increase in initial earnings. In contrast, working with a parent in the fourth and fifth quintile of the earnings distribution leads to a 28 and 16 log point increase in earnings, respectively. The estimated effects on the AKM firm effect follow a similar pattern. Panels A and B of Table B.4 present estimates by the quintile of parental earnings using the instrumental variables estimator and the mass layoff estimator, respectively. The two empirical strategies yield quantitatively similar results: individuals with higher-earning parents experience larger gains in both individual earnings and the AKM firm effect.

Table 2 shows that working for the employer of a parent who is the primary earner in the household leads to a 21.2 log point increase in initial earnings. When I repeat this analysis for the parent who is the secondary earner, I find a similar earnings gain of

Figure 8: Effect on Initial Outcomes at First Job by Parental Earnings



Notes: Each point represents an estimate of the effect of working for a parent's employer based a separate regression, which is estimated on one of five distinct samples defined by the quintile of parental earnings. The outcome is either individual log earnings at the first stable job or the AKM firm effect associated with the first employer. The instrumental variables specification, defined in equation 4, uses the hiring rate at the parent's employer as an instrument and controls for fixed effects for the parent's employer; fixed effects for the local labor market (defined by the interaction between commuting zone, industry and quarter); and log parental earnings, measured in the year before the child begins their first job. Standard errors are clustered at the level of the parent's employer and the vertical bars denote the 95 percent confidence intervals.

20.7 log points (p-value$<$0.001). Thus, for the counterfactual exercise, I assume that the earnings gains of using parental connections are the same for both parents.

What are the implications for the IGE? Column 1 of Table 7 presents estimates of the IGE, which is the coefficient from a regression of the initial log earnings of the child at their first job on the log earnings of their parents. The estimated IGE of 0.136 is substantially lower than other estimates in the literature (Black and Devereux, 2010).[27] To understand why, Table B.6 presents alternative estimates of the IGE. The IGE is 0.482 when the earnings of the child is measured between the ages of 29 and 31 and one is added

[27]Figure A.10 presents a visual representation of the IGE by plotting the average log earnings at the first job against the average log earnings of the parents for each percentile of the parental earnings distribution. The flatter slope at the bottom of the parental earnings distribution is likely attributable to the fact that, by construction, everyone in my sample has a stable job.

Table 7: Changes in Counterfactual Intergenerational Elasticity of Earnings

	Observed IGE	Percent Change in IGE if No One Worked for Parent's Employer			
	(1)	(2)	(3)	(4)	(5)
Estimate	0.136	-11.5%	-9.0%	-10.1%	-7.9%
	(0.0001)	(3.8)	(2.9)	(3.5)	(2.7)
Empirical Strategy Used to Estimates Earnings Gains		IV	IV	Mass Layoff	Mass Layoff
Assume 20 Percent Would Have Gotten Job Absent Connections			X		X

Notes: Column 1 presents the observed IGE, which is the coefficient from a regression of the log initial earnings of the young worker on the log earnings of the parent. Columns 2 through 5 use the methodology described in Section 2 to compute the estimated change in the IGE if no one worked for a parent's employer, which is presented as percent change relative to the observed IGE. Columns 2 and 3 use the instrumental variables strategy to estimate the earnings consequences. Columns 4 and 5 use the mass layoff estimator. Columns 3 and 5 assume that 20 percent of individuals who work for a parent's employer would have gotten these jobs absent connections, and multiplies the share of individuals who work for a parent's employer by 0.8.

to earnings to include zeros. The IGE drops to 0.162 if I then omit children whose average quarterly earnings between ages 29 and 31 do not exceed $3,300 (the same restriction used to define the first stable job).[28] These patterns demonstrate that the lower IGE observed in my sample is primarily due to the fact that the sample is restricted to children who are employed. It is worth emphasizing that estimates of the intergenerational persistence in earnings typically use long-run measures of earnings for both parents and children, whereas I focus on initial labor market outcomes of the children.

Column 2 of Table 7 shows that the IGE would be 11.5 percent lower if no one worked for a parent's employer. This estimate is constructed using the methodology described in Section 2 in combination with the descriptive statistics in Figure 7 and instrumental variables estimates in Figure 8. Standard errors are calculated using the delta method and take into account the uncertainty in the estimates of the earnings consequences. These findings are the intuitive consequence of the fact that young workers with higher-earning parents benefit more from parental connection because they are more likely to work for a parent's employer and they experience larger earnings gains when they do.[29]

[28]Column 4 of Table B.6 shows that I find a similar IGE if I measure parental earnings as the average earnings in the years when the child was between the ages of 16 and 18.

[29]Figure A.10 presents a visualization of these results and compares the observed IGE to the counter-

My conclusions are robust to making conservative adjustments for the possibility that some individuals would have worked for these firms absent parental connections. The analysis of the parent's future employers in Figure 1 demonstrated that up to 20 percent of individual who work for a parent's employer would have gotten the job absent parental connections. Column 3 of Table 7 shows that if I adjust the probability of working for a parent's employer accordingly—i.e., replace $\mathbb{E}[D_i]$ with $0.8 \times \mathbb{E}[D_i]$—the counterfactual IGE would be 9 percent lower.

Column 4 of Table 7 shows that I arrive at similar conclusions if I instead use estimates of the earnings consequences based on the mass layoff analysis described in equation 6 and presented in Panel B of Table B.4. Specifically, the IGE would be 10.1 percent lower. Column 5 shows that this estimate drops to 7.9 percent after adjusting for the probability that some individuals might have worked for their parents' employers absent connections.

Parental connections also amplify the initial gender pay gap. At their first job, young men earn 7 log points more than young women, on average. Table B.5 presents the estimated earnings gains of working for a parent's employer based on the instrumental variables strategy for four samples defined by the sex of the child and the parent. Working for the father's employer leads to a 28 and 23 log point increase in initial earnings for daughters and sons, respectively. Working for a mother's employer leads to an 18 and 30 log point earnings gain for daughters and sons, respectively. Furthermore, sons are 2.6 times as likely to work for their father's employer while daughters are 1.3 times as likely to work for their mother's employer. These estimates imply that the initial gender pay gap would be 9 percent lower if no one worked for a parent's employer.[30]

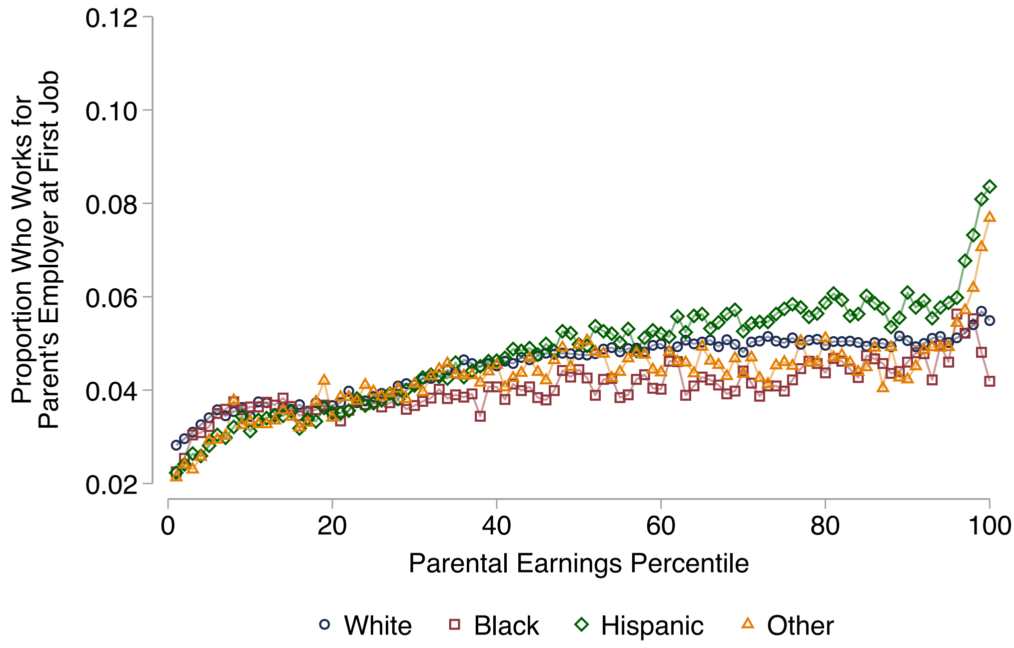
I disaggregate the results by sex, race, and ethnicity. Figure 9 shows that for daughters, there are not large differences across racial groups in the propensity to work for a parent's employer conditional on parental earnings. In contrast, Black sons are significantly less like to work for a parent's employer relative to White sons whose parents are in the same percentile of the earnings distribution. This aligns with prior work from

factual IGE.

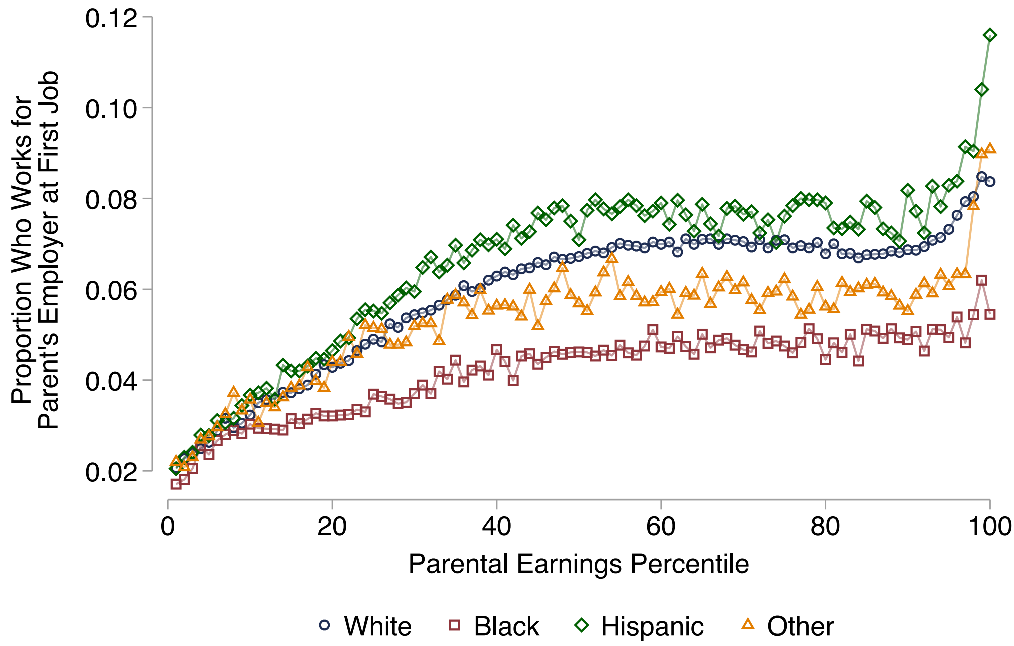
[30] I calculate this statistic using the same underlying methodology described in Section 2. Specifically, $-0.09 = (0.28 \times 0.015 + 0.18 \times 0.034 - 0.23 \times 0.039 - 0.30 \times 0.026)/0.07$.

Figure 9: Works for Parent's Employer by Parental Earnings, Sex, and Race

(A) Daughters



(B) Sons



Notes: Each point represents the proportion of individuals who work for their parent's employer for a sample defined by the interaction between sex, race, ethnicity, and the percentile of the parental earnings distribution.

Chetty et al. (2020), who find that, conditional on parental income, Black males have lower expected income compared to White males. Figure A.11 replicates this finding, and shows a conditional Black-White earnings gap of 8 log points in my sample. I calculate the counterfactual earnings for both groups—by subtracting $\mathbb{E}[D_i \mid r_p]\mathbb{E}[\beta_i \mid r_p, D_i = 1]$ from the average log earnings—and find that this conditional Black-White earnings gap would be 4 percent smaller if no one worked for their parent's employer.[31]

7 Conclusion

This paper shows that the intergenerational persistence in earnings is attributable, in part, to parents using their connections to provide access to higher-paying firms. Existing research documents the ubiquitous use of social contacts in the labor market but has less to say about the earnings consequences. I exploit transitory and firm-specific fluctuations in the availability of jobs at the parent's employer and estimate substantial earnings gains from finding a job through parental connections. Individuals with higher-earning parents are more likely to work for a parent's employer, and experience larger earnings gains when they do, and thus connections at the parent's employer lead to a modest increase in the intergenerational persistence in earnings.

While connections within the parent's employer are clearly not the main determinant of the intergenerational persistence in earnings, individuals may find jobs through a wider set of social contacts such as friends or extended family. Understanding how these broader connections shape intergenerational mobility should be a priority for future research. Furthermore, the analysis focuses on the first job and future research should examine how unequal access to jobs through social connections shapes labor market outcomes throughout the rest of the life cycle.

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

[31]This counterfactual exercise uses the earnings estimates in Figure 8 because I do not have sufficient power to estimate heterogeneous effects by sex, race, and parental earning.

that an individual's success be a function of their hard work and ability rather than the circumstances into which they were born. Thus, from an equity standpoint, the findings raise concerns about the relatively low levels of intergenerational mobility in the United States. Although, it is worth noting that the primary beneficiaries of these parental connections are blue-collar workers, a group that has experienced declining labor market fortunes over the past few decades. The results do not speak directly to the implications for efficiency and future research should aim to understand whether family connections lead to gains or losses in productivity.

The results also inform the positive assessment of what would be required to achieve equality of opportunity. One view is that economic rewards are determined by hard work and ability, which suggests that efforts to expand economic opportunity should aim to equip everyone with the skills they need to succeed in the labor market. The results challenge this purely meritocratic view of the labor market, as individuals from high-income families earn more not only because they are more skilled, but also because their parent's connections 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.

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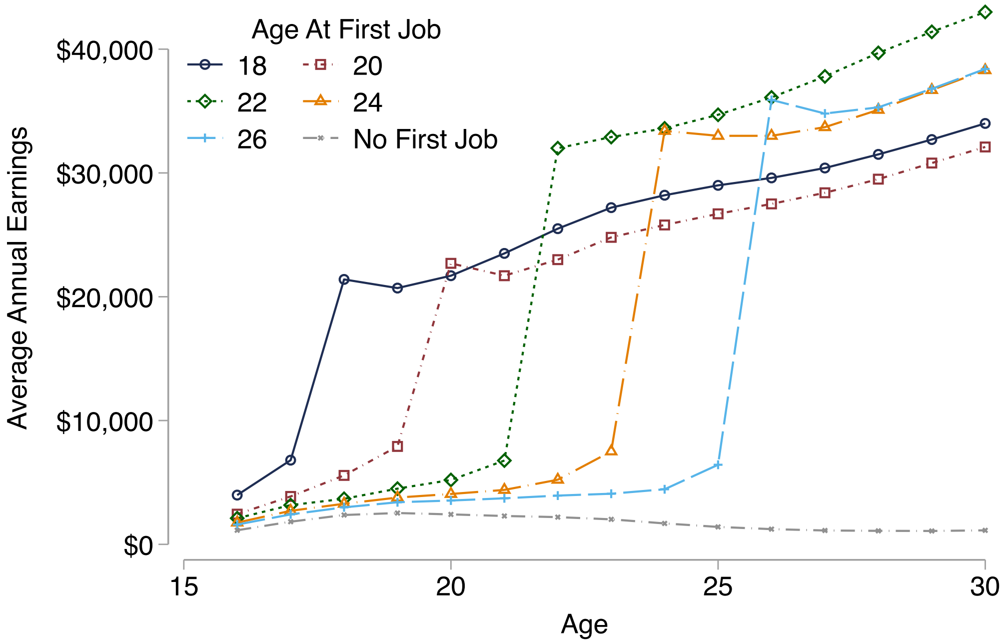
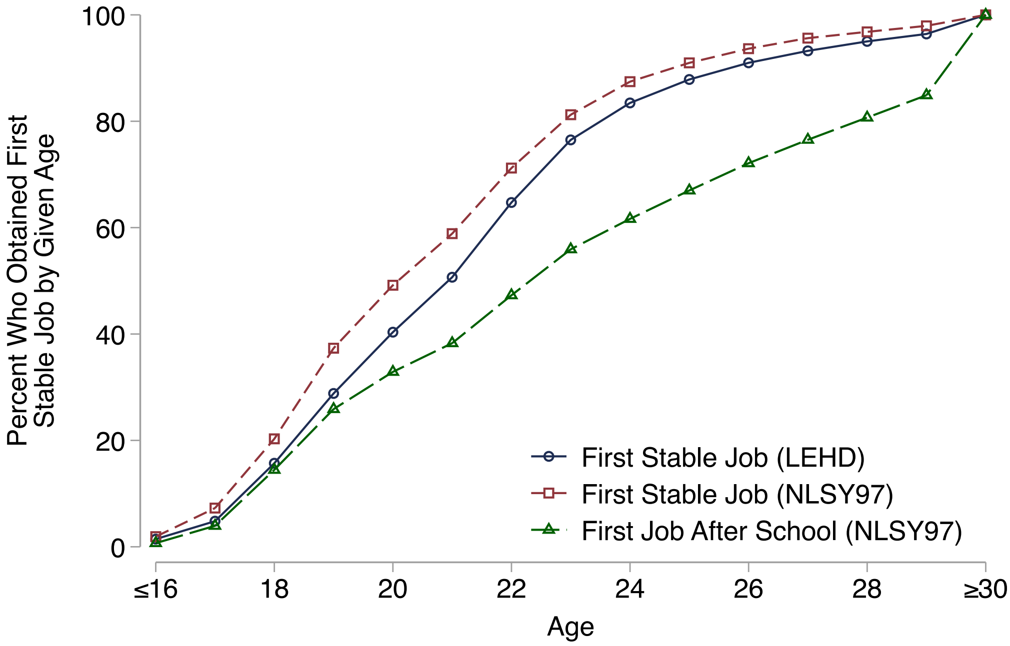
Appendix A Additional Figures

Figure A.1: Age-Earnings Profile



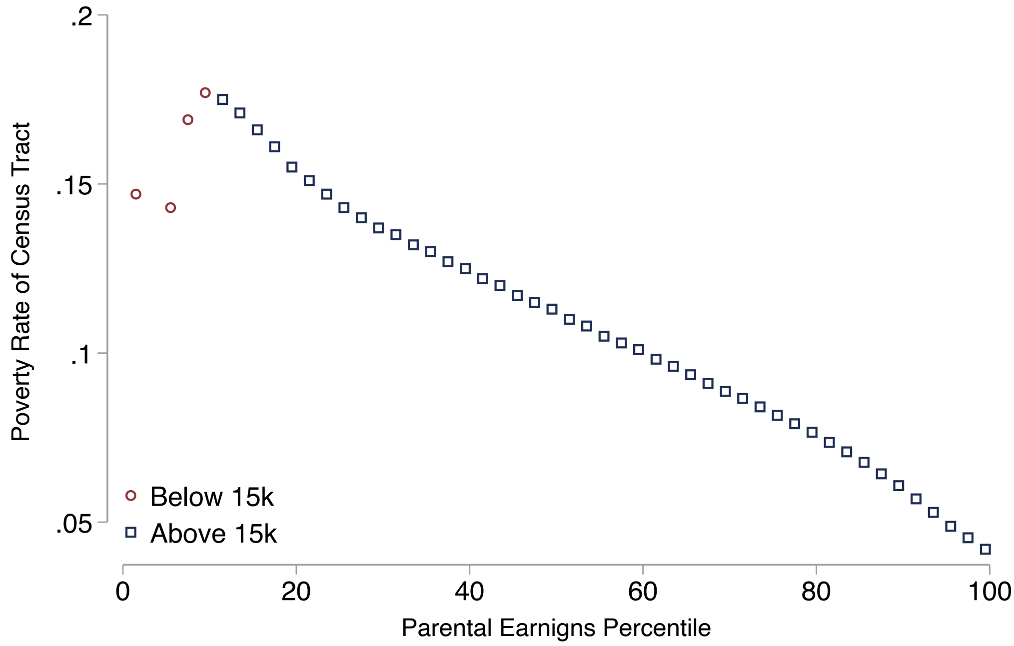
Notes: The figure plots the average annual earnings by age for different groups of workers defined by the age they were when they found their first stable job. The sample includes individuals who turned 30 by 2018.

Figure A.2: Age of Entry



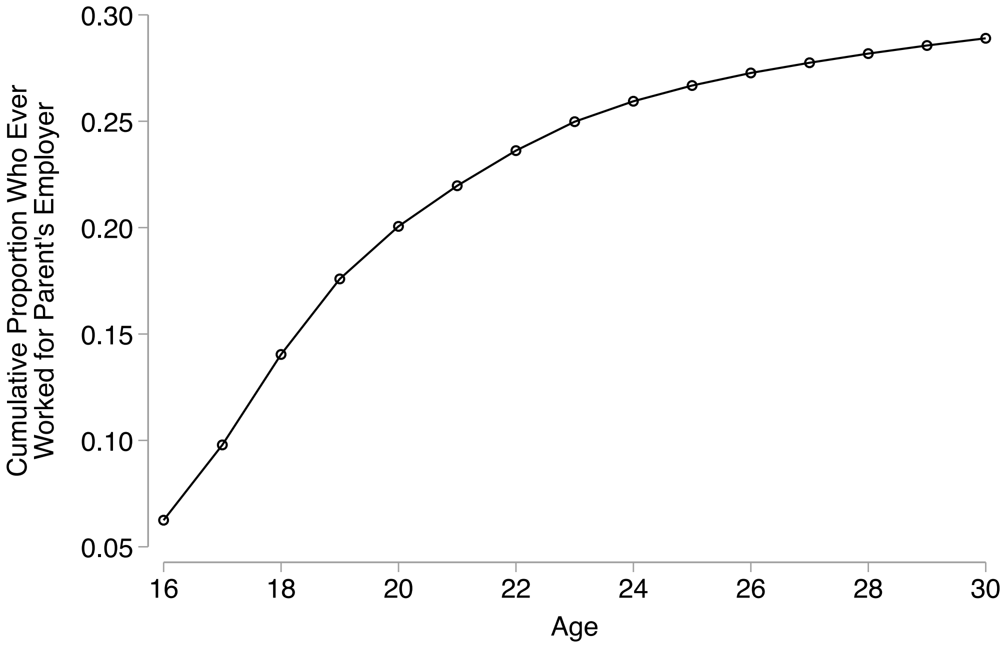
Notes: The figure plots the cumulative proportion of children that have entered the labor market by the age indicated on horizontal axis. For comparison, I also plot results using alternative measures of entry constructed from the NLSY97. These measures include the first stable job (working at least 35 hours for 36 consecutive weeks) and the first stable job after all schooling is completed.

Figure A.3: Parental Earnings and Neighborhood Poverty



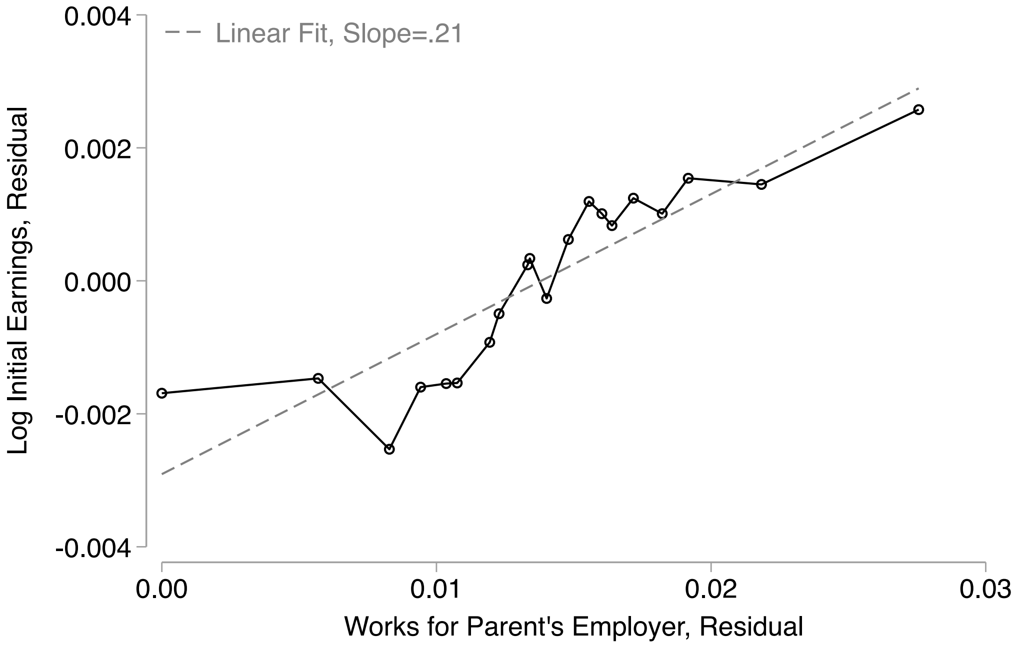
Notes: The figure plots the average poverty rate of the Census tract in which the parents lived in 2000. Parents are grouped into 50 equal-sized bins based on their earnings and each point represents a statistic for one of these distinct samples.

Figure A.4: Likelihood of Working for Parent's Employer by Age 30



Notes: This figure presents the cumulative share of workers who ever worked for a parent's employer between the ages of 16 and 30. The statistics are calculated on the sample of individuals who turn 30 by 2018.

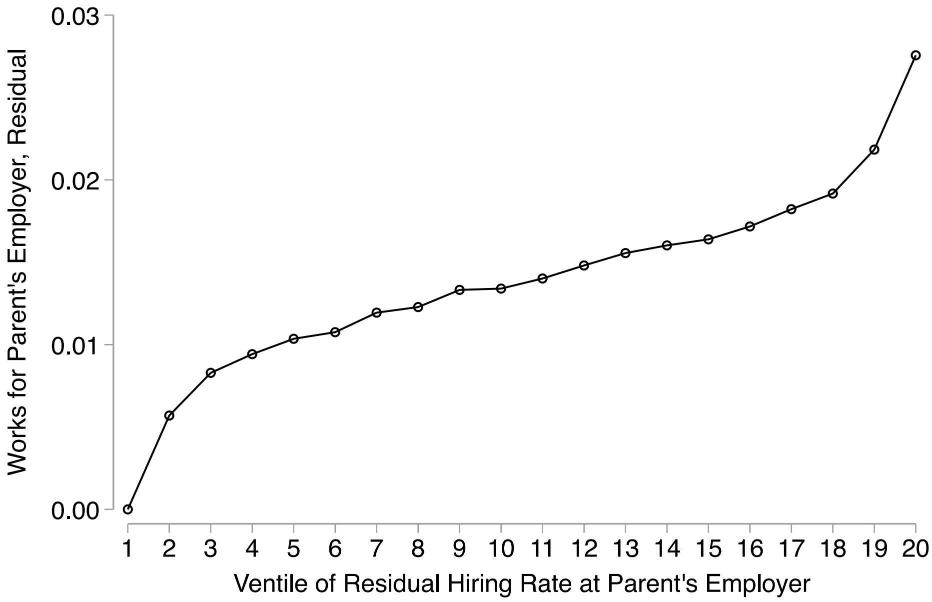
Figure A.5: Visualization of Treatment Effect Heterogeneity



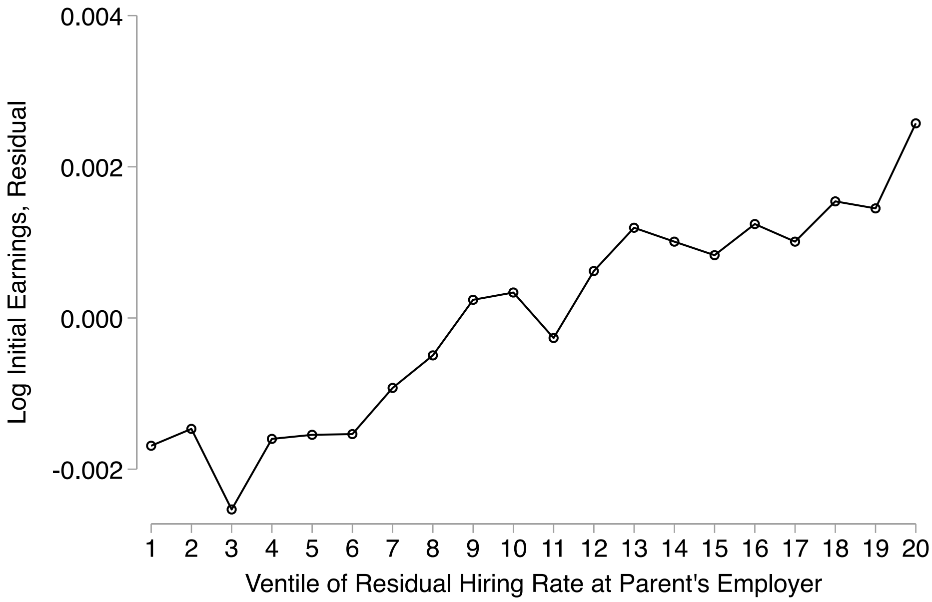
Notes: The hiring rate at the parent's employer is regressed against the standard set of covariates in equation 4 including fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. The data are grouped into ventiles based on the residuals from this regression. The initial log earnings and the indicator for working for a parent's employer are also residualized based on these same covariates and the figure presents the average values of these two residualized variables for each ventile of the residualized hiring rate. The solid line connects ventiles that one rank apart and the dashed line is the linear fit.

Figure A.6: Visualization of First Stage and Reduced Form

(A) First Stage

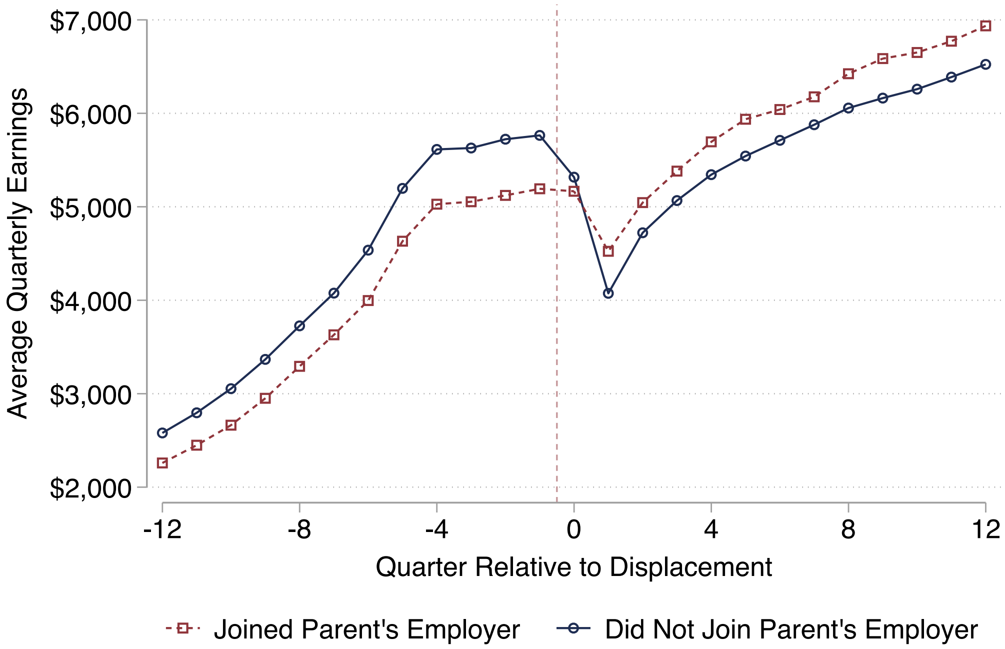


(B) Reduced Form



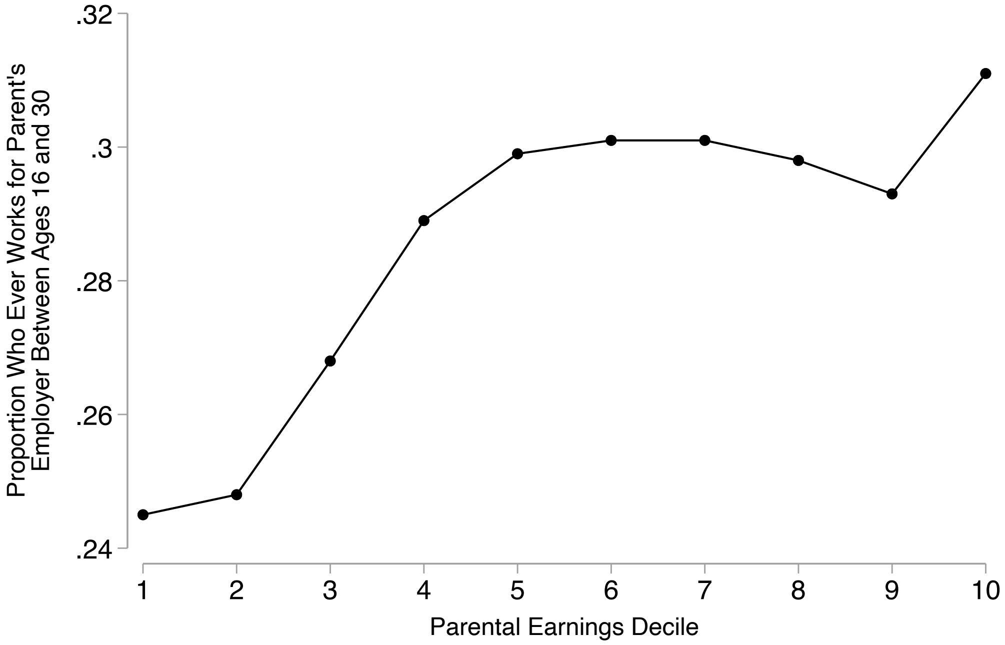
Notes: The hiring rate at the parent's employer is regressed against the standard set of covariates in equation 4 including fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. The data are grouped into ventiles based on the residuals from this regression. The initial log earnings and the indicator for working for a parent's employer are also residualized based on these same covariates. Panel A presents the average value of the residualized indicator for working for the parent's employer for each ventile of the residualized hiring rate. Panel B presents the average value of the residualized log initial earnings for each ventile of the residualized hiring rate. The solid line connects ventiles that one rank apart.

Figure A.7: Average Earnings of Displaced Workers



Notes: This figure presents the average quarterly earnings of workers displaced during a mass layoff event for two samples defined by whether or not the worker joined their parent's employer after being displaced. The sample includes young workers who were displaced by a mass layoff and is a balanced panel containing data 12 quarters before and after the mass layoff event.

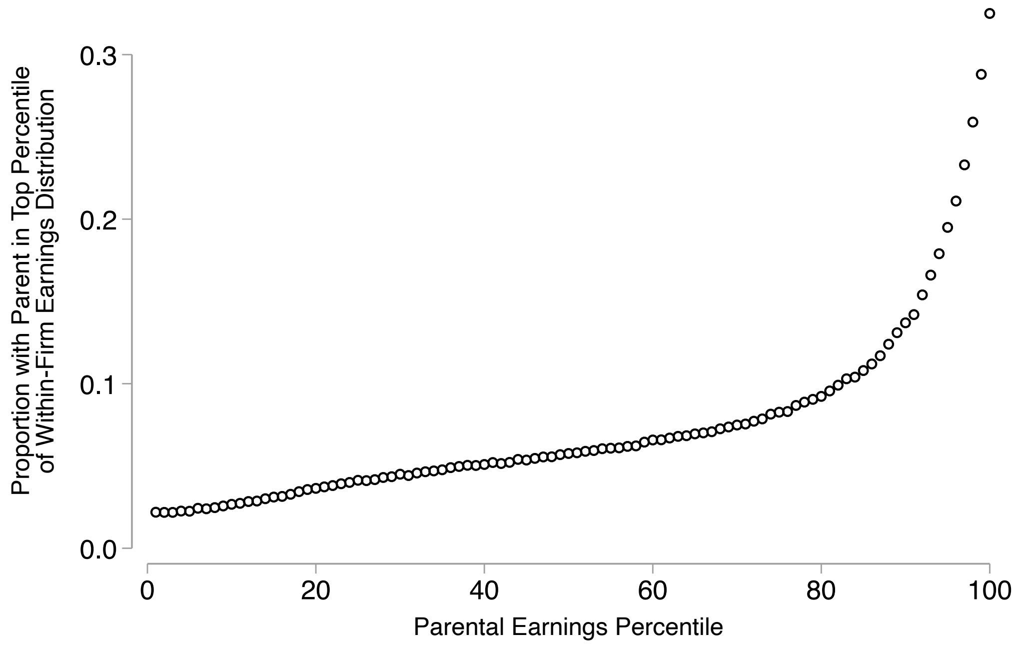
Figure A.8: Works for Parent's Employer Between Ages 16 and 30



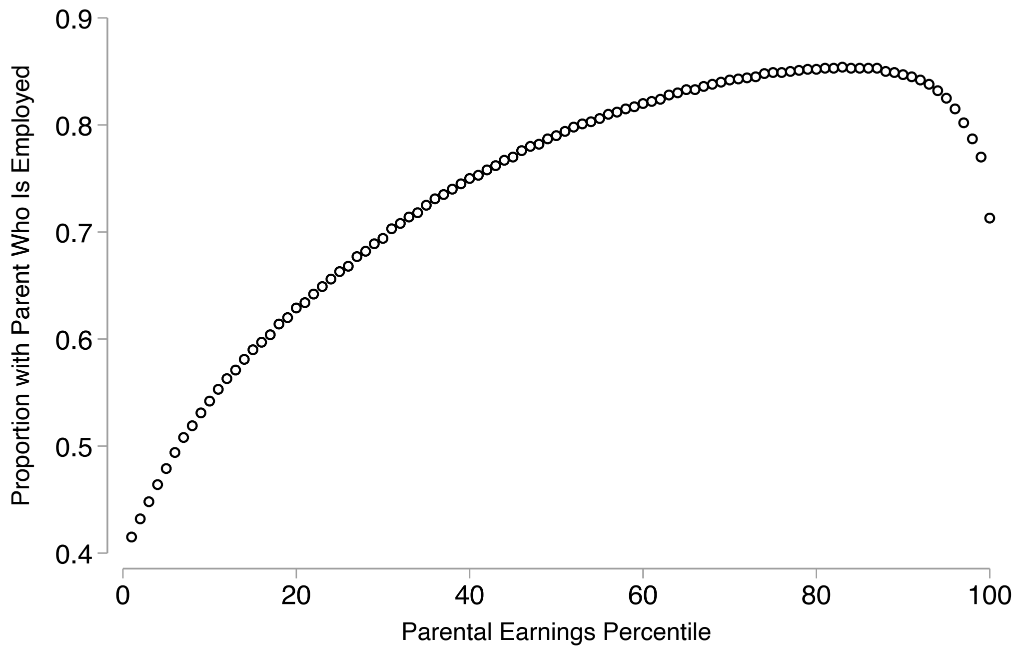
Notes: The figure plots the proportion of individuals who ever work for their parent's employer between the ages of 16 and 30 for each decile of the parental earnings distribution.

Figure A.9: Parent Characteristics by Parental Earnings

(A) Parent is Top Earner at Firm

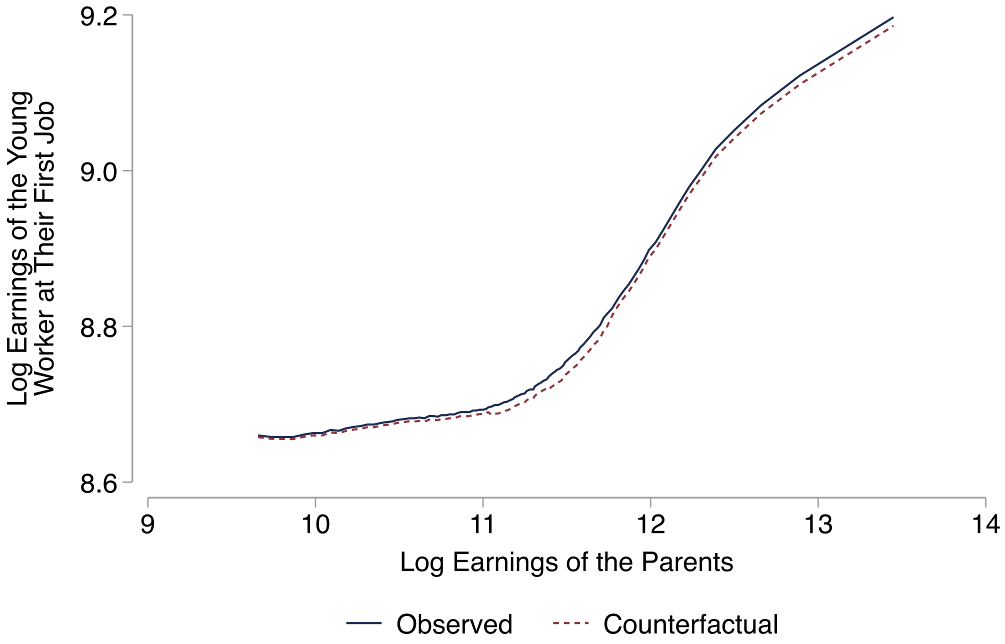


(B) Parent is Employed



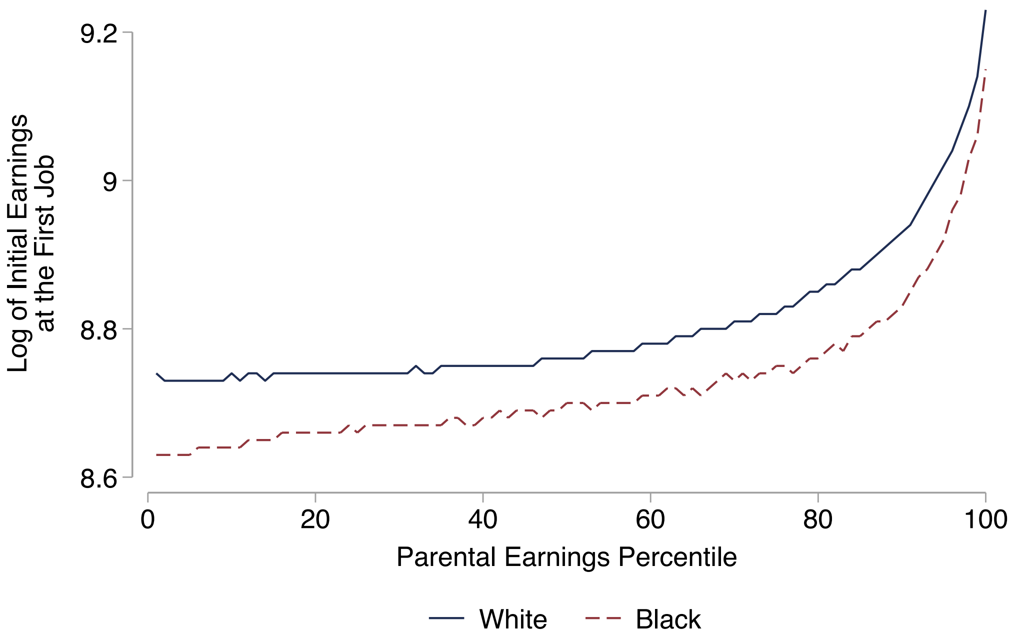
Notes: Panel A plots the proportion of individuals with a parent in the top percentile of the within-firm earnings distribution for each percentile of parental earnings. Panel B plots the proportion of individuals with a parent that is employed for each percentile of parental earning.

Figure A.10: Association Between Log Earnings of Children and Parents



Notes: The figure plots the average initial log earnings of the child against the average log earnings of their parent for each percentile of the parental earnings distribution. The blue solid line represents the observed earnings of the child. The red dashed line represents the counterfactual earnings of the child if no one were to work for a parent's employer.

Figure A.11: Black-White Earnings Gap for Sons



Notes: The figure plots that average initial log earnings of the child against the average log earnings of their parent for each percentile of the parental earnings distribution. The blue solid line and the red dashed line represents the earnings of White and Black sons, respectively.

Appendix B Additional Tables

Table B.1: Sample Restriction Criteria

Exclusion Criteria	Observations Remaining	
	Number	Percent
None (sample frame with no restrictions)	47,556,000	100
Child not assigned a unique PIK	38,701,000	81
Unable to link child to parents because either parent is not assigned a unique PIK or the households contains more than 15 people	35,375,000	74
Combined earnings of the parents does not exceed $15,000	31,693,000	67
The child does not find a stable job by 2018	25,860,000	54

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 cumulative count after the all sample restrictions described in preceding rows are applied. The third column presents this information as a percent of the total sample frame.

Table B.2: Summary Statistics

		Works for Parent's Employer	
	Full Sample	No	Yes
	(1)	(2)	(3)
Demographic			
Age	21.5	21.6	20.6
Male	0.51	0.50	0.57
White, Non-Hispanic	0.71	0.71	0.74
Black, Non-Hispanic	0.10	0.10	0.07
Asian, Non-Hispanic	0.03	0.03	0.03
Hispanic	0.13	0.13	0.13
Single Parent	0.16	0.16	0.10
Parent is Top Earner in Firm	0.08	0.07	0.19
Parental Earnings (Thousands)	54.7	54.2	63.0
First Stable Job			
Annual Aarnings (Thousands)	27.0	27.1	25.4
Stay for Three Years	0.40	0.39	0.56
Skilled Services	0.36	0.35	0.38
Unskilled Services	0.47	0.48	0.30
Blue-Collar	0.17	0.17	0.32
Large Firm (Employees>500)	0.41	0.41	0.41
Urban	0.73	0.73	0.70
Observations (Millions)	25.86	24.51	1.35

Notes: Each row presents average value for a different variable. Column 1 presents results for the full sample. Columns 2 and 3 present results for the sample of children who do not and do work for a parent's employer at their first stable job, respectively.

Table B.3: Effect on Initial Earnings with Binary Instrument

	(1)	(2)	(3)
A. Second Stage			
Works for Parent's Employer	0.250***	0.263***	0.224***
	(0.022)	(0.023)	(0.023)
B. First Stage			
Binary Indicator for Hiring at Parent's Employer	0.012***	0.010***	0.011***
	(0.000)	(0.000)	(0.000)
Hiring Rate Above	p25	p50	p75
Proportion of Treated Who Are Compliers	.16	.08	.05
First-Stage F-Statistic	6,421	6,180	5,643
Observations (Millions)	17.60	17.60	17.60

Notes: Each column presents estimates from a separate specification, which use a different binary variable as the insturment. To construct the binary instrument, the hiring rate at the parent's employer is regressed against the standard set of covariates including fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. I then create three binary instruments that are equal to one if the residualized hiring rate is larger than the 25th, 50th, and 75th percentiles, respectively. Panel B presents first-stage estimates, where the outcome is an indicator for whether the individual works for their parent's employer at their first stable job and the main independent variable is the binary hiring rate variable. Panel A presents second-stage estimates, where the outcome variable is initial log earnings. All regressions control for the standard set of covariates. Standard errors are clustered at the level of the parent's employer and are presented in parentheses. I calculate the share of individuals who work for their parent's employer who are in the complier group by multiplying the first-stage estimates with the proportion of the sample with a instrument value of one and then dividing by the overall proportion who work for a parent's employer.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.1$

Table B.4: Effect of Working for Parent's Employer by Parental Earnings

	Parental Earnings Quintile				
	First (1)	Second (2)	Third (3)	Fourth (4)	Fifth (5)
A. Instrumental Variables Estimator					
A.1. Effect on Individual Log Earnings at First Job					
Works for Parent's Employer	0.097	0.106*	0.200***	0.279***	0.156**
	(0.072)	(0.050)	(0.044)	(0.050)	(0.050)
First-Stage F-Statistic	444.5	918.4	1,264	1,118	1,319
Observations in Millions	1.927	2.769	3.386	3.765	3.73
A.2. Effect on AKM Firm Effect of First Employer					
Works for Parent's Employer	0.059	0.139***	0.226***	0.328***	0.279***
	(0.060)	(0.041)	(0.036)	(0.040)	(0.038)
First-Stage F-Statistic	436.8	907.3	1,245	1,106	1,318
Observations in Millions	1.914	2.75	3.364	3.74	3.7
B. Mass Layoff Estimator					
B.1. Effect on Change in Individual Log Earnings After Displacement					
Works for Parent's Employer	0.114*	0.215***	0.160**	0.201***	0.258***
	(0.053)	(0.049)	(0.049)	(0.045)	(0.045)
Observations	62,000	65,000	66,000	65,000	55,000
B.2. Effect on Change in AKM Firm Effect After Displacement					
Works for Parent's Employer	0.069***	0.173***	0.171***	0.247***	0.274***
	(0.015)	(0.014)	(0.015)	(0.013)	(0.015)
Observations	61,000	64,000	65,000	64,000	54,000

Notes: Each column presents estimates from a separate regression. The sample in Columns 1 through 5 is restricted to individuals whose parents are in the first through fifth quintile of the earnings distribution, respectively. Panel A presents estimates from the instrumental variables estimator defined by equation 4. The outcomes in Panels A.1 and A.2 include individual log earnings at the first stable job and the AKM firm effect associated with the first employer, respectively. The estimates are from the second stage and working for the parent's employer is instrumented for using the hiring rate at the parent's employer. All regressions include fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. Panel B presents estimates from the mass layoff estimator defined by equation 6. The outcomes in Panels B.1 and B.2 include the change in individual log earnings after displacement and the change in the AKM firm effect, respectively. The main dependent variable is an indicator for whether the individual joined their parent's employer in their first job after displacement. All specifications control for a fixed effect for the employer from which the individual was displaced, the average of the outcome variable in the four quarters prior to displacement, parental earnings rank, fixed effects for age, and fixed effects for the interaction between race, sex, and year of birth. The sample includes young workers displaced by a mass layoff. Standard errors are presented in parentheses.
*** $p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.1$

Table B.5: Effect of Working for Parent's Employer by Sex of Parent and Child

	Initial Log Earnings			
	(1)	(2)	(3)	(4)
Works for Parent's Employer	0.28***	0.23***	0.18***	0.30***
	(0.07)	(0.03)	(0.04)	(0.05)
Sex of Parent	Father	Father	Mother	Mother
Sex of Child	Daughter	Son	Daughter	Son
Proportion Works for Parent's Employer	0.015	0.039	0.034	0.026
First-Stage F-Statistic	1,178	2,946	1,465	1,188
Observations (Millions)	4.96	5.22	5.69	5.73

Notes: This table presents second-stage estimates from the two-stage least squares specification defined in equation 4. Each column presents estimates based on a distinct sample defined by the sex of the parent and the sex of the child. The outcome is the log earnings of the young worker and all regressions include fixed effects for the parent's employer; fixed effects for the local labor market in which the parent's employer is located, where the local labor market is defined by the interaction between commuting zone, industry, and quarter; and log parental earnings, measured in the year before the child begins their first job. Standard errors are clustered at the level of the parent's employer and are presented in parentheses.
*** p≤0.001, ** p≤0.01, * p≤0.1

Table B.6: Intergenerational Elasticity of Earnings

	Log Initial Earnings	Log Average Earnings Ages 29-31		
	(1)	(2)	(3)	(4)
Log Parental Earnings	0.136	0.482	0.162	0.491
	(0.000)	(0.002)	(0.000)	(0.002)
Sample Excludes Low-Earners	Yes	No	Yes	No
Measure of Parental Earnings	Long-Run	Long-Run	Long-Run	Age 16-20
Observations (Millions)	25.860	7.619	5.150	7.073

Notes: Each column presents estimates from a separate regression of the log earnings of the child on the log earnings of the parent. In column 1 the earnings of the child are measured at the first job. In columns 2-4 the earnings of the child are measured as the average annual earnings between the ages of 29 and 31. In columns 1-3 parental earnings corresponds to the long-run measure described in the text. In column 4 parental earnings corresponds to the average earnings of the parents in the years when their child was between the ages of 16 and 20 and the sample excludes observations if the combined earnings of the parents is less than $15,000. Columns 1 and 3 exclude children with sufficiently low earnings, while columns 2 and 4 add one to earnings in order to retain zeros.

Appendix C Description of Data

C.1 Measuring Parental Earnings

I estimate the long-run earnings of the parents using all available data. Specifically, I construct a panel with all strictly positive quarterly earnings records for each parent between 2000 and 2016 and estimate the following regression:

$$y_{it} = \alpha_i + \beta^g X_{it} + u_{it} \tag{C.1}$$

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, education (less than high school, high school, some college, bachelor's degree or advanced degree), and state of residence in 2000.[32] The sample is restricted to individuals between the ages of 25 and 65 and excludes individuals who have fewer than 12 quarters of strictly positive earnings over the entire time period.

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

I validate my measure of parental earnings by showing that it strongly correlates with neighborhood poverty rates. Using the Decennial Census, I identify the neighborhood, or Census tract, in which each household lives. Figure A.3 plots the average poverty rate of the neighborhood of residence against the percentile rank of parental earnings. For households with income above \$15,000, there is a negative association between earnings and neighborhood poverty rates. However, this strong relationship breaks down for parents with earnings below \$15,000, which is why I drop these individuals from the sample.

C.2 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 blue-collar 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).

C.3 Firm and Industry Pay Premiums

I estimate firm-specific earnings premiums using the AKM model (Abowd et al., 1999). I estimate the following specification,

$$y_{it} = \alpha_i + \Psi_{j(i,t)} + X_{it}\beta + \epsilon_{it} \tag{C.2}$$

[32] The education data are either measured using the 2000 Decennial Census long-form and the American Community Surveys, or are imputed (based on earnings) for workers that do not respond to these surveys.

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.[33] $\hat{\Psi}_{j(i,t)}$ is the firm pay premium.

I estimate equation C.2 using a national sample of quarterly earnings records from the LEHD measured between the years 2000 and 2016. The sample includes full quarter jobs for workers between the ages of 15 and 65.[34] I drop children from my intergenerational sample. As is standard in the literature, I restrict the sample to the largest connected set. I estimate the model by implementing the iterative method proposed by Guimaraes and Portugal (2010). I am unable to compute the firm pay premium for firms that lie outside of the largest connected set.

I estimate the industry-level premium using the similar data and methodology. Because all industries are connected through worker mobility, I estimate the industry premiums on a 10 percent sample of workers and collapse the quarterly data to an annual frequency. In the empirical model I replace the employer fixed effect with a fixed effect for the industry code. I am able to estimate an industry-level pay premium for all industries, and thus there are no missing data for this variable.

C.4 References

Abowd, J. M., F. Kramarz and D. N. Margolis (1999) "High wage workers and high wage firms", *Econometrica*, 67(2), pp. 251–333. doi: 10.1111/1468-0262.00020.

Card, D., J, Heining, and P. Kline (2013) "Workplace Heterogeneity and the Rise of West German Wage Inequality", *Quarterly Journal of Economics*. Oxford University Press, 128(3). doi: 10.1093/qje/qjt006.

Guimaraes, Paulo, and Pedro Portugal (2010) "A simple feasible procedure to fit models with high-dimensional fixed effects." *The Stata Journal*, 10(4), pp 628-649.

[33]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.

[34]If the worker has multiple jobs in a quarter, I retain the highest-paying job. To limit the influence of outliers, I drop observations if the quarterly earnings exceed one million dollars.

Appendix D Theory

D.1 Approximation Methodology

By definition, $\mathrm{cov}(D_i\beta_i, y_p) = \mathbb{E}[D_i\beta_i y_p] - \mathbb{E}[D_i\beta_i]\mathbb{E}[y_p]$. By iterated expectations,

$$\mathbb{E}[D_i\beta_i] = \mathbb{E}\big[\mathbb{E}[D_i\beta_i|D_i]\big] = \mathbb{E}[D_i]\mathbb{E}[\beta_i|D_i = 1] \tag{D.1}$$

and

$$\mathbb{E}[D_i\beta_i y_p] = \mathbb{E}\big[\mathbb{E}[D_i\beta_i y_p|r_p]\big] \tag{D.2}$$

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

$$cov(D_i\beta_i, y_p|r_p)^2 \leq var(D_i\beta_i|r_p) \times var(y_p|r_p) \tag{D.3}$$

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,

$$\begin{aligned} \mathbb{E}[y_p D_i\beta_i|r_p] &= \mathbb{E}[y_p|r_p] \times \mathbb{E}[D_i\beta_i|r_p] + cov(D_i\beta_i, y_p|r_p) \\ &\approx \mathbb{E}[y_p|r_p] \times \mathbb{E}[D_i\beta_i|r_p] \end{aligned} \tag{D.4}$$

where equation D.3 suggests that $cov(D_i\beta_i, y_p|r_p)$ 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 3.

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

$$\mathrm{cov}(y_{ij}, y_p) \approx \mathbb{E}\Bigg[\mathbb{E}[y_p|r_p] \times \mathbb{E}[y_{ij}|r_p]\Bigg] - E[y_p] \times E[y_{ij}] \tag{D.5}$$

This approximation relies on the same assumption used to derive equation 3.

D.2 Stylized Model

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

$$y_{ij} = h_i + f_j + u_i \tag{D.6}$$

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 firm pay premium of the child's employer can be written as,

$$f_j = f_{j(0)} + D_i\beta_i \tag{D.7}$$

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:

$$f_{j(0)} = \lambda h_i + \nu_i \tag{D.8}$$

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.[35] Furthermore, the relationship between the human capital of the child and earnings of the parent is characterized by,

$$h_i = x + \theta y_p + \eta_i \tag{D.9}$$

where $y_p \equiv y_{pj(1)} = h_p + f_{j(1)} + u_p$ 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 parent's employer makes a job offer to the child and zero otherwise. The offer decision depends on a hiring cost, $z_i \in \{z', z''\}$ 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.[36] Let 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,

$$D_i = \mathbb{1}\{\phi h_p + \gamma h_i > z_i\} \times \mathbb{1}\{\beta_i > c\} \tag{D.10}$$

Unlike the standard selection models, equation D.10 illustrates that selection into treatment depends on the choices of multiple agents.

Combining equations D.6, D.7, D.8, and D.9 yields the following relationship between the earnings of the child and the earnings of their parents,

$$y_{ij} = \alpha_1 + \alpha_2 y_p + D_i \beta_i + \epsilon_i \tag{D.11}$$

where $\epsilon_i = \nu_i + (1 + \lambda)\eta_i + u_i$, $\alpha_1 = (1 + \lambda)x$, and $\alpha_2 = (1 + \lambda)\theta$.

Regressing y_{ij} on y_p yields an estimate of the intergenerational elasticity of earnings (IGE). My goal is to understand how the IGE would change if no one worked for the same employer as a parent; i.e., if $D_i = 0$ of all i. Because of the presence of heterogeneous treatment effects and the potential correlation between D_i and ϵ_i, controlling for D_i will not provide an answer to this question.[37] For this reason, I rely on the approximation methodology discussed in Section 2.

[35] 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 $\beta_i = (\frac{\lambda}{1+\lambda} - \lambda\theta)y_p + [\lambda/(1 + \lambda)](\lambda\nu_p - u_p) - [\lambda x + \lambda\eta_i + \nu_i]$.

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

[37] To see the relationship between D_i and ϵ_i note that $\epsilon_i = \nu_i + (1+\lambda)\eta_i + u_i$, $O_i = \mathbb{1}\{(\frac{\phi}{1+\lambda} + \gamma\theta)y_{pj(1)} + \gamma x - \frac{\phi}{1+\lambda}(\nu_p + u_p) + \gamma(x + \eta_i) > z_i\}$, and $A_i = \mathbb{1}\{(\frac{\lambda}{1+\lambda} - \lambda\theta)y_{pj(1)} + (\frac{\lambda}{(1+\lambda)})(\nu_p/\lambda - u_p) > c + \lambda x + \lambda\eta_i + \nu_i\}$.

The counterfactual analysis requires an estimate of the average treatment effect on the treated (ATT), and the stylized model highlights why an instrumental variables estimator might recover that parameter. 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,

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

$$\underbrace{E[\beta_i|D_i = 1]}_{ATT} = \underbrace{E[\beta_i|D_i(z') < D_i(z'')]}_{LATE} \tag{D.12}$$

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,*

$$\begin{aligned} \underbrace{E[\beta_i|D_i = 1]}_{ATT} &= E[E[\beta_i|A_i = 1]|O_i = 1] \\ &= E[E[\beta_i|A_i = 1]|O_i(z') < O_i(z'')] \\ &= \underbrace{E[\beta_i|D_i(z') < D_i(z'')]}_{LATE} \end{aligned} \tag{D.13}$$

where the first and third inequalities hold by the law of iterated expectations and the second inequality holds as a result of $O_i \perp\!\!\!\perp \beta_i$.[38]

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.

The following proposition shows that the intergenerational transmission of employers has a theoretically ambiguous effect on intergenerational mobility.

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:*

- *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 $\rho(y_{ij}, y_{pj(1)}) < \rho(y_{ij(0)}, y_{pj(1)})$*

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

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,*

$$\begin{aligned} O_i &= \mathbb{1}\{(\frac{\phi}{1+\lambda} - \theta\gamma)y_{pj(1)} > 0\} \\ A_i &= \mathbb{1}\{(\frac{\lambda}{1+\lambda} - \lambda\theta)y_{pj(1)} - \lambda x > c\} \\ \beta_i &= (\frac{\lambda}{1+\lambda} - \lambda\theta)y_{pj(1)} - \lambda x \end{aligned} \tag{D.14}$$

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 highlights two competing forces. On the one hand, high-income parents are best able to procure high-paying job offers for their children. On the other hand, children from low income households have lower levels of human capital and are more reliant on their parents to find a descent paying job. Thus, while my empirical evidence suggests that the intergenerational transmission of employers increases the intergenerational persistence in earnings, this conclusion might differ in other contexts depending the characteristics of the labor market and the human capital accumulation process.

D.2.1 Extension with Parental Investment in Human Capital

In the spirit of the canonical models of intergenerational mobility from Becker and Tomes (1976, 1986), I extend the baseline model to allow parents to 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.6, D.7, and D.8. However, I do not impose the assumption stated in equation D.9, 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 example, $h_i = log(H_i)$).

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:

$$\max_{C_p, C_i, B_i} \{v(C_p) + u(Y_{ij} + RB_i)\} \text{ subject to } C_p + S_i + B_i \leq Y_p \tag{D.15}$$

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.

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 complementarities 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, the expression determining optimal investment can be rewritten as follows,

$$\alpha(1+\lambda)H_p^{\sigma(1+\lambda)}S_i^{\alpha(1+\lambda)-1}exp\{D_i\beta_i\} + H_p^{\sigma(1+\lambda)}S_i^{\alpha(1+\lambda)}\frac{\partial exp\{D_i\beta_i\}}{\partial S_i} = R \tag{D.16}$$

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 it is straight forward to show that the optimal level of investment is given as:

$$S_i' = [\frac{R}{\alpha(1+\lambda)}]^{1/[\alpha(1+\lambda)-1]}H_p^{\sigma(1+\lambda)/[1-\alpha(1+\lambda)]} \tag{D.17}$$

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, $h_i = x + \theta y_p$, where $x = \frac{-\sigma}{1-\alpha(1+\lambda)}log\big(\frac{R}{\alpha(1+\lambda)}\big)$ and $\theta = \frac{\sigma/(1+\lambda)-(1-\alpha)}{1-\alpha(1+\lambda)}$. Note that this linear relationship is exactly the one assumed in equation D.9.

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,

$$s_i'' - s_i' = \frac{D_i\beta_i}{1-\alpha(1+\lambda)} \geq 0 \tag{D.18}$$

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.[39]

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

[39]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.

employer ($\frac{\partial exp\{D_i\beta_i\}}{\partial S_i} \neq 0$).[40] 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.16 the sum of the terms of the left hand side would 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 consequences for parental investment are theoretically ambiguous.[41] 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 experience smaller earnings gains when they do. Thus, if firms were prohibited from hiring children of current employees, it is theoretical ambiguous whether parents would respond by investing more or less in the human capital of their children.

The implications for intergenerational mobility are similarly ambiguous. Consistent with my empirical findings, consider the case in which $\theta(1+\lambda) < 1$ and $\phi > -\theta\gamma(1+\lambda)$, which implies that children from high-income families are more likely to work for a parent's employer and experience larger earnings gains when they do. 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. Thus, if firms were prohibited from hiring children of current employees, changes in parental investment behavior (not estimated in my paper) could either amplify or mitigate the direct effects of using the parental connections on intergenerational mobility.

D.3 Interpreting the LATE

This section provides a theoretical argument for why the LATE may be a reasonable approximation of the ATT in my context.

Let $Y_i(d, z)$ denote the potential outcome of individual i who has the 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 $d = \{0, 1\}$, (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

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

[41] 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.

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.[42] 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

$$\underbrace{\mathbb{E}\big[\beta_i|\{\eta_i^A < \beta_i\}, \{\underline{z} < \eta_i^O < \bar{z}\}\big]}_{\text{LATE}} = \underbrace{\mathbb{E}\big[\beta_i|\{\eta_i^A < \beta_i\}, \{Z_i < \eta_i^O\}\big]}_{\text{ATT}} \tag{D.19}$$

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.2 develops a stylized behavioral model and provides a more detailed discussion of the intuition by focusing on a specific case of equation D.19.

D.4 References

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[42] 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 $x \in \{O, A\}$.